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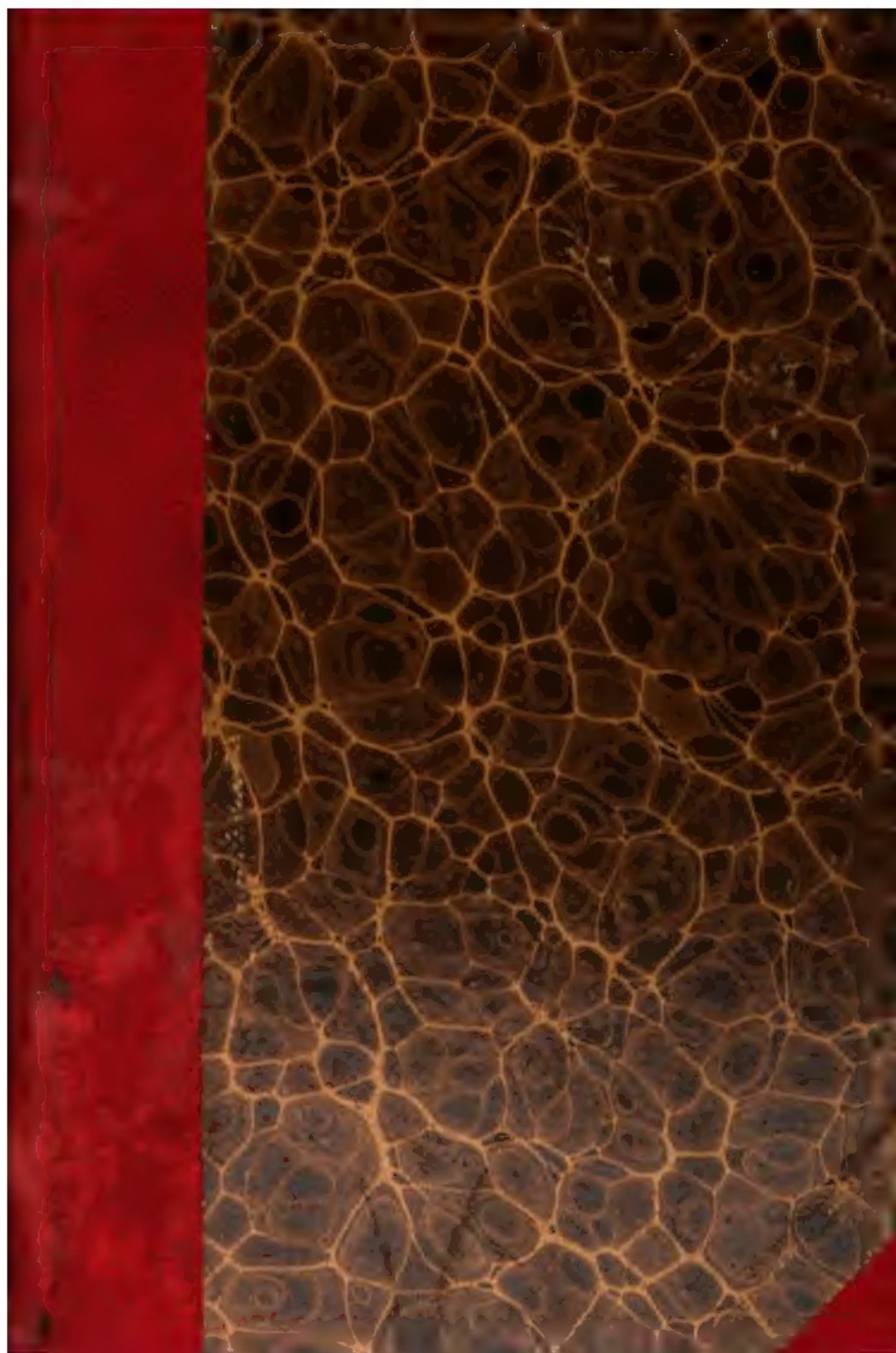
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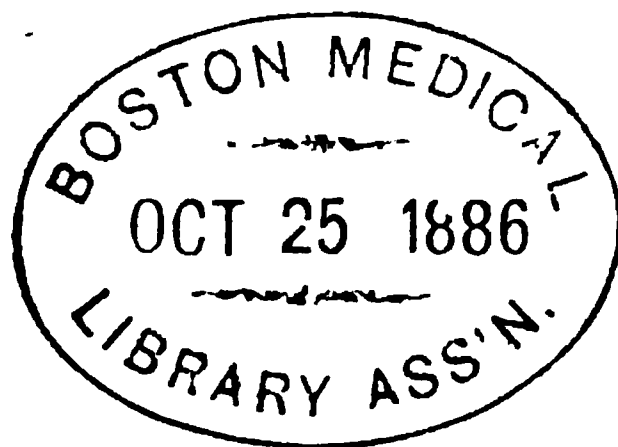
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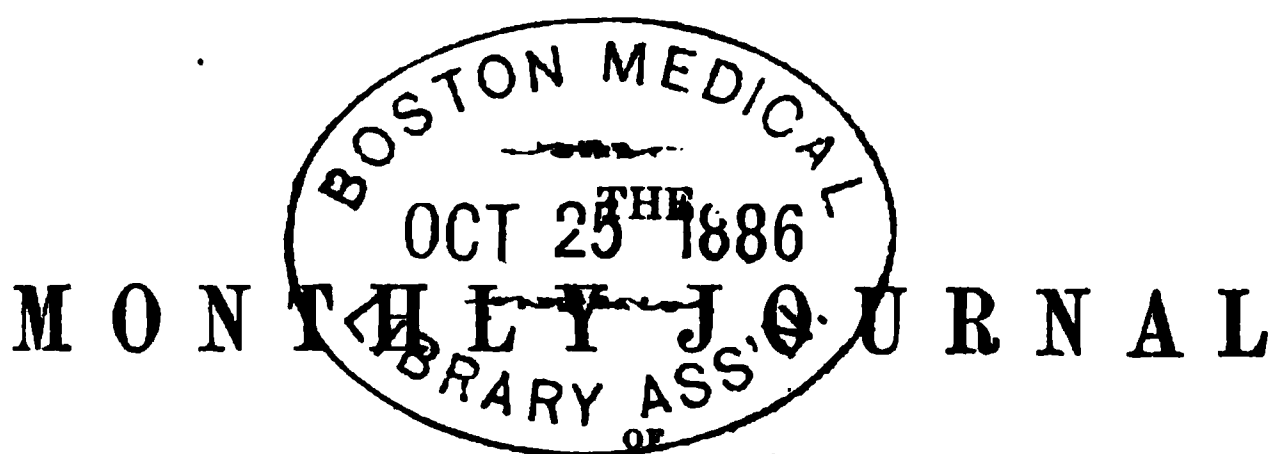
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MONTHLY JOURNAL

MEDICAL SCIENCE.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pedunculated Exostosis of the Long Bones.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

(Read to the Medico-Chirurgical Society, Nov. 16th, 1853.)

As the expression exostosis literally implies merely an inordinate growth of bone, it has been employed to denote a great variety of morbid conditions in which this circumstance happens to exist. The "callus" that repairs a fracture—the new osseous shell which supplies the place of a dead shaft—the spina ventosa or expansion caused by the accumulation of pus or other fluid—the spicular or foliated excrescences that shoot into the substance of an osteo-sarcomatous tumour—and the bony prominences of morbid growth resembling the natural processes connected with the origin and insertion of muscles, have all been included within this comprehensive title. It is to a particular form in which the last mentioned of these productions appear, that I now wish to direct the attention of the Society.

In his Essay on Exostosis, Sir Astley Cooper has described the simple growth of osseous substance, and mentioned the situations where tumours of this kind are most frequently met with. Of these, the one by far most common is the thigh-bone, a little above the internal condyle near the insertion of the triceps adductor; and he expresses the opinion that the origin of such excrescences may be connected with some impulse to increased action caused by inor-

dinate contraction of the muscles. It may be objected to this explanation, that the tumours to which Sir A. more particularly alludes, though situated near the insertion of a muscle, are always placed at the side of it, so as not to be directly influenced by its contraction, and also that simple osseous growths are met with where the influence of any tendon or muscle is quite impossible—as at the distal phalanx of the great toe, from the extremity of which, on the upper surface, a troublesome exostosis is apt to grow. In the present state of our knowledge, it must be admitted, that in so far as the cause of such productions is concerned, we do not possess any satisfactory information.

The tendency to growths of this simple kind is greatest in the early period of life, especially about the time of puberty, as if, in the development of the body to its mature proportions, there were some redundancy of expansion in the osseous system. Indeed, young and growing persons often exhibit a somewhat similar tendency in the processes and articulating extremities of the long bones enlarging more rapidly than the parts connected with them, so as even to excite apprehensions of deformity. I have frequently been consulted by anxious relatives on such occasions, and always felt warranted to predict that, in the course of due time, symmetry would be restored. It may be remarked that there is thus afforded one of the numerous and diligently cultivated fields of orthopædic delusion, the changes resulting from spontaneous improvement being attributed to the influence of useless machinery. The simple exostosis is not unlimited in its growth, but after attaining a certain size, remains stationary through life. About thirty years ago, a gentleman now practising our profession in the neighbourhood of Edinburgh, and who was then attending my lectures on anatomy, applied to me one day in great distress, on account of an aneurism of the subclavian artery which he had just discovered. I found that there was an exostosis of the first rib at the insertion of the scalenus anticus, which had displaced the vessel, so as to excite his apprehension, and comforted him with the assurance that the growth was not likely to enlarge. I examined it lately, and could not discover the slightest alteration of size. I have met with several cases of the same kind since, and one that may be particularly mentioned, which I saw in consultation with the late Dr John Thomson and Professor Turner. The patient had been sent from the country on purpose to have the operation performed, and we excited considerable surprise by giving a very positive assurance that there was no prospect of any future inconvenience. About two years ago, a lady of title came to Edinburgh in very great alarm, on account of an exostosis in the pharynx, which she had been confidently assured was only of six weeks' duration. Finding that the growth was perfectly hard, and of bony consistence, I concluded that although only recently noticed, it had been of long existence, and was not likely to suffer any change, which it accordingly has not done.

The constitution of these growths is quite similar to that of the spongy extremities of the long bones, being composed of a thin, osseous lamina externally, and a more or less compact, consolidated texture within it. While enlarging, they have an incrustation of cartilage, whence they have been called by Sir A. Cooper and others—in my opinion, very improperly—the cartilaginous exostosis, since any bone in the body might with equal propriety be designated cartilaginous: and as this nomenclature is sure to occasion confusion between the truly osseous growths and those of a fibro-cartilaginous nature, which anatomically, pathologically, and practically require to be carefully distinguished. Thus Sir A. Cooper describes under the same title, well marked cases of these two most dissimilar conditions, and in his lately published work, Mr Paget has pursued this course with, if possible, still less regard to the diversity of the tumours thus associated together. Anatomically, they differ in constitution, which, in one case, is the simple bony texture encrusted externally, so long as it is growing, by a plate of cartilage; and, in the other, it is a fibro-cartilaginous growth enclosed within the expanded bone, or dispersed through the interstices of its honey, comb-looking structure. Pathologically, they differ in the one sort of growth being limited in its tendency to increase, and producing no inconvenience except from the peculiarity of its position, as when seated under the nail of the great toe, while the other has no bounds to its enlargement, which depends upon the fibro-cartilaginous substance, and not upon the bone. Practically, they differ in this important respect, that while the one may be removed by division of its neck or base; the other cannot be extirpated except by taking away the whole bone, or dividing it at a sound part beyond the confines of the disease. It was from not attending to these distinctions that Mr Liston, who first drew professional attention to the exostosis of the toe, insisted upon amputation as essential for the patient's permanent relief; while due regard to them led me to practise removal of the growth alone, as sufficient for attaining the object in view—a procedure which, though for a time reprobated by Mr Liston and his followers, has long since become the established rule of practice. On the other hand, a knowledge of the depth to which the fibro-cartilaginous growth strikes its roots into the bone affected, has put an end to those painful scenes which surgeons of the present day witnessed in their youth, when attempts were made, of course in vain, to dig out such tumours from the upper and lower jaws. By disarticulation or division of the bone where ascertained to be sound, the patient is now effectually relieved in a few minutes, instead of being subjected to the prolonged torture of hours, without the chance of benefit to his case, and with the greatest risk of its aggravation. Having happened to remove the first superior maxillary bone that was removed upon this principle in Great Britain, and having entered upon the records of surgery the first instance of this operation which they contain, I must here protest against the

unjust appropriation of any credit that may be due on this account, which has been copied from each other by the writers of students' books, of whom I may mention Mr Erichsen as the most recent. My operation was performed in the Edinburgh Surgical Hospital on the 13th of May 1829, and the case was published in the Edinburgh Medical and Surgical Journal for July of the same year, so that the question of priority admits of no dispute.

Next to the maxillary bones, it is the humerus in which the fibro-cartilaginous growths most frequently appear; and the two following cases of its occurrence in this situation may be shortly noticed as illustrative of the pathological characters which distinguish it from the simple exostosis. About twenty years ago, happening to be in Dundee, I was asked to look at an enormous tumour of the shoulder under which a poor man was labouring. It began at the middle of the humerus, and extended upwards, expanding in a conical form, so as to appear as if growing out of the patient's side. There was no motion in the shoulder-joint, but the arm could be slightly moved along with the scapula. From the firm consistence of the tumour, and the soundness of the general health, I concluded that the growth was of a fibro-cartilaginous non-malignant nature, and having ascertained that it had commenced in the humerus, I felt assured that its roots would not extend beyond the confines of this bone. Influenced by these considerations, I undertook to remove the tumour, and performed the operation in the hospital here, with no less difficulty than had been anticipated. All trace of the head of the tumour had disappeared; and the bone, enormously expanded in a cup form, surrounded the neck of the scapula. In order to facilitate removal, and also promote healing of the wound, I removed the extremities of the clavicle and acromion process, and also the glenoid cavity with the coracoid process. At the end of a fortnight the patient was able to return home, where he lived for many years afterwards without any trouble from the disease. The tumour weighed twelve pounds, and is the largest of the kind on record, although it affected only one-half of the bone.

The second case which I proposed to mention was that of a boy, of respectable parentage, from Kirkcaldy, who was brought here, about ten years ago, on account of a tumour seated midway between the elbow and shoulders. It was the size of a small orange, and presented the character of a fibro-cartilaginous growth from the bone. Being averse to amputate the patient's arm, and knowing that mere removal of the tumour on a level with the surface from which it proceeded would not eradicate the disease, I divided the shaft of the humerus by an oblique application of the cutting pliers on each side of the neck by which the tumour was attached. Upon examination, the tumour proved to be what had been supposed, and was found to grow from the interior of the bone. Osseous union could not be completed, but the patient attained maturity, with a wonderfully useful arm, having all the actions of the hand perfect;

and being able also, through the assistance of a leather case compressing the humeral muscles, to make a considerable use of the elbow joint. So far as I know, this is the only instance on record of a fibro-cartilaginous tumour of a long bone being effectually removed without amputation.

From what has been said it will, I trust, appear that, under the title of cartilaginous tumour, have been comprehended two morbid conditions of the osseous system, of the most different natures, which require to be distinguished for the establishment of their proper treatment. In regard to the osseous growths, associated with malignant tumours, serous cysts, and abscesses—as the new-formed bone is always perfectly sound in its textures, and owes its production to the influence of another morbid condition,—I think the disease in such cases should be named, not from the exostosis, but from the cause which has occasioned it, whether this may be a growth, a cyst, or an abscess. The expression exostosis would thus be restricted to its proper sense of denoting the change effected in the bone itself, and would only require to be farther divided into the solid, hollow, and spicular forms. Attention would thus be directed to the disease essentially requiring remedy, and not to an accidental concomitant.

I may now proceed to the special object of this communication, which is to explain a circumstance in the constitution of the simple exostosis when it proceeds from the long bones of the extremities, which may be of considerable consequence in regard to both its diagnosis and treatment. The most frequent situations of this growth, as I have already stated, are the inner side of the thigh bone a little above the condyle, and the neck of the humerus, just below the tuberosities into which the scapular muscles are inserted. It has a neck from half an inch to an inch in length, which is usually placed obliquely in relation to the shaft, and beyond this expands into an irregularly rounded form, nodulated on the surface, which is covered by a thin incrustation of cartilage, that presents a shining, pearly lustre, and has no communication with the surrounding parts. In the case of a young man who came from Rothesay to the hospital for the removal of an exostosis on the *outer* side of the thigh bone, I find the same characters. The tumour consists of perfectly sound osseous substances, more or less tensely cancellated.

About five-and-twenty years ago I was consulted by a gentleman resident in this city, on account of a very large exostosis, in the usual situation, on the inner side of the thigh. He was a tall, strong, active man, and, in consequence of over-exertion, was suffering from swelling, redness, and pain of the limb at the seat of enlargement. I advised soothing measures, and expressed the opinion that an operation would not be requisite if he abstained from undue exertion in future. I frequently afterwards saw this gentleman, apparently in the enjoyment of perfect health, and had no

reason to entertain any doubt as to his being so for more than twenty years, when, in the month of February 1851, Dr Handyside asked me to see him, on account of an alarming change that had taken place in the condition of the tumour. The thigh was considerably swelled and painful, especially when pressure was applied on the seat of the exostosis, which could not be felt distinctly in consequence of the thickening around it that had taken place, together with the effusion of some fluid that was detected by a deep fluctuation. In explanation of this circumstance, as it was stated that the patient had latterly been leading a life of much greater exertion, particularly in walking, than he was previously wont to do, Dr Handyside enjoined rest, and, after a time, applied a blister. We thought that there was something seriously wrong, and were not free from the apprehension of cerebriform degeneration, but resolved to try the farther effect of rest, with gentle pressure. In the course of a few weeks the swelling and tenderness disappeared, the fluctuation could no longer be perceived, and the exostosis was felt no less distinctly defined than in former times.

I felt quite at a loss to account for this case, until the following one completely explained it. In November of last year Dr Duncan asked me to take under my charge, in the hospital, a female servant who, in crossing the ferry from Fife, had received a blow, which was supposed to have detached an exostosis from its usual seat of attachment, above the knee, on the inner side of the thigh bone. As the limb was very much swelled, and extremely painful, I feared that delay might lead to suppuration, and therefore cut freely down to the exostosis; but in doing so was surprised to find a quantity of serous fluid, tinged with blood, lying between it and the surface immediately surrounding it. The whole truth at once appeared, and I no longer felt any difficulty in accounting for the smooth shining surface presented by growths of the kind in question, or for the fluctuation which, in the former case, had so much perplexed Dr Handyside and myself. It was plain that a synovial membrane surrounded the exostosis, and was reflected from its neck, so as to afford a double covering, just as on the joints—the half of one being thus represented by the osseous growth, its crust of cartilage, and investing membrane.

I hope that the condition of what may be called the pedunculated exostosis, thus fully ascertained, may be of service, not only in its diagnosis, but also with reference to the means of remedy, since the complete insulation from surrounding textures, except at the comparatively small point of its attachment to the bone affords great encouragement to attempt removal, even under circumstances apparently much opposed to success. Thus, in the case of a young lady from Lancashire I saw lately with Dr Simpson, there was an exostosis at the neck of the humerus, which pressed upon the axillary nerves, so as to occasion great and increasing distress; and, from this position, had been deemed beyond the reach of removal.

But, having ascertained that the point of attachment was at the posterior surface of the bone, I made an incision between the deltoid and triceps, exposed the neck, detached it by cutting pliers, and extracted the exostosis, with hardly any bleeding, and no injury to the neighbouring parts. My friend, Mr Joseph Lister, who was present at this operation, examined the growth removed microscopically, and, at my request, will now have the goodness to record the result of his observations to the Society.

ARTICLE II.—*Notes of the Examination of an Exostosis removed by Mr Syme on the 2d of October 1853, from the Os Humeri of a young Lady aged about twenty years.* By JOSEPH LISTER, M.B., London, F.R.C.S.

(*Read to the Edinburgh Medico-Chirurgical Society, Nov. 16th, 1853.*)

THE tumour was situated at the posterior and inner aspect of the bone, two or three inches from its upper end. Some idea of its general appearance may be gathered from figure 1 of the accompanying woodcut (see next page), which gives a lateral view of it of the natural size. It is seen to be of irregular form, presenting at its most prominent part several smooth rounded tuberosities: these were covered with cartilage, while the more circumferential parts of the tumour rose gradually from the normal level of the bone around, and were destitute of cartilage. The whole surface of the tumour was invested with extremely loose cellular tissue, which must have allowed very free gliding motion of superjacent parts; this cellular tissue adhered firmly, both to the cartilaginous and osseous portions of the surface. At *a*, a piece of the tumour had been broken off, exposing the cancellated texture of the interior, which in the deepest parts of the exostosis, was extremely loose, consisting of medullary substance traversed by very delicate spicula, which presented the microscopical characters of true bone (a lacuna with canaliculi from one of these spicula was shown in a sketch at the reading of the paper). In the circumferential parts of the tumour, which, as above stated, were destitute of cartilage, this loose cancellous structure extended to within a very short distance of the surface, which was formed by a thin layer of compact, true osseous tissue. But beneath the cartilaginous prominences there was a considerable thickness of compact substance, of a peculiar white aspect, too gritty and friable for true bone, and having a dark confused appearance under the microscope, with no definite structure; but after maceration in dilute hydrochloric acid presenting both to the naked eye, and under the microscope, the characters of cartilage. As a general rule, this calcified cartilage was present in greatest amount where the cartilage was thickest. Figure 2 exhibits part of a section made perpendicular to the surface of one of the prominences of the exostosis; *a* is the cartilage covering the surface, and is upwards

of a line in thickness; the calcified cartilage *b* immediately beneath it was very dense, while at *c* the texture was looser, but even there, at a depth of five-eighths of an inch below the surface, cartilage cells showed themselves after maceration in acid.

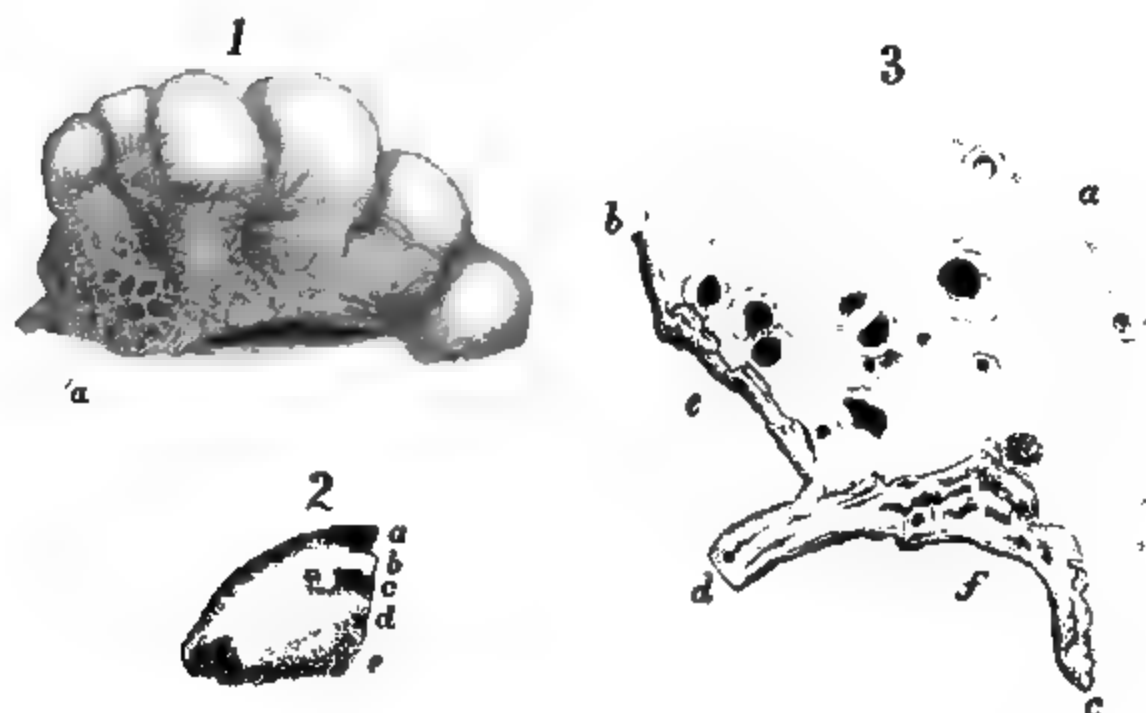


FIG. 1 is a lateral view of the exostosis: at *a* a piece of the tumour has been broken off, and the loose structure of the interior shows itself.

FIG. 2 exhibits part of a section through one of the prominences of the tumour: *a* is the superficial cartilage; *c* is a portion of cartilage situated deeply, and surrounded on all sides by dense calcified cartilage, *b* and *d*; *c* is the deepest part of the calcified cartilage, of looser texture than the more superficial parts.

FIG. 3 shows a section of a portion of the tumour at the line of junction of the calcified cartilage and the cancellous structure of the interior; the earthy matter has been removed by dilute hydrochloric acid. *a* is the cartilage with its cells changed by the process of calcification; *b c* is true bone (containing lacunae) lining the excavations in the calcified cartilage; *d* is part of a spiculum of the cancellous structure; *e* and *f* are spaces formerly occupied by medullary substance.

The microscopical appearances of the superficial cartilage presented nothing very remarkable; the cells were larger than they are found in human articular cartilage, and many of them were of rather complex structure; they were much elongated at the free surface, where, indeed, it was difficult to distinguish them from the contiguous part of the investing cellular tissue; while in the deeper part of the cartilage they were more or less elongated in a direction perpendicular to the surface of calcification (a figure was shown representing one of these deeper cells). The matrix was homogeneous or faintly granular. The matrix of the calcified cartilage as seen after maceration in acid, was more granular, and the cells were rounder, and often surrounded with a broad pellucid ring, but the cells of the immediately adjacent part of the uncalcified cartilage assumed more or less of the same characters. The boundary between the uncalcified and the calcified cartilage was rendered very distinct by the circumstance that the matrix of the former was

coloured brown by the action of the acid, while that of the latter generally remained colourless.

At one spot, viz., at *c* in Fig. 2, a piece of cartilage existed at a considerable depth below the general surface of calcification, and surrounded on all sides by the dense pseudo-bone; this cartilage was extremely soft, and its cells remarkably large; one of them of circular outline measured $\frac{1}{285}$ inch in diameter; the nuclei also presented considerable variety of appearance, and complexity of structure. It might be supposed that these characters of the cells were connected with great rapidity of growth in this cartilage, and such may perhaps be the case; but the cells are not larger than exist in the central parts of costal cartilages. In the centre of a costal cartilage of a woman about sixty years old, I found cells very similar both in size and appearance, and it can hardly be supposed that any very rapid cell-development had been going on in that situation at such a period of life.

The fact that the calcified cartilage was in some places looser in texture at its deep than at its superficial parts (*e. g.* looser at *e* than at *b* in Fig. 2) seems to indicate a change going on in the calcified cartilage by which it becomes converted into the loose cancellous structure of the interior. Examination of the deepest parts of the calcified cartilage under a low magnifying power, after the earthy matter has been removed by acid, shows that it is invaded by processes of the medullary substance of the cancellated tissue beneath it, which advance for a variable distance into its substance, and give a very irregular eroded character to its outline. The excavations thus seen to occur in the deep border of the calcified cartilage, are often lined with a thin layer of true bone containing lacunæ, which has, no doubt, been deposited subsequently to the formation of the excavations. Fig. 3 represents a small portion in this condition, where *a* is the deepest part of the calcified cartilage, *b c* is a layer of true bone lining the excavations *e* and *f*, which were formerly occupied by processes of the medullary substance; and *d* is part of one of the spicula of the cancellous structure. The true bone is distinguished from the matrix of the calcified cartilage by having a higher refractive power than it.

In the part where I first noticed this osseous lining of the calcified cartilage, it was almost universally present; very few of the excavations being even partially destitute of it. And the same appearance presented itself in the head of a metatarsal bone of a boy sixteen years of age, at the line of junction between the epiphysis and the cartilage which separated it from the shaft. Indeed, the resemblance between the two objects was exceedingly close, for the disposition of the cartilage cells in piles perpendicular to the surface of ossification, while present in the shaft of the metatarsal bone, was absent alike in the epiphysis, and in the exostosis; also the part of the cartilage that was immediately contiguous to the newly-formed bone of the epiphysis was evidently calcified, and closely resembled

the calcified cartilage of the exostosis, both in the condition of the cells and in the eroded form of its margin; the eroded edge, again, was almost universally lined with a layer of true bone exactly as in the exostosis, except that the layer was generally somewhat thicker.

These facts appeared at first inconsistent with the idea of the layer of true bone having been deposited subsequently to the formation of the excavations, for that supposition implied that both in the epiphysis and in the exostosis, the process of excavation of the calcified cartilage had almost entirely ceased, or, in other words, that the conversion of the calcified cartilage into true bone was almost or altogether suspended; and this appeared particularly unlikely in the case of the exostosis, which was known to have been growing rapidly before its removal.

The examination of the ossifying epiphysis of one of the bones of a calf's foot has, however, convinced me that the layer of true bone is deposited on the walls of previously existing excavations. The cells of the calcified cartilage are there seen to enlarge at the expense of the matrix as they approach the cancellous structure of the epiphysis, and at the same time to acquire a granular appearance, just like that of the rudimentary medullary substance with which the last formed areolæ are filled; and finally, they evidently form by their coalescence, the excavations in the margin of the calcified cartilage. Those parts of these excavations or areolæ, which are farthest from the perfect bone, and which have been last formed, are devoid of any osseous lining; but at a very short distance from their extremities they acquire upon their surface a thin layer of lacunated bone, which is seen to increase gradually in thickness at the expense of the cavities as they are traced nearer to the perfect bone.

A further examination of the exostosis also showed that in some parts the osseous lining of the margin of the calcified cartilage was absent, while, on the other hand, there appeared at these parts, evidence of a change in the deepest cells of the calcified cartilage, like that observed in the calf's foot, viz., an enlargement of the cells, and a conversion of their contents into a granular substance previous to their coalescence to form the excavations.

It thus appears that the process by which the calcified cartilage of the exostosis was converted into the cancellous structure of the interior, is essentially the same as what occurs in the ossification of the epiphysis of a metatarsal bone. The only difference between the two cases appears to be that in the ossifying epiphysis, the calcified cartilage is a very thin layer, while in the exostosis it is present in considerable thickness; but even in this particular there was not a constant difference between them; for the calcified cartilage varied much in thickness at different parts of the exostosis, and at some spots was, to the naked eye, absent or nearly so, even where cartilage existed on the surface.

The great thickness of the calcified cartilage probably results, in part at least, from a want of energy in the process by which it is converted into the cancellated tissue, and accordingly it was just at

that part where the calcified cartilage was thickest (viz., at *e* in fig. 2), that the osseous lining of the calcified cartilage was found almost universally present; and we have seen that this condition implies an arrest in the process of conversion of calcified cartilage into cancellous structure. The languid condition of the ossific process at this part, was probably also the cause of the piece of cartilage *c* in fig. 2, being left uncalcified below the general level of calcification.

The general conclusion to which the examination of this exostosis leads, is that it grew at the surface as cartilage, which became converted into cancellated bone by an ordinary process of ossification, in which, however, the stage of calcification of cartilage occupied an unusually conspicuous position.

ARTICLE III.—*Contributions to Toxicology.* By DOUGLAS MACLAGAN, M.D., F.R.S.E, etc. (*In continuation from Vol. XVI. p. 22.*)

Oil of Bitter Almonds. Experimental investigation of the alleged poisonous quality of the Oil when freed from Hydrocyanic Acid.

(*Read before the Pharmaceutical Society, November 10, 1853.*)

IN the month of September 1853, a servant girl in a family in Edinburgh was poisoned, from having swallowed a small quantity of a bottle of flavouring matter sold under the name of quintessence of ratafia, but which, instead of being a solution of essential oil of bitter almonds in spirit, proved to be the undiluted oil itself. The case was sent to me for investigation, but as I was from home at the time, it was forwarded to Dr George Wilson, who ascertained these facts. Dr Wilson estimated the quantity of the essential oil which the girl had swallowed at a little under half a fluid drachm.

The ordinary commercial essential oil of bitter almonds, as is universally known, consists of a peculiar oil, (hydruret of benzule,) to which its peculiar aroma and pungency are due, associated with anhydrous prussic acid; the proportion of the latter varying from three to fourteen per cent. This hydrocyanated oil is therefore always more or less dangerous as a poison, generally highly so. It has also all along been matter of general belief that the potency of the oil as a poison was greater, the larger the amount of prussic acid which it contained, but it has been disputed whether its poisonous action was due solely to the prussic acid, and whether the hydruret of benzule itself did not contribute to its noxious qualities. The hydrocyanic acid, as is well known to chemists, can be separated from the hydruret of benzule, and thus that constituent of the crude oil, which is admitted to be the chief source of its poisonous properties, can be removed without impairing the peculiar flavour for which the oil is used in cookery. Dr Wilson brought this fact under the notice of the authorities here, who called public attention

to it by a newspaper paragraph. Several of our chemists in Edinburgh, who had been accustomed to deal in flavouring essences made with the hydrocyanated oil, forthwith acting upon this intimation, set themselves to prepare oil free from prussic acid, and essences containing only pure hydruret of benzule, are now to be found in all the best establishments in this city.

The separation of the hydrocyanic acid from the hydruret of benzule is theoretically simple enough, but to accomplish this completely is not so easy, as the statements in chemical works would lead us to suppose. Distillation from some base, such as lime, potassa, or baryta, has been adopted, and the process most commonly given in books is to agitate the crude oil with a large excess of lime or caustic potash, to add to this a considerable quantity of a salt of iron and to distil; the prussic acid remaining behind in the form of prussian blue. The proceeding which seems to have met with most favour at the hands of our pharmacutists in Edinburgh, is to agitate the oil with a large bulk of a mixture of milk of lime and caustic potash, and to distil without adding any salt of iron. As the use of common sulphate of iron, however, does not materially add to the expense, and as the compound, formed with the iron, is more stable, and less likely to yield up again any of its hydrocyanic acid, than the cyanides of calcium and potassium which are formed when no iron is present, I should be disposed to prefer the addition of the sulphate. The object of the present communication, however, being toxicological and not pharmaceutical, I do not enter into any details on this point, nor should I have expressed any opinion regarding it, had it not been that, operating on the small scale of two or three ounces of oil, I have succeeded easily and completely in removing all traces of prussic acid by the iron process. The most economical methods of working will doubtless soon be ascertained, because some of the wholesale establishments in London which deal extensively in this article, are now preparing the pure hydruret of benzule on the large scale. So far as our present experience goes, it appears to be essential to success, whatever process is followed, that the agents destined to retain the prussic acid should be brought by continued agitation into accurate and prolonged contact with the oil. The prussic acid adheres to the oil with great tenacity, and notwithstanding all precautions, a second distillation has generally been required before the oil can be pronounced entirely free from prussic acid.¹

Whatever method of purification be adopted, it is necessary that the distilled product be accurately tested for prussic acid. The ordinary iron test is the simplest and best, and if performed with a little

¹ It was suggested by a chemical friend that perhaps a single distillation might be made to yield at once oil free from prussic acid, by putting the lime or potassa into the still along with the marc of bitter almonds and water. This *a priori* did not appear to be likely, from the known facility with which the amygdalin and emulsin, the two constituents of the almond which yield the hydrocyanated oil, are attacked by alkalies. To satisfy my friend, I made the trial, and found as I expected, that no oil at all was obtained, the distillate reacting and smelling strongly of ammonia.

care, is capable of revealing exceedingly minute traces of prussic acid. For this purpose twenty or thirty minims of the sample under trial are to be agitated for several minutes in a test tube with twice their bulk of aqua potassæ of the ordinary pharmacopœia strength, and the mixture thrown upon a wetted filter, by which the oil is all retained, and a clear fluid is got, to which a few drops of a solution of sulphate of iron are to be added. This solution answers best if it has been made for some time, and has by the action of the air become partially peroxidated. Hydrochloric acid is now added in sufficient quantity to re-dissolve the excess of oxide of iron, which is thrown down by the potash, and if there be any prussic acid present, the prussian blue in the fluid becomes at once apparent. Agitation of the oil with water is quite insufficient for bringing the prussic acid within the agency of the iron test. The oil seems to prevent the water from dissolving out the prussic acid.

It appears frequently to have happened that in a sample of oil which has, when newly distilled, been tested and pronounced free from prussic acid, a subsequent trial by the same operator has shown it to be present in notable quantity. This may of course sometimes be due to the agitation having been more carefully attended to in a second trial. But perhaps it may be the case that by rest the prussic acid has some tendency to separate spontaneously from the oil, and that thus the tests detect it more readily. It has been suggested, that another explanation of this may be, that in process of time, prussic acid, which has actually been at first completely removed by the rectification, is re-developed in the oil by keeping; this, if true, would form a serious objection to the use, as a flavouring condiment, of the oil, however carefully rectified. It is not the case however. First, Because pure hydruret of benzule contains no nitrogen, as may be proved by applying to it Lassaigne's elegant process, of heating it with a fragment of potassium, and subsequently testing it for the cyanide in the usual way by the iron and hydrochloric acid. Secondly, In a sample of oil rectified by myself and found free from prussic acid in February 1852, the application of the iron test to half a drachm in November 1853, did not reveal the least trace of prussic acid. Lassaigne's test also showed it to contain no azote, and of course in the absence of this element, no hydrocyanic acid can be developed. There is, therefore, if due care be taken, no difficulty, either in freeing the oil from prussic acid, or in determining that this has been successfully accomplished.

But the important question in relation to public welfare is,—supposing the oil to be quite free from prussic acid, does it or does it not possess poisonous properties? As very conflicting statements have been put forth on this point, it will be proper briefly to notice what has been said regarding it.

Vogel of Munich, in 1821, appears to have been the first to make direct experiments on the subject. He used oil which had been rectified by distilling it twice from a concentrated solution of caustic potash. He inferred it to be pure, because he could find no hydro-

cyanic acid in the residue left in the retort on the second distillation. One drop of this oil placed on the tongue of a small bird proved instantly fatal with strong convulsions; but four drops on a puppy six weeks old produced only temporary exhaustion. From these experiments, very unsatisfactory both as regards the physiological effects and the purity of the oil, he concluded that the oil was an active poison. Orfila is stated by Dr Christison to have confirmed these results by his experiments. I have not been able to find the original account of any experiments by Orfila with pure oil. In his *Médecine Légale*, vol. iii. p. 744, he does not cite any experiments of his own, but says in reference to the hydruret of benzule, "It is extremely poisonous and acts nearly like the empyreumatic oil of tobacco."

Liebig (*Chemie Organique*, 1843) states simply that the hydruret of benzule, when swallowed, acts as a poison; and Dr Gregory (*Hand-book of Organic Chemistry*, 1852) says, "It ought to be borne in mind that the commercial oil is highly poisonous, not only because it contains hydrocyanic acid, but because the hydruret of benzule is poisonous."

On the other hand, it has been contended upon experimental evidence that the oil if quite free from prussic acid is not poisonous.

Stange (*Büchner's Repertorium*, xiv. and xvi.) rectified the oil by caustic baryta, and found after this treatment that five drops of the oil killed a kitten in four minutes. He afterwards found, however, that the oil which he had regarded as pure still contained prussic acid. He rectified it again, and then found that in doses of from four to twelve drops it did not injure a rabbit, nor did six drops placed in the cellular tissue act as a poison. He found that of the unrectified oil two drops placed in the cellular tissue produced fatal effects on a rabbit.

Schröder and Hertwig, at Berlin, 1826, used a salt of iron as a means of removing the hydrocyanic acid. They found that neither dogs, cats, nor pigeons were poisoned by the purified oil. The pigeons and cats got from three to eight drops, the dogs twenty or thirty drops, without injury. Hertwig himself swallowed ten drops without experiencing any other effects than those due to its pungency in the mouth. Göppert of Breslau found that four drops of the purified oil had no effect on small birds, and twenty-one drops none on a rabbit, except what was due to the pungency. Robiquet and Boutron Charlard also came to the conclusion that if quite free from hydrocyanic acid it was not poisonous.

Wöhler and Frierichs, 1848, in the course of their experiments on the elimination of foreign matters by the kidneys, gave to a small dog two grammes, equal to thirty-two grains, with no poisonous effect. They showed that the oil in passing into the urine was converted into hippuric acid, but that if the dose was large, part of the oil passed through the kidneys unchanged. This observation, which has been abundantly confirmed by other experimentalists, I have also been able to verify.

Of the more recent British authorities almost all countenance the idea that the purified oil is not poisonous. Dr Christison, as a deduction from the various experiments quoted by him, states that it is not more poisonous than other volatile oils, and that "if the purified oil retains active poisonous properties, it must be owing to the hydrocyanic acid not having been entirely removed."

Dr Taylor (On Poisons) states shortly that "the oil deprived of prussic acid and perfectly pure does not act injuriously;" and in reference to this he quotes a paper by Mr Grindley.—(Pharm. Journ., vol. vii.)

Mr Grindley used red oxide of mercury, lime, and chloride of iron for purifying the oil, as he states that he could not succeed with the lime and iron salt alone. Mr Grindley's purified oil, subjected to Lassaigne's test, showed that it contained no nitrogen, and was therefore free from all traces of prussic acid. Of this purified oil five, and afterward ten, drops given to a rabbit produced no bad effect.

Drs Royle and Neligan, in their respective works on *Materia Medica*, both admit that the pure oil is not poisonous.

An experiment of Dr Pereira's, as narrated in his own words in the posthumous volume of his *Elements of Materia Medica*, p. 1776, shows how readily mistakes may be made as to the purity of the oil. "In some earlier experiments which I made on this subject, I found it to be highly poisonous, though I could not detect an atom of hydrocyanic acid in it. After the sample had been kept a few months, however, I readily detected the acid in it by the potash and iron test. By a second and third rectification, I completely deprived it of all traces of the acid, and I then found that four drops of it given to a small rabbit had no more effect than the same quantity of any other volatile oil; that is, the animal appeared dull for a few minutes, and the respiration was quickened." But to the article on Bitter Almond, in which this statement of Pereira's is made, there is appended the following remark by his editors, Drs Taylor and Rees, "Mitscherlich has experimented on the effects of this oil on dogs and rabbits; he found it poisonous even when quite free from hydrocyanic acid." This statement, made without any qualification, and backed by the authority of so eminent a toxicologist as my friend Dr Taylor, was somewhat alarming to those, who believed, that in selling essences made of the purified oil, they were vending harmless articles.

I may remark in passing that there is a trifling error in this statement. Dr C. G. Mitscherlich does not appear to have made any experiments on dogs. I have not seen his original paper, which seems to have been printed in the *Medizinische Zeitung*, 1848, but in a very full abstract of it in Büchner's *Repertorium* for 1850, I find no mention of any experiments save on rabbits. Dr Mitscherlich's observations are known in England chiefly from a very brief abstract in the *Pharmaceutical Journal*, vol. x., translated from the *Pharmaceutisches Central Blatt*.

The more important points in Mitscherlich's experiments are the following. Two drachms of the purified oil were injected into the

stomach of a middle-sized rabbit. The animal was much agitated, as usual in such experiments. The pupils became dilated, and the iris immoveable. Motion and sensation were so rapidly destroyed that in five minutes the animal fell down, and showed no action when mechanically irritated. It lay like a body before the setting in of cadaveric rigidity; the globe of the eye alone showing some sensibility. It lay in this state for an hour, with quick breathing, and rapid, rather strong action of the heart. These gradually became slower, and the animal died in an hour and forty minutes. No morbid appearances of any importance were found.

A similar experiment was made on another rabbit with one drachm of the oil. The animal looked dull in five minutes. In ten minutes the irritability of the hinder legs was almost extinct, that of the fore legs less so. The heart's action and breathing were accelerated. The animal in fifteen minutes fell motionless on its side, and remained so for four hours, when it died. There were no important morbid appearances.

In a third experiment, a scruple of the oil was given to a rabbit, but beyond having its breathing quickened it was not injured.

Mitscherlich's conclusions are, that the oil, free from hydrocyanic acid, is a strong poison, weaker than oil of mustard, but stronger than the oils of savin, cinnamon, carraway, nutmeg, fennel, turpentine, lemons, juniper, and copaiba,—that the phenomena preceding death in poisoning by it are rapid exhaustion of voluntary motion and sensation, whilst the heart's action and respiration continue,—that the oil when not free from prussic acid acts in virtue of the presence of the latter, since a scruple of pure oil may be given without fatal effects,—that in large doses it is poisonous, like other volatile oils.

I submit that the detailed account of Mitscherlich's experiments does not warrant the unqualified statement of the editors of Dr Pereira's posthumous volume. In this state of uncertainty, therefore, it appeared to me right to make some experiments to satisfy myself and others as to the real state of the case. My experiments were made on dogs as well as rabbits. For ascertaining the *modus operandi* of a substance of known poisonous qualities, rabbits answer well, from their not being liable to eject the poison by vomiting; but for determining whether or not a substance may prove fatal to human beings, dogs, as more resembling man in organization and habits, are much to be preferred. The condition of the stomach as regards repletion can be easily regulated in dogs by keeping them from food; but in rabbits uniformity in this respect can never be attained, for even when they have been kept from food for days, masses of undigested vegetable matter will be found in the stomach.

In all the following experiments the oil was administered by injecting it mixed with a little water into the stomach through an elastic tube.¹ The following trials were made upon dogs:—

¹ The experiments were made at the Veterinary College; and I have to express my thanks to Mr Dun, lecturer on Materia Medica there, for his valuable assistance.

EXP. I.—To a small terrier weighing ten pounds were given twenty-five minims of oil rectified from a mixture of caustic potash and quicklime. This oil, by the iron test, was perfectly free from prussic acid. No visible effect of one kind or another was produced in half an hour. The dog then ate some meat, and continued quite well.

EXP. II.—Two drachms of another sample of rectified oil were given to the same dog seven hours afterwards. In twelve minutes he vomited a little slimy matter smelling of the oil; but he was not in the least distressed. In thirty-five minutes he was quite well, and three and a half hours afterwards, fed heartily.

EXP. III.—One drachm of the same oil used in Exp. II. was given to another dog weighing eleven pounds. It had produced no effect in half an hour. He then became quite lively, having recovered from the usual agitation of the experiment, and remained quite well for a period of several hours, during which he was under observation. Later in the day he had vomited a little, but in the evening was quite well, and ate greedily.

The oil used in these two last experiments had been redistilled from potash and lime, and was reported pure by the gentleman who prepared it; but on applying the iron test to half a drachm of it, I got from it a very decided precipitate of prussian blue.

These experiments show that in doses of one or two drachms, oil which has been rectified, but in which some hydrocyanic acid may still be detected, is not poisonous to dogs. Instead, however, of applying to them this somewhat hazardous signification, I would rather have them interpreted as showing that the iron test, as applied to the oil, is of sufficient delicacy to detect traces of prussic acid so small as not to be poisonous.

EXP. IV.—To a dog, weighing sixteen pounds, three drachms of this same oil were given. No effect was produced for twenty-five minutes, when he vomited once. No other symptom showed itself, and the animal was quite well next day.

EXP. V.—To the same dog, as in Nos. I. and II., three drachms of another sample of rectified oil were administered. This oil, with the iron test, gave so infinitesimal a trace of prussian blue, that it may be said to have been free from hydrocyanic acid. The dog vomited slightly in thirteen minutes, and, in about three minutes more, passed some urine smelling distinctly of the oil. No further symptoms were produced in half an hour. Shortly after that he vomited once again, but otherwise remained quite well.

The exemption from the action of poison in these trials cannot be ascribed to the expulsion of the oil by vomiting; for in one it did

not occur for 25 minutes, and in the other not until a quantity of the oil had been absorbed into the circulation.

EXP. VI. was made, to contrast with these trials, the effects of the unrectified oil. Half a drachm of ordinary commercial oil was administered to a dog weighing eighteen pounds. In three minutes he fell on his side in strong convulsions, and in five minutes more was dead.

The following experiments were made upon rabbits:—

EXP. VII.—One drachm of the oil, not quite free from prussic acid, used in Exp. II. and III., was given to a middle-sized rabbit. No effect ensued for seventeen minutes, except rapid, rather strong, action of the heart, and quick breathing, which at that period was, 96 in the minute. In twenty-five minutes the pupils were sluggish but not dilated. The animal was dull, but moved readily when touched. It remained quite well for four hours, at which time it fed heartily. It was apparently well next morning, but died early in the subsequent day, thirty-nine hours after getting the oil. On dissection, the muscles were very rigid; there was no odour of the oil in any part of the body. The stomach was full of food, and the intestines of semifluid fæces. The mucous membrane of the stomach presented a moderate diffuse redness, but no extravasation. Both lungs were of a bright scarlet colour, and the lower lobes, on both sides, were in the congestive stage of pneumonia.

EXP. VIII.—Two drachms of the same oil were given to a strong white rabbit. In five minutes it was dull, and not easily roused, breathing 76 per minute. In eight minutes it lay on its side, and did not attempt to move any of its legs. In ten minutes the pupils were closely contracted, though in a dull light—the animal was an albino. All the muscles became relaxed; it made a feeble attempt to cry; the breathing became rapid and irregular; there were twitchings of the muscles of the head and neck, and it died in sixteen minutes, without any farther approach to convulsions.

On dissection, there was a slight smell of the oil in the cellular tissue, more distinct in the blood, very marked in the abdominal cavity, and intense in the stomach, the mucous membrane of which was of a natural colour. The bladder contained some urine, with a slight smell of the oil, and yielding readily, by the usual process, the characteristic crystals of hippuric acid. This, with Exp. V., enables me to add my testimony to the observation of Wöhler and Frierichs, that the oil appears in the urine in the form of hippuric acid; but if the dose be large, that part of it passes unchanged.

EXP. IX.—Two drachms of another sample of oil, entirely free from prussic acid, were given to an albino rabbit, of middle size. In seven minutes the animal passed some urine, quite free from the

smell of the oil. At ten minutes it fell languidly on its side, and lay motionless, the pupils contracted, and the ears cold. The heart's action was rapid and feeble, and the breathing exceedingly quick, short, and ultimately irregular. At nineteen minutes the animal was moribund. Being anxious to try the effects of cold affusion, so valuable in poisoning with hydrocyanic acid, I applied it to this animal. Under the action of a full stream of cold water the breathing for a while became deeper and more regular, but it soon became irregular again. The rabbit, however, did not cease to breathe till fifty minutes-after getting the oil. I have little doubt that life was prolonged by the cold affusion.

On dissection, the mucous membrane of the stomach was found to be of an uniform pale pink colour, but no extravasation of blood. Both sides of the heart were much distended with blood. All the other organs were natural.

EXP. X.—Two drachms of oil, rectified by myself ten months ago, and both then and now found to be free from hydrocyanic acid, were given to a very strong buck rabbit. After the first agitation had subsided, no symptoms were observed for twenty minutes, when the animal looked dull and languid. At thirty-two minutes, however, it revived and ate a little bit of cabbage. It was watched for one hour, and during that time appeared quite well. It was again seen, after four and a half hours, apparently well, but it died in the course of the night.

On dissection, the mucous membrane of the stomach was found slightly reddened in patches. All the other organs were healthy. The bladder contained a little urine, smelling feebly of the oil.

These two last experiments show, that in rabbits death will ensue from hydruret of benzule, quite free from hydrocyanic acid. But, before fixing our ideas as to its activity as a poison, let us compare it with another volatile oil, which could have no possible connection with hydrocyanic acid. That which I used was the oil of cloves, as being, in pungency and density, the likeliest to hydruret of benzule.

EXP. XI.—A drachm of oil of cloves was given to a middle-sized rabbit. The breathing became rapid, as usual, but no bad effects followed. The animal was quite well at three and a half hours, fed freely, and remained well.

EXP. XII.—Two drachms of oil of cloves, after an interval of two days, were given to the same rabbit. In eight minutes it began to grow very dull, the ears became cold, the heart's action was rapid and strong, the breathing slow and irregular. At twelve minutes the iris was somewhat dilated, and barely sensible to strong light. In fifteen minutes the animal lay motionless on its side; the breathing was slower (32 per minute), and more irregular. There was a temporary slight return of heat in the ears, but the breathing continued

slow and irregular; the extremities became cold, and the animal died in an hour and a quarter.

On dissection, there was found some serous effusion into the peritoneum, but no traces of inflammatory action there. The serum had a slight aromatic odour, but not recognisable as that of oil of cloves. The contents of the stomach smelled strongly of the oil. The mucous membrane of the stomach was not inflamed, being pale throughout its whole extent, but marked by numerous small spots of dark bloody extravasation into the submucous tissue. The lungs were healthy.

EXP. XIII.—Two drachms of oil of carraway were given to a dog weighing sixteen pounds. The animal, which was naturally sluggish, was very dull in thirty-five minutes, and continued so for three hours more. It vomited a little during this interval, and refused to eat, but no other effect was observed, and it quite recovered.

From these experiments of my own, and those recorded by others, I am led to draw the following conclusions:—

1. That the marked difference between rectified and unrectified oil of bitter almonds, shows that the poisonous action of the latter is essentially due to the hydrocyanic acid which it contains.

2. That the oil, really free from hydrocyanic acid, in doses of a few drops, does not act as a poison on animals generally; and that the instances of fatal effects on man and animals of such doses of the unrectified oil, must be referred entirely to the hydrocyanic acid.

3. That experiments on rabbits with quantities of half a drachm and under, invariably show, that if quite free from prussic acid, such doses do not cause fatal effects.

That in larger doses (a drachm and upwards), it does, even when quite free from hydrocyanic acid, prove fatal to rabbits, but with great variation as respects the rapidity of the death, which variation is due to the physiological peculiarities of these animals.

4. That on dogs, whose organization renders them much better subjects for testing the probable effects of the substance on man, doses, even so large as three drachms of the oil, entirely or nearly free from prussic acid, produce no other effect than a little vomiting, and do not cause death, or even dangerous symptoms.

5. That experiment shows, that if this substance is to be called a poison at all, it must be regarded as one of no great activity; but that, in reality, it cannot, even on the ground of its effects on rabbits, be styled a poison, without including under this denomination many other substances, such as oil of cloves, which cannot be regarded as poisonous, in the common sense acceptation of the term.

6. That the use of the purified oil to make flavouring condiments is open to no objection which would not apply to ordinary aromatic volatile oils, and that the spirituous solutions sold for this purpose, if made of properly purified oil, are not dangerous.

7. That since, by due care, the oil can be so entirely freed from hydrocyanic acid as to deprive it of active poisonous properties, great culpability will attach to the sale of preparations made with unrectified oil.

ARTICLE IV.—*On the Thickness of the Articular Cartilages at Different Periods of Life in the Human Subject.* By P. REDFERN, M.D. Lond., F.R.C.S.L., Lecturer on Anatomy and Physiology in the University and King's College, Aberdeen.

(*Read to the Medico-Chirurgical Society of Aberdeen, December 1, 1853.*)

OF the various phenomena presented to us by a living being, there are perhaps none more interesting, nor more strikingly illustrative of the action of the vital forces upon the tissues, than those of the development of different structures from the same primitive blastema, or from the same blood. What is the precise nature of the process by which a certain amount of material is converted in one position into bone, in another into muscle or nerve, it seems entirely beyond the reach of our present faculties to comprehend. We are, therefore, limited to the determination of the particular modes of development of different tissues, and the general laws which may be deduced from them. In such inquiries, nothing is more likely to lead away from the truth than prejudice in favour of a particular doctrine or system, nor does anything more surely obstruct the progress of science, or more certainly lead to the sacrifice of valuable time, than the existence of conclusions drawn by previous observers or authors from data which were not sufficiently accurate or extensive.

In investigations into the structure of healthy and diseased cartilage, commenced in the year 1848, and continued since that time, I have often been surprised at the statement that the thickness of articular cartilages gradually diminishes as life advances, owing to the conversion of the cartilage into bone; and my object in this contribution to the history of articular cartilage, is to show how far that statement is strictly accordant with facts. I do not hesitate to state that my earliest impressions were, that articular cartilages do not become gradually thinner as life advances, for I had observed them very thick in the aged on many occasions, and spongy and fibrous in old people generally. As such observations increased in number, I at length resolved to test the matter fairly, and with that view, I made a large number of measurements of the cartilages of various joints at different ages, at a great sacrifice of time and labour when compared with the importance of the result.

If the thickness of the articular cartilages concerned merely their

amount of elasticity, and their adaptation to the uses of the parts of the body to which they belong, it would be of great importance to know precisely what changes take place in them as life advances. We know that the bones and muscles of the aged person are very different from those of the young adult, and it might be expected that the cartilages would also be found changed, or, if they should not be so, we should look for some cause for this exemption in the case of tissues made use of for the same general purposes as the bones and muscles, and intended to harmonise with them in their action.

But if the assumed diminution in the thickness of articular cartilage with advancing age, be made use of as an argument in illustration of the nature of changes which take place in another texture, in the development of bone, it becomes doubly important to determine what grounds there are for the assumption which is thus founded on, lest another investigation be complicated by the introduction of erroneous data, which must be shown to be such before any considerable progress can be made.

Let us first examine the statement itself, and the authority on which it is founded ; secondly, What use has been made of it in the study of the development of bone ; and, lastly, The results of actual measurements of the thickness of articular cartilages at different ages, in a great number of instances.

Mr Toynbee, in his admirable paper "On the Organisation and Nutrition of Non-vascular Animal Tissues," read before the Royal Society, and published in the *Philosophical Transactions*,¹ says :— "The articular cartilage is gradually being converted into bone during the whole of life ; thus, it is thicker in young than in old subjects, and, as Sir Benjamin Brodie informs me, it is much thinner in old age than in the adult ; in fact, it is not very rare to find that the articular cartilage of the head of the os femoris, in many old persons, has completely disappeared—a change which is probably to be attributed to its entire ossification. In a note, Mr Toynbee adds :—"This appears to be another of the many instances of the disappearance of the animal, and the increased deposit of the earthy, constituents of the body in old age." And again, as one of seven leading facts which Mr Toynbee believes that his researches tend to establish, he gives the following :²—"Articular cartilage, during the whole of life, gradually becomes thinner, by being converted into bone."

Dr Carpenter, after speaking of the normal proportion in the length of a bone being almost wholly acquired by the growth of the cartilage between the shaft and the epiphysis, says :³—"What further increase it gains seems chiefly, if not entirely, due to the

¹ *Philosophical Transactions* for 1841, Part I. p. 167.

² *Ibid*, p. 173.

³ *Principles of Human Physiology*, Lond. 1846, 3d edit., p. 151 ; and 4th edit. Lond. 1853, p. 266.

progressive ossification of the articular cartilage covering the extremities, which progressively diminishes in thickness during the whole of life, and which in old age sometimes appears to have been almost completely converted into bone."

Thus it would seem that the statement made in Mr Toynbee's paper, has been the groundwork of Dr Carpenter's explanation of whatever increase takes place in the length of a bone, after the union of its epiphysis with the shaft. Then, as Mr Quekett states¹ that he found the articular lamellæ of the head of the humerus of a female, aged nineteen, "so very thin as only occasionally to be recognised," whilst the same part in a woman of upwards of seventy years of age was "of great breadth."² The conclusion which the reader of these statements is very likely to draw is, that articular cartilages become thinner, whilst the articular lamellæ thicken, these changes being probably proportionate to each other. Such a conclusion has not been deliberately announced; but if it be true that articular cartilages uniformly become thinner, and the articular lamellæ as uniformly and gradually thicker, as life advances, it would be perfectly warrantable to go much farther than this, and to affirm that the diminishing thickness of one tissue, and the increase in the other, are owing to the conversion of one into the other, or to the replacement of one by the other,—i. e., to one or other of those actions which we know takes place in the ordinary formation of bone.

But let us inquire *whether articular cartilages uniformly and progressively diminish in thickness as life advances or not.*

At the commencement of the inquiry, it must be observed that there are many occurrences which prevent the conclusions which may be drawn from measurements of the articular cartilages in different bodies from being as absolute and as perfect as many others. Scarcely any tissue is more speedily changed in bulk by the absorption of water into its substance, or by desiccation. The thickness of an articular cartilage can only be measured after a section has been carried through it and the bone which it covers; and the making of a large number of such sections and measurements is attended with a great expenditure of time and labour. Measurements must necessarily be taken considerably after the middle of life, and at that age it is difficult to meet with corresponding parts of large articular cartilages free from the changes which I have elsewhere³ described as a process of anormal nutrition, and which

¹ Histological Catalogue of the Royal College of Surgeons of England, vol. i. Lond. 1850, p. 130-131.

² "Nearly twice as broad" are Mr Quekett's words, in his Lectures on Histology, London, 1852, p. 148, in a comparison of the thickness of the articular lamellæ of the head of the humerus of a female of upwards of seventy years of age with that of one of nineteen,—most likely referring to the same sections as are described in the Histological Catalogue.

³ Anormal Nutrition in the Human Articular Cartilages, with Experimental Researches on the Lower Animals. 8vo. Edinburgh, 1850.

Table Showing the Thickness of the Articular Cartilages of the Bones named below, in parts of an Inch, at different Ages.

Age.	Sex.	Femur.			Tibia.		Fibula	Astragalus.			Os Calcis.		Sca-phoid.		Cuboid.		Internal Cuneiform.		1st Me-tatarsal			Humerus.		Radius.		Ulna.	
		Head.	Trochlea.	External Condyle.	Internal Condyle.	External Glenoid Cavity.	Internal Glenoid Cavity.	Lower end.	Tibial surface.	Scaphoid surface.	Concave surface for Os Calcis.	Convex surface for Astragalus.	Convex surface.	Posteriorly.	Anteriorly. (e)	Posteriorly.	Anteriorly.	Posteriorly.	Anteriorly.	Tarsal end.	Head.	Head.	Trochlea.	Head (cup).	Carpal end.	Trochlea.	Head.
23	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
23	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
25	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
25	m.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
28	m.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
36	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
36	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
45	m.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
72	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
74	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
74	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
76	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
80	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		
80	f.	$\frac{1}{4}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$							$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$		

(a) Slightly dried. (b) Fibrous and porcellanous. (c) Fibrous. (d) Section somewhat different from the others. (e) The cartilage on the ridges subdividing this surface is $\frac{1}{16}$ th of an inch thick. (f) Measured on the edge of the surface.

may, with great propriety, be styled *senile degeneration of cartilage*. Nevertheless, though the difficulties of obtaining a series of perfectly correct measurements are very considerable, it will not be doubted that the plan which I have followed is more likely to lead to the truth than that of judging from general impressions of the thickness of the articular cartilages in a series of cases examined merely by the eye, and without any definite standard of comparison.

The results of my measurements of the articular cartilages of a series of bones I have embodied in a table which is hereto appended. The thickest part of each cartilage was uniformly chosen for measurement on account of the greater facility of referring to this than to any other part in the different instances. By reference to the table the thickness of a particular part of a certain number of cartilages may be compared at different periods of life, and though the number of measurements is not so great as might be wished, many of them have been made at such ages as would most satisfactorily lead to the determination of the difference of thickness, if any such difference do actually exist at different periods of life.

It will be noticed that the greatest number of observations has been made upon the cartilage of the head of the femur and those which enter into the formation of the knee-joint, which is accounted for as follows. The greater size and thickness of these cartilages appeared to point to them as instances in which the most accurate measurements might be made, whilst their greater importance seemed to indicate that additional information regarding them would prove more acceptable than if similar information were obtained regarding the cartilages of less important joints. I anticipated no difficulty in the application of the conclusions drawn from the examination of the largest and most important cartilages in the body, to those of less size and importance, and I therefore commenced my examinations by directing special attention to the cartilages of these large joints. After making several measurements I was reminded of former observations on the change of structure of cartilage as life advances, and of the greater liability of these very cartilages to such changes, by finding them frequently so much altered after adult age, that to measure them with great accuracy was entirely out of the question. I had no longer any predilection in favour of measurements made on the cartilages of these joints, and I subsequently examined those of the smaller joints as frequently as those of the larger ones. I would rather place confidence in measurements of the thickness of the cartilages of the smaller than of those of the larger joints in advanced life for the reasons just named.

The table shows a greater variation of thickness in the different measurements of the cartilages of the femur and upper part of the tibia than of any others, and I account for this by the fact that the cartilages of those parts are most apt to become soft, velvety, and

fibrous, in which state it is quite impossible to determine their thickness with accuracy.

Notwithstanding the greater variation which occurs in the different measurements of the thickness of these large cartilages than of others, it will be found that there is nothing, either in these or in the smaller variations, observed in the other instances, nor yet in the whole series of measurements, which will warrant the conclusion that these structures become gradually thinner as life advances, nor even that they are thinner in the aged than in the young person.

From a conversation with Mr Toynbee on the statement made in his paper before referred to, I found that his evidence on this matter had been derived from the observations and statements of others, rather than from measurements or observations of his own, and thus I believe that he has been thrown off his guard, and made to appear as the author and supporter of a statement which, under other circumstances, his habitual care and anxiety to arrive at the truth, would effectually have prevented from being made in so definite a manner.

In conclusion, I may state my conviction, founded on the measurements indicated in the table appended, that *articular cartilages do not become gradually thinner as life advances, and that they are not uniformly thinner in aged persons than in early life*. I may also remark that, in endeavouring to estimate the thickness of articular cartilages at different periods of life, I have had presented to me a large amount of evidence in addition to that which I formerly made public, that, as life advances, these structures, and especially those of certain joints, change their elementary characters, and become fibro-cartilaginous, or altogether fibrous. This change has its analogues in the conversion of the costal cartilages into bone, of tendons into fibro-cartilage and bone, and in those changes which age induces in the bones generally, in the cornea and other tissues. It may not improperly be styled *senile degeneration of cartilage*.

ARTICLE V.—*Remarks concerning the Diastolic Mitral Murmur.*

By W. O. MARKHAM, M.D., Assistant Physician to St Mary's Hospital.

It has often been to me a matter of surprise that a diastolic mitral murmur should be freely admitted as a well established and frequently met with fact, by writers on the Continent, and that our own authors upon diseases of the heart should either have placed it aside, as being of excessive rarity, or have altogether denied its existence. Dr Latham speaks of such a murmur as a sort of clinical curiosity; and he observes "that it would almost seem that the

mitral orifice could be the seat of only one murmur, and that murmur the systolic;" and his opinion is more or less typical of the opinion of those of his countrymen who have followed or preceded him. Lænnec, Bouillaud, and the best observers in France and Germany, on the other hand, admit the existence of the murmur without hesitation, and write of it as a thing regularly established in the clinical history of disease.

Three cases of cardiac affections have lately come under my notice in which I have been compelled, after the most careful and oft repeated examinations, to assign the origin of the morbid sounds heard to the mitral orifice, and their coincidence in time to the diastole of the heart. These cases I will briefly relate, premising that, in every instance, I did not trust to my own ear alone, but obtained the assent of other capable observers; the difficulties of auscultation being, in my judgment, of much too overwhelming a character ever to warrant any ordinary observer speaking freely upon his own authority of a disputed point in stethoscopy.

The first case was that of a man who had long suffered from rheumatic gout. A soft murmur, distinctly falling in with the heart's diastole, was heard over the left side of the heart, beneath and to the outer side of the nipple; the sounds over the aortic valves and along the aorta, and down the sternum, were distinct and clear and natural. The murmur was permanent, for it remained when the symptoms for which he had presented himself for treatment had disappeared, viz., dropsical effusions and short breath. Am I not justified in attributing such a murmur to some roughnesses about the auricular surface of the mitral valves, or to some contraction of the mitral orifice? What other account can be given of it?

The second case is of a still more positive nature, inasmuch as the pathological conditions discovered after death fully bore out the diagnosis made during life. It is interesting also in another sense, because I think it perhaps gives some explanation of the reason why the murmur I am writing of is so seldom recorded; a reason to be found in the extreme difficulty of, and caution requisite in, its diagnosis, and particularly as regards fixing it to its proper coincidence with the diastole of the heart. I have, on more than one occasion, seen experienced observers mistake the first for the heart's second sound, when the action of the organ was great, and its sounds unnatural, and have frequently fallen into the same error myself. If the heart's beats exceed a certain number in a minute, I believe it is almost impossible, without at the same time feeling the arterial pulse in the neck, or the impulse of the heart's apex, to say which is the systolic, and which the diastolic sound; what the number of the heart's pulsations is, which so confounds the sounds and perverts the periods of their intervals, it is not possible to state, for it naturally has a relation to a variety of circumstances to which I need not here refer, and may be therefore greater or less.

The subject of this case, a girl about 17 years of age, was for

many months previous to her death constantly under my observation. She had long suffered from difficulty of breathing, and throughout her illness this was her prominent source of suffering; slight exertion increased the accustomed violence of the heart's action and the force of the respiratory movements. The signs I am about to relate were constantly present, but in a more or less marked degree, according to the action of the heart; a strong and prolonged vibration was felt where the heart's apex beat (which was to the left of the nipple), producing a distinct *frémissement cataire*. There was a loud and prolonged murmur over the precordial region, loudest over the left side of the heart, and most concentrated at its apex, becoming weaker as the stethoscope was removed further from that spot. This murmur was sometimes high-pitched and musical, as though produced by the continued tension and vibrating of a membrane, sometimes it was deeper and rougher, and this, I think, generally when the heart's action was slower; no bruit was heard along the aorta, and its sounds were audible. The rhythm of the heart's movements was entirely perverted, the movement, associated with the murmur, occupied nearly the whole of the time of the systole, the diastole, and the interval; the impulse—that is, the first sound—immediately followed the cessation of the murmur, or rather seemed to be its conclusion, and suddenly to wind it up; it was brief and instantaneous, and, after a very short pause, followed by the recommencing murmur. My examination of this case was very frequent, and careful, and minute, and I therefore speak with some confidence as to the correctness of the facts observed; and I will add that others, among whom it is sufficient to mention the name of Dr Sibson, admitted the propriety of the murmur being considered diastolic, though expressing differences of opinion as to the morbid state producing it—my own diagnosis, resting on the general symptoms and local signs, was contraction of the mitral orifice. The rapidity of the heart's action necessarily rendered the investigation difficult, but if we are to take as a settled canon that the impulse of the heart's apex is coincident with the first sound, and this we must do, unless we reject one of the best established facts in physiology, then I see not how we can for a moment refuse to admit that a murmur which precedes that beat must of necessity be diastolic. In this case, however, therapeutics came to the aid of diagnosis. Under the influence of digitalis, and through a moderate dose of it, her pulse fell to about 40; and now there was no difficulty whatever in analysing the sounds and murmurs of the heart, and the diastolic nature of the murmur was placed beyond doubt. I should mention that the frequent attacks of hæmoptysis to which the girl was subjected, and the constant oppression of the lungs, reasonably confirmed my diagnosis, by indicating the *immediate* nature of the impediment to the flow of blood from the lungs; the freedom from dropsical symptoms also suggested that the impediment was most probably not on the right side of the heart.

The girl, at her own request, left the hospital; but the exertion of removal overcame her, and she died a few hours after reaching her home. Mr Trotter, the house-surgeon of St Mary's Hospital, obtained her heart, and gives this account of its state: the mitral orifice was represented by a small slit-like opening, the segments of the valves being thickened and adherent; the aortic valves were healthy; the heart large; the right ventricle hypertrophied and dilated; the left dilated, but its walls not thickened.

The third case, which I have still under observation, presents general symptoms very similar to those in the last case, but in a much less marked degree. There is a slight *frémissement* perceptible at the heart's apex; the bruit heard immediately precedes the heart's impulse, this seeming to conclude it; it is not heard along the aorta, or over its valves; the pulse in the carotids and its branches is very weak. This girl I have still under observation; and I should observe that, in her case, difference of opinion has been held as to the nature of the murmur, though none as to the fact of the bruit preceding the beat. Slight exertion produced difficulty of breathing; and, lately, hæmoptysis has taken place. If this be not a diastolic mitral murmur, it seems to me impossible to give any account of it. A consideration of these cases, of the pathological states which produce the murmur under discussion, and of the unhesitating manner in which some of the best continental writers speak of it in their text-books, has inclined me to the belief that there must be something erroneous in the views which we take of it in this country; and I will presently suggest what are, in my opinion, the possible sources of the error.

How can be reconciled the statement of Dr Walshe, "that he does not ever remember to have observed cardiac thrill synchronous with the ventricular diastole," with the history of two of the cases I have described, and with the statements of men like Hamernjk, and Skoda, and Corvisart? "When the contraction of the mitral orifice is great," says Hamernjk, "the second murmur is long and loud; and some portions of it are louder than others, producing the hum of a spinning-wheel, and so has been taken for a double sound. Such a second (diastolic) murmur is protracted, *and ends in the systole*; there is a distinct pause *between the systole and the diastole* of one complete heart's movement, but none between the diastolic and recurring systolic murmurs." How exactly does this account agree with the description of the second case given! "It is especially," says Skoda, "in cases like these that vibrations are felt when the hand is laid upon the precordial region—the *frémissement cataire* described by Lænnec." "It is a common opinion," says Professor Jacksch of Prague, "that a fremitus which is felt at the apex of the heart, and which accompanies the diastole, is a pathognomonic sign of an obstructed mitral orifice."

Corvisart speaks, when relating of contracted mitral orifice of the "*bruissement particulière, difficile à décrire, sensible à la main ap-*

pliquée sur la région précordiale," etc. Are all these authorities mystified?

Now, I will venture to offer, as explanatory of such great contrarieties of opinion on a matter of fact, two suggestions which seem to me reasonable. The first is, that, from the rapid and tumultuous action of the heart in advanced conditions of the disease, from the complete change in the rhythm of its movements, from the altered character of its sounds, and from *not fixing the exact time of the heart's systole by feeling its beat*, a diastolic has been often mistaken and set down for a systolic murmur. The next suggestion is, that we have approached the subject with preconceived physiological ideas, and have prejudged the question, and wrongly. "Thus," writes Dr Latham, "the blood glides into the ventricles without any impelling force from behind," and *therefore* the onward current of blood is still without noise. Dr Williams, who says that the murmur is of excessively rare occurrence, explains it by supposing the left ventricle to be much thickened, and so to have gained diastolic elasticity, and "to suck the blood from the auricle with some force;" but, in one case I have related, the left ventricle was not thickened, though the diastolic murmur was exceedingly loud. Few persons will, I suspect, accept this explanation. The want of an impelling force to drive the blood through the contracted orifice has likewise been, with many other writers, a generally adopted axiom, by which observers have manifestly allowed their judgments to be guided; is it a true one? It is impossible to discuss the question here; but I will remark that Hamernjk, in his "Researches," etc., has proved to my mind beyond all doubt, by his observations and lucid reasonings, that the actions of the respiratory forces are, especially in cases of impeded circulation, and therefore markedly in contraction of the mitral orifice, brought to bear with great energy upon the movement of the blood, and are quite sufficient to account for that character of the murmur which gives to the ear the idea of fluid being forcedly driven through a narrowed aperture. I cannot but think that further investigation will show, that in this instance, as in so many others, the philosophic thought of Corvisart has found an exemplification: "à combien d'erreurs, même grossières, ne serait pas continuellement exposé celui qui atteindrait ainsi les phénomènes morbifiques aux notions de la physiologie, et qui trouveroit sans cesse dans ces notions, trop souvent hypothétiques, la connaissance des phénomènes qui doivent caractériser telle ou telle affection!"

It may perhaps be suggested, that, after all, it is a matter of little consequence in practice whether the murmur heard be systolic or diastolic. To this I will answer, that diagnosis cannot be too minute so long as its minuteness is capable of distinct appreciation, as I affirm it is in the present case.

As a resumé, I maintain,—

1st, That a diastolic mitral murmur is not so rare a phenomenon as it is generally held to be.

2d, That it is set down as rare, because the altered sounds and perverted periods of the rhythm of the heart causes a confusion of its diastole with its systole.

3d, That this confusion can only be avoided by feeling the heart's apex beat, while listening to its sounds.

ARTICLE VI.—*Observations on the use of Opium as a substitute for Purgatives in severe cases of Obstruction of the Bowels.* By GUSTAVUS EVANS, M.D., Carlisle.

NOTWITHSTANDING the frequency, alarming character, and an fatal termination of those cases which belong to that class of diseases of which colic is the type, and where there are symptoms clearly indicating a serious obstruction to the natural passage of the contents of the bowel, there is not to be found in works on the practice of medicine a system of treatment specifically based upon physiological indications. If we except the treatment of idiopathic enteritis, we find that authors sanction the purgative plan of treatment, including the exhibition of croton oil and other powerful cathartics. They, however, admit that it is absolutely impossible to determine with any degree of certainty the existence or non-existence of a mechanical body; its precise character, and whether removeable or not. Dr Watson and Dr Copland wisely denounce in strong terms the exhibition of purgatives in obstructed bowel connected with idiopathic enteritis; they, however, admit that in colic, inflammation may be present without any indication of its existence. Dr Copland says that "the pulse is often a most fallacious guide in every form of colic and ileus; and that the diagnosis between colic and inflammation cannot be stated with precision, for there is no symptom which can be relied upon, for inflammation with its consequences, may exist, and yet the abdomen may not be painful on pressure;" he even enumerates among the aids to a correct diagnosis, the operation of the remedies administered. Dr Watson would discontinue the exhibition of purgatives in colic after a fair trial, from the conviction that the mechanical obstacle is such that it cannot be overcome. Alluding to the purgative plan of treatment, he says:—"Common sense and common humanity answer, you must stop it the instant you are convinced that there is a mechanical obstacle which cannot be overcome; to persist in the use of drastic purgatives after that conviction is to inflict wanton and needless torture upon the patient." The reader who may be inclined to peruse what has been written by the following authors¹ on colic and ileus will not be surprised

¹ Mackintosh's *Practice of Physic*, chap. v., p. 247; Abercrombie's *Researches on the Bowels*, p. 134; Copland's *Medical Dictionary*, vol. i., p. 360; Watson's *Lectures on the Practice of Medicine*, vol. ii., p. 433; Tweedie's *Library of Practical Medicine*, vol. iv., p. 125; Eliotson's *Lectures*, p. 648.

when I state that the pathology and treatment is most indefinite, unsatisfactory, and uncertain. It would seem from the purgative treatment sanctioned that the pathological phenomenon of spasmodic intestinal stricture has been entirely overlooked, and that the primary indication of treatment is the removal of constipation, and that evidently without any special reference either to the causes giving rise to it or to the altered physiological conditions leading to spasmodic constriction of the intestine.

However, I maintain that the primary and most important indication to be pursued is the alleviation of spasmodic constriction, and that this is scientific, and in accordance with physiological indications, and in no case, however ambiguous, productive of mischief, nor even distress, to the patient. For supposing a removeable mechanical body existed which produced local irritation of the nervous fibres of a given portion of the intestine, and consequently spasmodic contraction of its muscular coat, it is manifestly clear that it can only be removed by the equable and consentaneous action of the intestine, and that, after the alleviation of spasmodic constriction.

The secondary and comparatively least important indication of treatment is the removal of constipation; and this it appears scientific to accomplish by the repeated injection of copious lavements, instead of the exhibition of purgatives by the mouth. For should the obstruction arise from causes which can only be discovered by a post-mortem examination, such as intussusception, internal hernia, or a tumour within the peritoneal cavity, after the partial or complete alleviation of general and local nervous irritation, the forcible injection of copious lavements is the only rational means of relief which the physician can with safety adopt.

I will now briefly detail two cases, the former of which came under my observation prior to having thoroughly and inwardly digested the principles upon which its treatment was based; however, the following apt observation of my esteemed and talented friend, Dr Henry Lonsdale, who saw the case in conjunction with me, will show the indications of treatment pursued, and in reference to which we were both perfectly agreed:—"Let us tranquillize the nervous system, and trust to opening the bowels from below." I consider this brief remark full of the most comprehensive and valuable import, and applicable in its practical bearing, to all these ambiguous cases:—

CASE I.—*Aug. 21, 1853, 9 A.M.*—I was requested to visit Mr P., aged 42, of good muscular development, possessed of a strong vigorous constitution, and ordinary good health. I ascertained that he had been to Shap Wells, and had drank moderately of the water; had suffered from a slight attack of diarrhoea, which entirely subsided about two days before his return. He now had all the symptoms of a severe colic; the abdomen slightly more distended than usual; severe twisting pain over the left iliac and umbilical regions, not aggravated on pressure, but rather relieved; constant nausea, with occasional vomiting; constipation, accompanied with a great sense of bearing down, and frequent inclination

to go to stool. The paroxysms of pain, which were frequently attended with vomiting, were of a very severe description, and returned about every 15 or 20 minutes with a hard and contracted state of some of the abdominal muscles. The pulse was full, soft, and only quicker than ordinary during the paroxysm; the tongue perfectly moist, and presented a healthy appearance; the skin rather hot and dry. Administered Tr. Opii ʒiss. Ol. Ricini ʒss., hot applications to the abdomen. At 10 A.M. the pain had considerably abated in severity, and he was comparatively easy. Ordered a pill, containing Pulv. Opii gr. i., Hydr. Chlorid, gr. iss., every two or three hours; or at shorter intervals, should the pain in the least assume its former features. 8 P.M.—Had taken three doses during the day; pain entirely abated; bowels moved; motion rather watery; pulse quiet. Ordered to be taken, at bed-time, Ol. Ricini ʒvi., Tr. Opii m. xv.

Aug. 22.—Was called to visit him at 6 P.M. He had slept about four hours, but awoke with a similar paroxysm to the first, and his attendants immediately gave him a dose of castor oil. He had all the previous symptoms, but the paroxysms were decidedly much more severe, and regular in their occurrence at shorter intervals, with but slight intermission of pain. His countenance was anxious, and indicative of excruciating suffering; vomiting for the first time was distinctly stercoraceous; his pulse was upwards of 100, but full and soft; tongue quite moist. Gave him ʒiss. Tr. Opii in water, and very shortly after a copious injection, containing about ʒi. Ol. Ricini, Ol. Terebinthinæ ʒi. Tr. Opii ʒiv. This was soon returned with a quantity of dark flocculent fecal matter. In about forty minutes he took, in water, Tr. Opii ʒss. The pain continued somewhat abated for about an hour, after which it returned in all its severity. He was then bled, and fully 28 ounces of blood were removed before any impression was produced upon the pulse, or syncope supervened. He then took Tr. Opii ʒiv.; the pain soon after considerably abated, both in severity and frequency, and the vomiting had almost subsided.

At 9 A.M. he was seized with another severe paroxysm of pain: Pulv. Opii gr. iss. Hydr. Chlorid gr. ii. was administered to him; hot fomentations to abdomen, to which had previously been applied a mustard plaster, a copious warm injection containing a ʒiss. of chloroform, which was shortly followed by the same effects usually produced by its inhalation, and with marked relief, although after its influence had subsided, the pain was more or less felt, but not nearly so severe. 10.30 A.M. Dr H. Lonsdale saw him in conjunction with me: he was then considerably easier than he had previously been. There was but slight tenderness over the abdomen; pulse full and inclined to hardness. It was agreed to persevere with the use of injections; hot turpentine applications to abdomen; occasionally sponging with vinegar and water; and to administer a pill containing Pulv. Opii gr. iss. Hydr. Chlorid gr. iij. every two hours, the opium to be increased according to circumstances, and to repeat the bleeding, if the pulse indicated.—3 P.M. During the previous hours the paroxysms had materially abated in severity and frequency, and the vomiting had almost entirely subsided. The iliac and umbilical regions were very sensitive and painful on pressure; pulse one hundred and twenty, small and hard; a copious warm injection was administered, from which the patient experienced great ease; the greater portion was retained, the other voided with a quantity of similar feculent matter, to that passed before, also a little of a more natural quality. About fifteen ounces of blood were removed, this was followed by the most perfect syncope, from which he very slowly rallied. In about thirty minutes he took a pill containing Morphia Hydrochl. gr. i. Ext. Hyoscyam gr. iv. after which he slept about five hours, his skin warm and covered with a copious perspiration.—11 P.M. Still continued inclined to sleep, but expressed himself free from all pain; the pulse soft and quiet.

Aug. 23.—Had slept soundly during the night; continues free from pain; pulse quiet and soft. Ordered beef tea, which he took with evident relish; bowels moved during the day; a tolerable healthy motion.

Aug. 24.—Doing well. Suffering slightly from the effects of the mercury. Took the following aperient draught, to be repeated occasionally when necessary :—Tr. Rhei. C. Ziiss., Tr. Card. ʒi., Mannæ ʒii., Pulv. Rhei. gr. viij., Tr. Zingib m x. Aquæ Menthæ Pip. ad. ʒi., Mt. Haustus.

Aug. 25.—Continues well ; bowels moved ; motions natural.

Aug. 30.—He is perfectly well, and tolerably strong.

CASE II.—*Sept. 8, 1853.*—9 P.M. I was requested to visit Mrs S., aged 28, a very delicate and slender woman, of only moderate muscular development ; general health not very good ; her appearance upon the whole indicative of deficient constitutional vigour and energy. I ascertained that during the previous fortnight she had suffered from the effects of cold, pain in her back and limbs, accompanied with slight bronchial irritation, and on the 6th was seized with pain over the left iliac, extending over the lower portion of the umbilical region ; she also had paroxysms of more severe pain, which varied in frequency from fifteen to twenty minutes. She distinctly recognised these attacks, and states that occasionally she was free from them for two or three hours, although she constantly felt a certain amount of tenderness over the part. She had constant nausea, with occasional vomiting ; occasional watery stools, accompanied with pain, and a frequent inclination to go to stool. She states that the only motion passed that day was watery, and contained a little blood ; and had scarcely slept any for the last three nights.

Symptoms.—Anxious expression of countenance ; abdomen natural size ; no tympanitis ; acute sensitiveness and extreme pain on pressure over the left iliac and lower portion of the umbilical regions. During the only paroxysm that occurred since my visit, there was extreme hardness and contraction of some of the abdominal muscles, and below the umbilicus there was distinctly felt by the hand a hard, narrow, contracted substance, about three or four inches long. Tongue moist, and natural in appearance ; skin dry, and of moderate temperature ; slight thirst ; constant nausea, and occasional severe vomiting ; pulse upwards of 100, small, hard, and wiry. Took about twenty ounces of blood from the arm before the slightest syncope occurred ; hot turpentine applied to the abdomen, Gum Opii, gr. iij., Hydr. Chlorid, gr. iij., in a pill immediately given, and a pill containing half these quantities to be given every hour. 12 P.M.—Pain almost entirely abated, with little or no increase on pressure ; pulse about 90, and soft. Administered an injection containing about five pints of warm water, Ol. Olivæ, ʒiij., Tr. Opii, ʒss. This was returned with a quantity of slimy, unhealthy matter, and about an ounce of small, soft portions of fecal matter. Took Gum Opii, gr. iij., Hydr. Chlorid, gr. iss., and the former pill to be repeated every two hours.

Sept. 9th, 8 A.M.—Pain entirely removed, with not the slightest uneasiness on pressure ; had slept about two hours, and had no vomiting ; pulse about 100, soft and feeble. Administered a copious injection, containing Ol. Ricini, ʒij., Ol. Terebinthinæ, ʒiss., Tr. Opii, ʒij. The greater part was retained, the other rejected, containing a large quantity of white flakes of slimy matter, a few streaks of blood, together with nearly an ounce of similar fecal matter to that previously passed. Beef-tea to be taken frequently at short intervals, and a small quantity of brandy and water every half hour or hour, according to circumstances. Took Gum Opii, gr. iij., Hydr. Chlorid, gr. iss. The only manifest indication to be followed at this stage was the judicious support of the system, and its tranquillity. 10 P.M.—Strength not diminished, although not increased ; indications of a return of vomiting ; still perfectly free from pain. Administered a copious injection, with Tr. Opii, ʒiij., and ordered Gum Opii, gr. ij.

Sept. 10th.—Pulse soft and very feeble ; had slept a few hours ; evidence of serious prostration ; stomach very irritable ; beef-tea, brandy, and water administered during the day ; a starch enema, about a pint, with Tr. Opii, ʒiij. During the day, the stomach more frequently rejected than it retained what

was taken. 11 P.M.—Slight pain on pressure over the stomach. With a view of allaying irritability of the stomach, as well as producing sleep, administered Etheris Sulph. C. ℥ij., Tr. Opii, ℥i., and ordered a mustard plaster over the stomach.

Sept. 11th.—Vomiting and nausea had entirely subsided after the draught; slept soundly fully five hours; prostration considerably less; strength returning; pulse quiet; to take no medicine or stimulants, but simple nutritious food.

Sept. 12th.—Continues to improve; suffering slightly from the effects of the mercury; simple nutritious food as before.

Sept. 13th.—Still improving; pulse quiet; complains of slight headach. To take the following draught, Tr. Rhei. ℥ij., Tr. Card. ℥i., Mannæ ℥ij., Pulv. Rhei., gr. x., Tr. Zingib, ℥xii., Aquæ Menthæ Pip. ad ℥iss.

Sept. 14th.—Pulse quiet, and very much stronger; appetite good; bowels thrice moved; motions copious, consisting of healthy fecal matter.

Sept. 15th.—Expressed a desire to sit up, and says she feels comparatively strong; appetite still good; bowels naturally moved; motions perfectly healthy.

Sept. 18th.—She is quite well, and tolerably strong, and says she never felt better.

The reasons which induced me to refrain from the exhibition of purgatives in the early stage of these cases, were,—*Firstly*, The inability to determine, with absolute certainty, that inflammation did not exist, either as a cause or as an effect. *Secondly*, That there was not an irremovable mechanical obstacle in existence. *Thirdly*, That whatever the nature of the mechanical obstacle (if any existing), the true pathology of colic and ileus, in its earliest stage, beyond all doubt, must necessarily embrace intestinal spasmodic constriction. *Fourthly*, A reference to the therapeutic action of purgatives, in some respects, shows an analogy between their physiological mode of operation and that of inflammation or of a mechanical body, which, however, by producing undue irritation only of a limited portion of the bowel, gives rise to inordinate muscular contraction. *Fifthly*, That if a portion of the bowel be closely constricted, in consequence of either sympathetic action, inflammation, or the presence of a removeable or irremovable mechanical body, purgatives, if retained by the stomach, must necessarily act upon that portion of the bowel above the seat of stricture, and would be eminently calculated to lead to ileus, by inverting the peristaltic action of the intestines. *Sixthly*, A reference to the physiological action of purgatives and the pathology of colic show, that purgatives are contraindicated, inasmuch as they are calculated to develop inflammation, and also to aggravate it if present; and by increasing both general and local nervous irritation, conduce to keep up the immediate exciting cause of constriction of the muscular coat of the intestine.

My reasons for perseveringly adopting the opiate plan of treatment were,—*First*, That general or local nervous irritation, either singly or combined, is the exciting cause of intestinal spasmodic constriction,—its alleviation, a rational and scientific treatment, devoid of speculation, I would consider as the primary indication of pursuit.

Second, That the pursuit of this primary indication of treatment is in accordance with physiological indications; and however ambiguous the case may be, is perfectly safe.

Many arguments might be adduced in support of the superiority of copious injections to purgatives exhibited by the mouth, to accomplish the secondary indication of treatment,—namely, the evacuation of the bowels; but suffice it, that to administer purgatives, unless we could absolutely determine that an irremovable mechanical obstacle did not exist, is purely speculative, and consequently empirical.

And since we know that it may exist undiscovered during life, copious injections, forcibly thrown into the bowels, is clearly the best means to remove it, in cases either of intussusceptio or internal hernia, and by equally distending and stimulating the intestinal canal, is attended with less danger of renewing the spasm; and if the stricture was originally incomplete, or had been partially reduced, we can readily conceive that a portion of the injection passing upwards through the partial stricture would, on mechanical principles, distend it, this it would also do when the upper portion of the bowel, by its increased peristaltic action, would force downward the same column of fluid, which I apprehend would mechanically distend to some extent, on the same principle as the common wedge.

The beneficial effects of early bleeding in these cases may be attributed to its influence in subduing inflammation, preventing its development, or even alone subduing the spasm; but it would powerfully conduce to further the action of opium in allaying nervous irritation, absolutely essential to the reduction of the spasm. The calomel was likewise calculated to be of great service, from its well-known power to check inflammation and its consequences, and also, by stimulating the liver, lead to an increased discharge of bile into the intestines, which would act as its natural stimulus. The repeated application of hot turpentine to the abdomen, I consider, in these cases, well calculated to diminish local congestion, and also contribute to the alleviation of that spasm of the abdominal muscles, which always more or less accompanies a severe colic; but this application cannot be employed if leeches are applied to the abdomen.

Sydenham,¹ like authors of the present day, confusedly mixes up the opiate and purgative treatment; but, from a reference to his work, it would appear that in some cases he adopted exclusively the opiate treatment with great success; however, he does not lay down any special principles of treatment, but in a vague manner sanctions the exhibition of purgatives. This is not so surprising when we consider the imperfect pathology of his day; but now, that pathology and physiology have made such rapid progress, it is necessary that, if our treatment is to assume a scientific character, and be divested of all empiricism, it should at least be in accordance with physiological indications.

¹ Sydenham's Practice of Physic, p. 145.

ARTICLE VII.—*On the Prevalence of Chromato-Pseudopsis or Colour-Blindness ; its Evils, and the Means of Diminishing its Frequency.*
By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.

(Continued from p. 507. Vol XLVII.)

III.—*General Conclusions concerning the Colours perceived with most difficulty, or most liable to be confounded with each other by the Colour-Blind.*

IN the first section of this paper I have made some observations on the colours most liable to be confounded by the colour-blind. I propose now to consider the subject more fully, with special reference to the conclusions deducible on this point from the cases which I have reported. This may be most conveniently done by making each colour the text for some remarks ; and first of the colours generally accepted as primary, viz., red, blue, and yellow.

Confusion of Red, Blue, and Yellow, with each other.

The extent to which the primary colours are confounded with each other, varies very greatly, according to their intensities. It will suffice to regard them as offering themselves to the eye in the three modifications of *full* colours, dark shades and light shades.

So far as I have observed and read, it is not characteristic of colour-blindness to confound one of the full primary colours with one or both of the others. There are persons as already noticed, who are reputed to be unconscious of every colour, and who, of necessity, see no difference between the purest and deepest red, blue, and yellow. But such parties cannot be said to confound these colours with each other. They neither distinguish nor confound them ; they simply ignore them.

There are persons also like Mr P. (p. 388), and Dr Y. (p. 491), uncertain as to all colours, and therefore *liable* to confound those which are primary with each other ; but on consulting their cases it will be seen that they rarely do confound these, and that their difficulties are between primary colours on the one hand, and secondary or more complex colours on the other. Dr Bryce mistook a very red scarlet for yellow (p. 392), but this was at a distance, and may be explained by supposing that his eye being blind to red, he saw only the yellow in the scarlet. Professor N., however, confounds carmine-red with blue (p. 501), and this appears an undoubted example of the confusion of one pure primary colour with another. Other examples, doubtless may occur, but certainly few eyes are so false in their vision of colour, as to regard as similar or identical chrome-yellow, carmine-red, and ultramarine blue, when these are

displayed, undiluted, and undarkened, in white light, such as that of the sun, of the ignited lime-ball, or of the voltaic arc. Full blue and full yellow contrast more strongly in these circumstances with each other, to colour-blind eyes, than red does with either; and it is with blue rather than with yellow (as will appear in the sequel) that red is liable to be confounded.

Shades of the primary colours are greatly more liable to confusion with each other by all eyes than the full colours are. There is a very marked difference as to the point at which eyes reputed normal cease to perceive red, blue or yellow, when these are darkened by the addition of black, or diluted by the addition of white. So far as the deeper shades are concerned, blue suffers first from darkening, and soon becomes undistinguishable from black; red disappears next, and yellow survives longest. To a colour-blind eye, on the other hand, red, which is always the least visible of the primary colours, appears first to become indistinct by darkening, then probably blue, and finally yellow; and all of them become sooner indistinct, *i. e.*, after a smaller amount of darkening, to the abnormal than to the normal eye. This is an important fact which has not hitherto been noted otherwise than incidentally. A review of the cases which I have published, and of those detailed by others, will justify the conclusion that the sensitiveness to colour of a colour-blind eye suffers sooner from the withdrawal of light than that of a normal eye; and the fact is the more interesting that perception of form in partial darkness appears to abide longest with the abnormal eye. Thus Mr P. states, that he is "very uncertain about any colour when very dark" (and his "very dark" may be measured by his calling oxide of chromium black, (p. 389), and that he distinguishes "bright and transparent colours best;" yet, whilst working as an amateur photographer, he found a very faint light, sufficient to see by. Dr Y. (p. 492) regards the quality of his vision, in reference to form and outline as "first-rate," but browns, russets, maroons, olives, citrines, and a host of others, are "anything he can guess at," and purple he confounds with black.

Mr N. is quite insensitive to olive, or anything approaching to brown, chocolate, etc. (p. 496.)

Mr T. thinks himself better off than his neighbours, so far "as strength of vision, and clear definition, both of near and distant objects are concerned" (p. 498), yet he states, that "very dark green, brown, blue, claret, or black cloths, seem to be cut from the same piece."

The countess of D. cannot confidently decide between "red, green, black, brown and lilac," (p. 504.)

From these cases it will be seen, that dark shades of all colours, compound as well as primary, are confounded with each other by the subjects of Chromato-pseudopsis, and that it is only the expression of a fact to say, that, apart from confounding colours, they become blind to those they can see, in partial darkness, much sooner than others

do; or to state the truth otherwise, the diminution or withdrawal of light sooner arrests the sensitiveness of a colour-blind than of a normal eye to colour.

It is more characteristic of a colour-blind eye, however, to confound the light shades of the primary colours than it is to confound their dark shades. When red, blue, or yellow is diluted with white a point is ultimately reached with every eye, at which the excess of uncoloured light accompanying the colour renders the retina insensitive to the impression of the latter. The light shades appear to disappear to a normal eye in the reverse order of the dark shades, yellow being the first to become undistinguishable from white; then red; finally blue. Pale pink and pale yellow are undistinguishable by many eyes, not otherwise faulty in their discrimination of colours; and Wilkie was not the only painter whom Haydon might have reproached for painting his flesh-tints in yellow. Blue, on the other hand, must be very largely diluted before it becomes invisible to normal eyes, or is liable to confusion with light red or yellow. To the colour-blind, again (especially to those who frequently mistake full red), pink and light blue are constant puzzles, and this long before they have undergone great dilution with white. A reference to the cases of Mr Hughes, Dr Y., Prof. N., Mr R., Admiral —, and the six parties authenticated by Mr S—— will illustrate this. Further, cases not unfrequently occur of persons confounding pink and blue, who do not, so far as they are aware, confound other colours. Of these I have given three examples (p. 384) occurring in dyers.

The pinks thus confounded with blues include diluted crimsons, *i. e.*, red with a little blue in it, as well as diluted reds; and it is not certainly surprising that an eye which cannot distinguish pale blue from pale red, when these are placed side by side, should confound these when they are mingled together. But there is this difference to be noticed between crimson and pure red, that the deeper the latter the less liable is it to confusion with blue equally deep, whereas the deeper (in the sense of the more purple) a crimson, the more undistinguishable does it become from full blue, as will be noticed more at length further on.

Altogether, then, it may with great confidence be stated to be a marked characteristic of colour-blindness to confound with each other the *light* shades of the primary colours, and by the term “light” is not signified a mere tinge of colour. Dr Y., for example, sees roses of all shades of red, *blue* (p. 492); and the skeins of pink and light blue which I have seen put side by side as matches, were, to my own eye, conspicuous for their amount of contrast of colour.

In connection with this matter, I notice that the Countess of D. (p. 504), and Mr P. (p. 388), both mention of their own accord, that pale shades of *all* colours are perplexing to them; and the general conclusion may be drawn, that as a colour-blind eye suffers sooner in sensibility to colour, from the removal or diminution of white

light than a normal eye, so it also suffers sooner from an excess of uncoloured light. Its perception of colour is limited in the direction both of dark and of light shades much more narrowly than that of a normal eye, whilst its perception of form and outline is as acute, if not more so, especially when light is feeble.

Confusion of Primary with Complementary Colours—Red with Green; Blue with Orange; Yellow with Purple.

By far the most important variety of colour-blindness is that which shows itself in the confusion of primary with complementary colours, and especially of red with green. Before discussing at length the difficulties which attend the perception of red, I shall dispose of the little I have to say concerning the other primary colours.

I am not aware of any case having occurred where blue was habitually mistaken for orange; nor have I seen any indications of a tendency to such a confusion among the colour-blind whom I have examined.

The confusion of yellow with purple appears to be equally rare, and I am not aware of any example of its occurrence being on record. The great unlikelihood of its presenting itself had been brought so forcibly before me by Professor Kelland, in some important observations on colour-blindness, which will be given in the sequel, that I had ceased to look for cases, when I unexpectedly encountered, in quick succession, two examples of at least an approximation to the identification of yellow with its complementary purple. Both occurred in the persons of artillery soldiers at Leith Fort. Whilst testing the colour-vision of one of them, I showed him a square of chrome-yellow paper, which he at once pronounced to be purple. Startled by so unlooked for a reply, I said nothing, but put in his hands a bundle of coloured wools, and begged him to select the purple skeins. The first he selected was chrome-yellow, then he picked orange, pink, crimson, red-purple, and purple-brown, as if he were feeling his way from yellow to purple, which last, however, he did not reach. He stopped at the purple-brown, and continued without saying a word, to retain the yellow skein in his hand as if it came nearest to his idea of purple. I asked him to explain what he understood by this colour, but he could not give any definition of it, and when I showed him one by one the purple skeins, none of which he had selected, and said these were purples, he at once accepted them as such, and pronounced the yellow and orange to be yellows.

This man was very nervous during his examination, and especially at first, but such a condition of discomposure probably favours the manifestation of a latent weakness, and does not lessen the interest of his case. The firm grasp he retained of the yellow skein, his long glances at it before he selected other colours, his apparent contentment with its place in the same bundle with red-purple and purple-brown, and yet his manifest perplexity as to all his colours being the

same, and his dead stop and refusal to name them, coupled with his ready reception of a colour the opposite of yellow as purple, are exactly the features of colour-blindness as I have constantly seen it in those who confound red with green.¹

The other artillery soldier, on being shown a slate-coloured paper, pronounced it to be yellow, but on this name being challenged, described the colour correctly. On asking him why he had previously named it differently, he replied, that on first looking at it it had appeared to him yellow. This perhaps was simply a case, where a fixed gaze on one colour brought up its complementary colour, according to the well-known law of such alternations. But even regarded as such, it is curious, for the man had done no more than glance at the paper when he named it, whereas a normal eye must look long and steadily at a colour to bring into view its complementary; and, moreover, in this case the complementary colour *alone* was seen without a *previous* perception of its primary.

Altogether, therefore, both of the cases detailed are probably entitled to rank as examples of a slight or incipient manifestation of a rare variety of colour-blindness, of which more marked cases will be found if sought for.

The confusion of red with green, which first largely attracted attention to colour-blindness, extends, though not equally, to all the shades of both colours; and each is also liable to confusion with other colours, so that the great majority of cases of chromato-pseudopsis group themselves under red or green, or both. I shall first consider the confusion of these colours with each other.

Red, without any tinge of yellow on the one hand, or of blue on the other, is for most persons rather an ideal than a real colour, and there is a very marked difference in the sensitiveness of eyes reputed normal, as to the distinction between red and crimson, red and scarlet, and crimson and scarlet:² but on this point it is needless to

¹ Above 700 soldiers in the Edinburgh garrison were examined as to the quality of their vision of colours, but as they were purposely left in ignorance of the object of the examination, many of them supposed that Mr Dun, who assisted me, and myself were government or military inspectors, and that dismissal from her Majesty's service might follow the discovery of any defect in their eyesight. We did our best to set the men at their ease, but several of them were a good deal discomposed, and a few were somewhat sulky. The particulars of this examination are given in another part of the paper.

² This is curiously exemplified in the apparent total inability of medical men to determine whether arterial blood is scarlet or crimson. The poets and painters appear to have unanimously decided that it is crimson; whilst the Spanish grandee prides himself on his blood being blue. In reality, blood shows all these colours. Seen through a vein it appears blue; drawn from it, it appears purple; as it undergoes oxygenation it becomes crimson and then red. When coagulation commences, the yellow colour of the serum shades the red into scarlet, and when allowed to dry up, blood becomes brown. We should avoid a needless difficulty, if without affecting too great precision, we styled living arterial blood, neither crimson nor scarlet, but simply red.

dwell at length, for all those colours are confounded with green. I wish, however, to notice, that as crimson by the addition of blue, passes insensibly into red-purple; and scarlet by the addition of red into orange, it is to be understood, unless otherwise stated, that by crimson and scarlet are signified always the redder, and generally the reddest shades of these.

No term includes a greater number of different shades of colour than GREEN, chiefly, no doubt, because nature presents to us on every side so great a mass of green, and so many tints of it. I cannot, accordingly, decide what colour exactly is signified by those subjects of colour-blindness, who have communicated to me their cases in writing; but, in general, I intend by green, a mixture of yellow and blue, in which a normal eye sees no excess of either. Very great differences occur in the judgment of different persons as to the preponderance of one or other of its components in a green; and it would be convenient to have a standard shade for this abundant and important colour, such, for example, as the green which is complementary to pure red, *i.e.*, a compound of all the blue and yellow in the solar spectrum. But very few are familiar with this green, and I must leave each normal-eyed reader to choose for himself a shade of this colour free from the least bluish or yellowish tinge.

It is scarcely necessary to particularize the cases on record in proof of the insensibility of colour-blind eyes to the difference between red and green by daylight. Dr K. and Dr Y. confound full red, scarlet, and crimson, with various shades of equally full and bright green. The former gentleman, as well as Mr T. and Admiral —, have independently referred to a red or scarlet cloak, coat, or jacket, as undistinguishable by them, from a hedge or the leaves of a tree. Lord V. thought a lady's green dress scarlet. Mr N.'s brother picked up a red-hot coal as "a green thing." Lady D., Dr E., and Mr Hughes, cannot tell the scarlet berries of the mountain ash from its leaves. Dr Bryce could not distinguish a scarlet geranium from its foliage. Dugald Stewart made the same mistake with cherries. Dalton held red sealing-wax to match with grass in colour. The tailor's foreman referred to, proposed to sew green tapes to a scarlet waistcoat. Dr Y. could not see a difference between the red and green railway signal glasses. And in addition to the cases described in the previous portions of this paper by me, I found in the Edinburgh garrison eleven soldiers, who put side by side as of the same colour, full scarlet and crimson wools, along with full and pale greens, and who identified red with green glass.

A question here arises, much more easily asked than answered—Do colour-blind persons of the class described, see red, but not green; or green, but not red; or do they see both, or neither? This question, or series of questions, will be variously answered, as, so far, it has been by different authorities, and probably demands a different answer in different cases. My own strong conviction, which is at variance with the opinion of some distinguished writers

on optics, is, that red and green are *both* visible in favourable circumstances to the majority of the subjects of chromato-pseudopsis.

I am led to this conclusion by two considerations:—1. All the colour-blind persons whose vision I have formally tested, could in favourable circumstances occasionally distinguish red from green; and although always uncertain as to the difference between them, and easily shaken in their conclusion as to which was which, yet practically acted on their judgments, which were not always wrong.

2. I have stated, in reporting Mr Hughes' case (p. 391), that, "When asked to select, from a number of pieces of coloured glass, all the specimens which were red, the majority were rightly chosen, but two or three were green; and in the same way, when assorting greens, he placed a few reds among them." Struck by this fact, I watched narrowly the deportment of the colour-blind soldiers in the Edinburgh garrison, when asked to select reds from greens, and with great uniformity they acted thus:—From a heap of coloured wools, each was asked to select first the red skeins, and then the green, no notice being taken of the selection of individual skeins till eight or nine had been set aside as red, and as many as green. In all cases the majority of skeins were rightly chosen,—five or six, for example, in the so-called red bundle, were red, and two or three green; and *vice versa* in the so-called green bundle. It is impossible, I think, to avoid the conclusion that, to those who acted thus, the sensation of red, when felt in its full intensity, is distinct from the sensation of green; yet so slight was the difference to their eyes, that they would not unfrequently, on looking at the two bundles, transfer a red skein, as wrongly placed, from the red bundle to the green, or the opposite; and in no case, even when informed that certain of the skeins were in the wrong heap, did they succeed in a just assortment of them. Prolonged contemplation of the colours, indeed, seemed in all cases to make matters worse; and, in general, I have found that the colour-blind, when informed that they have made a mistake in selecting hues, become increasingly uncertain concerning them, and give up for the time attempting to distinguish between them.

It thus appears that red and green are *both* occasionally perceived in daylight by those who more commonly are blind to them, and that the majority of the colour-blind cannot be defined as possessors of "Dichromic Vision," in the strict sense of that term, although it is quite true that they are only *certain* regarding the two primary colours, blue and yellow.

In what has been stated, red and green are referred to as seen by sunlight; but it is a remarkable fact, which has not hitherto been sufficiently regarded, that, by gaslight or candlelight, the distinction between red and green, which to the colour-blind was so slight by daylight, becomes in many cases quite apparent. My attention was first directed to this matter by the statement of Mr N. (p. 496), that by

candlelight crimson flowers, such as fuchsias, which by day had been lost among the leaves, stood out in contrast with the foliage, so that it was "*then* quite an enjoyment to look" at them.

On reading this, I recalled the account given of his colour-blindness by Mr D. (p. 336), that "the flowers of a scarlet geranium I cannot see distinctly by daylight; but by candlelight there is a marked contrast between them and the leaves."

Without any knowledge of Mr N.'s statement, Prof. Y. repeated (p. 499) his experience in nearly similar words, adding that he "always delighted in going into a conservatory by candlelight, because all the purple and red flowers stood out in such brilliant contrast to the green leaves;" and he further mentions that scarlet flowers then appear brighter than by daylight. Mr R., who professed the account of his case, stated that,—"*Sometimes I can see some reds and greens by lamplight*" (p. 494), and in illustration of this mentioned the very striking fact, that a baize curtain which he bought as green by daylight, and which was then agreeable to his eyes, proved excessively painful by lamplight, and turned out to be "*a very bright red.*"

Dr E., in answer to queries, reported his experience to be exactly similar. Dr Y. whom, by daylight, I had found to make the greatest mistakes between red and green, by gaslight made far fewer blunders (p. 493).

Lastly, Mr S. reports one of the colour-blind persons known to him as being in the practice of resorting to a room lighted with gas or candles, when he wished to tell the difference between scarlet and green, and crimson and blue. (P. 502.)

(To be continued.)

Part Second.

REVIEWS.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By JAMES PAGET, F.R.S., lately Professor of Anatomy and Surgery to the College, etc. etc. 2 vols. 8vo. London, 1853.

DURING the period that Mr Paget held the Professorship of Anatomy and Surgery of the Royal College of Surgeons of England, he delivered an annual course of six or eight lectures to its fellows and members, in order to illustrate that portion of their museum which comprehends general pathology. These lectures extended and rendered more complete by the introduction of numerous facts and quotations, which time, or their inaptness for oral delivery then obliged

him to omit, are contained in the present volumes. Why they should be called on *Surgical Pathology* is not stated, nor, indeed, is it easy to find a reason why pathology any more than physiology, should be divided into medical, surgical, and obstetrical, unless their being taught by persons who practice different departments of the profession, be deemed a satisfactory one. It frequently happens that these subjects, such as healthy and morbid nutrition, inflammation, mortification, specific diseases, cancerous and tubercular formations, etc., are taught by physicians, yet it would be as injudicious in them to call these topics medical pathology, as it is in Mr Paget to denominate them surgical pathology. We trust our readers, therefore, will not be misled by the title of these volumes. They will be as useful to the physician and obstetrician as to the surgeon. They bear no especial reference to the art of surgery, but rather to that science on which every branch of the profession ought to be grounded.

With regard to the work itself, it is one of comprehensive aim, executed with a complete knowledge of the present state of pathology, together with a thorough acquaintance of the whole range of ancient and modern literature, domestic and foreign. It aspires not only to communicate and to generalise upon what is known, but to advance the boundaries of science by the acquisition of new facts, by the results of independent and original research, and by the careful, candid, and earnest endeavour of a mind qualified to investigate and able to interpret. Such a work ought, and we have no doubt will, exercise a powerful influence on the progress of pathology, and consequently on the practice of medicine and surgery. It should therefore be carefully studied by all our readers who desire to see the profession connected with a sound philosophy, and to exercise their art on a scientific basis.

Having said so much, we might here pause, satisfied that whatever objections might be made to this or that portion of Mr Paget's writings by the individual reader, his general opinion would coincide with our own. But it is exactly on account of its great merit as an exponent of facts, that we feel indisposed to allow that all the doctrines it contains should be considered as proven. On the contrary, many are far from being established, and several are in our opinion faulty. It is the more important to remember this, and to be somewhat guarded in admitting all the views of the author, because, we have no doubt, the work will justly be received as one of high authority. We shall endeavour to illustrate this by discussing with Mr Paget one or two points in which we differ, leaving our readers to judge whether he or ourselves be in error.

Most writers on inflammation have expressed the same idea as Mr Travers, when he observed "that a knowledge of the phenomena of inflammation, the laws by which it is governed in its course, and the relations which its several processes bear to each other, is the key-stone to medical and surgical science." To the truth of this statement we emphatically subscribe, and hence we deem it a duty

carefully to scrutinize all that relates to this morbid process, on the comprehension of which so much of sound theory and good practice depends. What inflammation really is we have great difficulty in gathering from Mr Paget's observations. A definition of it he purposely avoids, because he thinks we are not yet in a position to give one. He says :—

“ Just definitions cannot be made in any science till some of its broad and very sure principles have been established. Such principles we cannot boast to have yet attained in the study of pathology ; and the attempts at precise definitions that have been made hitherto, seem to have led to confusion, or to false and narrow views of truth. Besides, to define inflammation is the less necessary, because, practically, we all know sufficiently well what the term implies : we know the signs of the presence of the disease in all its chief forms ; and, when we watch these signs in any external part, we see them so often followed by peculiar changes in the part, that we are justified in recognizing the changes as effects of inflammation, and in believing that, wherever we find them, the similar or corresponding signs of inflammation have preceded them.”—P. 292.

This passage will certainly not bear criticism. It assumes that science is not yet sufficiently advanced to enable us to define inflammation, an assumption which, as we shall subsequently endeavour to show, is erroneous. Then, it is argued, a definition is unnecessary, because practically we know what the term implies ; but if science is incapable of defining it, practically much unanimity as to its meaning cannot and does not exist ? Lastly, it asserts that because external inflammations are so often followed by peculiar changes of the part, that therefore wherever we find these latter, we are warranted in believing that similar signs of inflammation preceded them. But is this true—do the signs of inflammation, (pain, heat, redness, and swelling), invariably precede internal, as they do external, inflammations ? Are there no such diseases as latent pneumonias, without these signs, and true inflammations leading to suppuration of the liver, kidney, spleen, or brain, in which they have been all absent ? Every physician can point to such cases, and hence the danger of the surgeon generalising on a morbid process, which is alike common to external and internal parts, from an observation of the latter alone. It is this surgical idea of inflammation which has hitherto reigned in pathology, an idea which, instead of looking to its essence, regards its occasional symptoms, and because the cardinal signs are very commonly present in external lesions, has led to the most deplorable mistakes in the detection of them when internal, and when such signs are absent.

Now, it has long been maintained in the Edinburgh school of medicine, and taught from the chair of the Institutes, that inflammation is the exudation of the liquor sanguinis. This is at once the definition and the essential phenomenon of the morbid process. Those changes which precede it, if they stop short without producing it, do not constitute inflammation, while those which follow it are the true results of the process. Without directly alluding

to the labours and investigations of those who maintain this doctrine, although fully aware of their nature, Mr Paget observes of it :—

“ We may, indeed, say that stagnation of blood, or effusion of liquor sanguinis, or some exudation, or some degenerative change in the elements of the affected tissue, shall be the condition *sine quâ non* of inflammation ; we may call whatever falls short of these, ‘ active congestion,’ ‘ irritation,’ or by any other name ; but, in reality, such distinctions are often impossible, and sometimes untrue ; and, in study, the terms are convenient for the sake of brevity rather than of clearness.”—P. 293.

Here, again, this paragraph deals more in assertion than in argument. Why are “ such distinctions often impossible ? ” To us they are always capable of being made with the utmost certainty, both in theory and in fact. How are they untrue ? It cannot be denied that such exudations are poured out. When they are so, we have to do with inflammation ; when they are not, we have no inflammation. With great deference to Mr Paget, such a doctrine not only appears consistent with all known facts, and is, therefore, true ; but, while brief, is also clear to the comprehension, which Mr Paget’s notions confessedly are not. For instance, he says :—

“ The several parts of the inflammatory process are—increased fulness of the blood-vessels, with retarded movement of the blood ; swelling ; pain, or other morbid exalted sensation ; increased heat ; exudation of lymph from the blood-vessels ; defective nutriment of the proper elements of the affected part. The first five are often spoken of as the signs of inflammation, the last two as its effects ; but these terms have reference only to the former, being more transitory phenomena than the latter : they are all, when they concur, constituent parts of the disease ; but the latter are less quickly recovered from than the former.

“ It would not be judicious, I think, to refuse to call that process inflammation, in which any one of the conditions just enumerated is absent or unobserved. Swelling, or pain, or, much oftener, increased heat, may be inappreciable in tissues that we may still rightly call inflamed, while the other evidences of the disease are present. The same may be said of increased or altered exudation from the blood-vessels. No such exudation is observed in the diseased cornea or articular cartilages ; but it would be unreasonable, in the case of an inflamed eye, to say that the changes are due to inflammation in every part but the cornea ; and to call the process leading to the ulceration or leucoma of the cornea by a name different from that which we give to the coincident and similarly excited process in the other tissues. So, during the inflammation of a joint, it would be, at the least, inconvenient, to say that all the tissues are inflamed except the softening or ulcerating cartilages. The progressive degeneration of tissue is, probably, never absent when the other parts of the inflammatory process exist ; but, in quickly transitory cases, it is often inappreciable. The altered state of the circulation may be unobserved : but it is, probably, always present ; for in the case of the parts that have no interstitial blood-vessels, inflammation may still be attended by enlargement of those of adjacent parts, on which their ordinary nutrition depends.

“ The conclusion, then, may be, that in what may be regarded as well-marked or typical examples of inflammation, all the characters I have enumerated are present as concurrent parts of the disease ; but that the same name should not be refused to diseases in which any one of these parts is absent or unobserved, especially when its absence may be explained, as in the case of inflamed cartilages, by some peculiarity of tissue or other condition of the disease. I think it would not be right to call any process inflammation

in which there is neither an exudation of lymph (*i. e.* of material capable of such developments or degenerations as I have described), nor a deterioration of the proper tissue of the affected part—even though the other characters of the disease might be present. But, really, whatever rule of nomenclature be adopted, we may expect to meet with many cases in which we shall doubt what name to give to the processes which we watch, or of which we see the results. There is neither here, nor in any other part of pathology, anything like the unity, or circumscription, of species by which the zoologist, whose nomenclature pathologists are prone to imitate, is justified in attaching to each specific name the idea of several constant and unalterable characters in the beings to which it is assigned.”—Pp. 428-430.

Hence, according to Mr Paget's views, nothing definite can be introduced into the pathology of inflammation. All is confusion. But, fortunately, the reason of this is apparent from the above passage. The obstacle to Mr Paget's clearness of perception in this matter consists in the fact that he is confounding morbid processes widely different, and this, as we shall attempt to show, in direct opposition to his own careful observations. To talk of inflammation, of non-vascular textures, such as of cartilage or of the cornea, is, according to the doctrine we are defending, evidently absurd. If exudation be the essential phenomenon of inflammation, then the softenings, abrasions, and ulcerations of the cartilage and cornea are either not inflammatory, or, if they be, they constitute an argument opposed to the correctness of our generalization. The latter is the view of Mr Paget. Now, the following are the changes observed in diseased cartilage, altogether independent of the penetration of lymph, as admirably observed by Dr Redfern, and recorded in the pages of this Journal. We quote from the condensed account of Mr Paget:—

“ These changes consist, essentially, in the enlargement of the cartilage-cells, with increase of the nuclei, or of peculiar corpuscles contained in them, or with fatty degeneration of their contents, and fading, or similar degeneration of their nuclei. The hyaline intercellular substance at the same time splits-up, and softens into a gelatinous and finely molecular and dotted substance, or else is gradually transformed, in the less acute cases, into a more or less fibrous tissue. The enlarged cartilage-cells on the surface are released, and may discharge their contents on the surface of the ulcer; and the intercellular substance is gradually disintegrated and similarly discharged, or, whatever part of it remains, is transformed into fibrous tissue, and becomes the scar by which the ulceration is, in a measure, healed.

“ Lastly, in the cornea, a series of observations on the effects of inflammation, purposely excited in it by various stimuli, have shown that the changes in it are not due to any free exudation of lymph in it, but to alteration in its proper constituent textures. They consist, chiefly, in swelling and enlargement of its corpuscles, the appearance of minute fatty molecules in them, and the increase and enlargement of their nuclei. The intercellular substance becomes, at the same time, turbid, more opaque, denser, more fibrous, and, sometimes, finely granulated; and in some cases fatty molecules appear in it. The changes thus produced in the cornea are not essentially different from those that follow its idiopathic inflammations; and, as Virchow concludes, they are extremely like those of the *arcus senilis*.”—Pp. 415, 416.

Now the processes here described in non-vascular textures, namely, the enlargement and endogenous multiplication of the *normal cells*

of the part, and their fatty degeneration, with fibrous alteration of the intercellular substance, are in no way analogous to the increased fullness of the blood-vessels with retarded movement of blood,—exudation of lymph,—new development of cells,—and defective nutrition of the proper elements of the affected part observed in vascular tissues. This is obviously because there are no blood-vessels in the part, and no blood; there is no exudation; no development of new cells; and, so far from nutrition being defective in the proper elements of the affected part, it is evidently increased, the proper cells of the tissue assuming augmented powers of development. The broad distinctions separating cartilaginous ulcerations and their mode of healing, from inflammatory ulcers, are thus laid down by Dr Redfern:—

“Articular cartilage contains no blood-vessels, consequently ulceration and other changes of structure confined to it are never complicated by inflammatory exudations, pus, etc., and can be examined very satisfactorily; ulceration in tissues containing blood-vessels is almost always complicated with inflammation, and the ejected particles of the tissues having undergone a very complete degeneration, and become mixed with a mass of structures formed in the inflammatory exudation, also degenerate, are, therefore, very difficult to recognise. Ulceration or other disease confined to articular cartilage has in no single instance been shown to be productive of pain, no doubt for the very simple reason, that these structures contain no nerves; in other textures, ulceration is often attended with very severe pain from implication of the nerves ramified in their substance.

“The *healing of ulcers* in articular cartilages, and in tissues freely supplied with blood-vessels, differs in this remarkable particular, that in the former, the fibrous cicatrix is invariably made up of the changed substance of a portion of the cartilage not subjected to the process of ejection, and in the latter, the cicatrix as constantly results from the development of the inflammatory exudation. This difference admits of the following explanation:—In the case of the ulcer affecting no texture but that of cartilage, there is no other matter from which a cicatrix can be produced than the actual cartilage tissue remaining; and it may be remarked, that cartilage is a very simple cellular texture, and that the greater number of the tissues are originally formed from cells,—that the cells of cartilage have a natural tendency to transformation into fibre, as is shown in the development of fibro-cartilage,—and that the inter-cellular substance shows as remarkable a disposition to produce the same result in its diseased state. In the ulceration of tissues supplied freely with blood-vessels the reverse maintains; for the degeneration and destruction of the tissue actually involved is so complete, that this can in no way assist in the formation of the cicatrix; and there is, therefore, nothing left in this instance but the inflammatory exudation from which that substance can be formed.”—*Dr Redfern in MONTHLY JOURNAL*, vol. xiii., pp. 212, 213.

The two processes, then, of inflammatory ulceration, and the ulceration in non-vascular parts, are widely different, and it is the confounding them together which constitutes the difficulty with Mr Paget, a difficulty that is at once got rid of by maintaining what we conceive to be the more correct view, namely, that these forms of anormal nutrition, in non-vascular parts, are not inflammation at all.

But it may be asked, if not inflammatory, what are they? Surely, it is urged by Mr Paget, the abrasions and ulcerations in cartilage and in the cornea are strictly analogous to ulcerations in vascular

parts. To this we say, by no means; the one is dependent, as shown by Dr Redfern, on increased development of the normal cells of the texture; the other is produced as the result of an exudation. Now there is another non-vascular texture where identically the same process is observable, where ulcerations occur, and are, in like manner, dependent on increased development of the normal cells of the part—we allude to the epithelial or epidermic structure. Let any of our readers study the accurate account given by Mr Paget of the structure comprehended in this ulceration, commencing p. 434 of the second volume, and it will be made apparent that we have here to do with a multiplication of epidermic and epithelial cells; that is, of the proper texture of the part. Hence the processes carried on in cartilage on the one hand, and in epithelium on the other, are strictly analogous, and yet Mr Paget labours to prove that the one is inflammation and the other cancer. In our opinion, he has here made a fundamental pathological error, separating lesions strictly allied, and extending the meanings of both inflammation and cancer, in order that they may embrace diseases which ought to be carefully distinguished from both those morbid processes. In consequence, his notions of cancer are as faulty as his views of inflammation. While most of the pathologists in Europe are carefully drawing distinctions between what is and what is not cancer, and showing the importance of their labours by clearing away the discrepancies attached to those much abused words, malignant and benignant, Mr Paget once again endeavours to mix them together under the terms malignant fibrous tumours, myeloid tumours, epithelial cancer, melanoid cancer, osteoid cancer, villous cancer, etc. etc. With great deference, we are of opinion that pathological science will be more benefited by limiting the terms inflammation and cancer to the essential and invariable phenomena of those morbid processes, than by continuing to confound, under new names, all kinds of different lesions together, in order to meet arbitrary notions of symptoms on the one hand, or of what is called malignancy on the other.

The second volume, however, containing the author's views on what he calls innocent and malignant tumours, will constitute the subject of a second notice in our next number.

Asiatic Cholera; its Symptoms, Pathology, and Treatment; with which is Embodied its Morbid Anatomy, General and Minute. Translated from a paper by Drs Reinhardt and Leubeuscher. By RICHARD BARWELL, Fellow of the Royal College of Surgeons, etc., etc. Small 8vo. London. 1853. Pp. 219.

WE have read this book with great satisfaction, and can recommend it to our readers as a very useful contribution to medical literature

at the present juncture. It succinctly reviews what is at present known regarding cholera, and is not deficient in novelty. For instance, it is pointed out that typhus fever generally increases in any country, as cholera approaches, and acquires a greater tendency to abdominal complication. It is therefore advanced hypothetically, that under the influence of the particular poison, typhus fever becomes a virulent remittent, the cold stage of which is algide cholera, and that this form is preceded, and in part induced, by an action causing alvine disturbance. The difference, in reality, between intermittent, remittent, and continued fevers, is very small; so slight, that in the same climate and in the same malaria, a difference of temperature as to season will change the aspect of the disease from one to the other. The relation of algide cholera to intermittent fever is thus further pointed out.

“ In the cold stages of a regular intermittent, ague for instance, the blood leaves the surface of the body, causing contraction and shrinking of the face and hands, and a peculiar rough sensation of the skin; and in cholera, the blood leaves the surface and remains pent up in the internal cavities; the body is cold in both: in time, if all goes well, warmth returns, which soon becomes heat and fever. The difference of this symptom, in fact, is more in degree than kind. The cholera patient often dies before the hot stage comes on, and never lives, as far as my experience goes, through a second cold fit to the next hot; the ague patient survives very many. Thus, though no one could say, with an approach to truth, that cholera is merely a deadly form of ague, yet it appears true, that it is a peculiarly deadly disorder with a decided remittent type, and that the sufferer is apt to die in the cold stage, or before the circulation, disturbed by the great power of this chill, can become fully restored. In all cases of cholera which live beyond the algide stage, a consecutive fever supervenes. In the three cases given above, and in four others at St Thomas's Hospital, the notes of which are now by me, the changes from one to the other, and back again to the cold stage, are well marked.

“ As in the cold stage of ague, the blood remains in the internal parts, so does it in cholera; but apparently a certain disorder, either of the intestines or of the blood, causes a tendency to the secretion of fluid therefrom, the congestion and the stagnation in the larger vessels, during the cold stage, increase this tendency, and the serum of the blood constantly runs off into the intestine.

“ Drs Reinhardt and Leubuscher, who published in 1849, their observations on cholera, give a most detailed and elaborate account of the appearances found in those dying in all stages of the disease. In those who died in the algide stage it appears, that most internal organs suffered from venous congestion, situated, not so much in the venules, as in the large veins of the part. Thus, the lungs and the liver are empty of blood; but all the right side of the heart and the vessels leading to it are gorged therewith, as Dr Parkes also has observed. The mucous membranes generally, but those of the chylo-poietic and of the urinary system in particular, are congested, even to the venules; the left side of the heart and its vessels are comparatively devoid of blood. Indeed, to state the facts broadly and in few words, there was a universal internal congestion, which appeared, as though beginning in obstruction about the right side of the heart, for all in its neighbourhood was full, and the left side of this organ was empty, as though it had, through life, retained power to pump away what little blood entered its cavities. The same condition of surface and the internal congestion, but in a less degree, takes place also in the cold stage of ague, and the comparison between the two diseases is assisted by the morbid anatomy of each.

“ However, as in all fevers of the remittent type, irregularities not unfre-

quently occur, so also in cholera ; for the consecutive fever, or the febrile stage of cholera, which is analogous to the hot fit of an ague, may be very slight and evanescent, in comparison to the intensity of the algide symptoms, or the cold stage may be short and slight, in proportion to the great amount of consecutive fever—there may even be no cold stage, no rice water purging, but simply violent bilious cholera, with vomiting and suppression of urine, followed by so great an amount of febrile disturbance, that, together with the entire disappearance of that secretion, it carries off the patient. Now, it cannot be said, that these irregularities militate against the truth of the hypothesis ; for I contend, that they rather aid in its establishment, showing, as they do, that even the unmistakeable cold stage is not a necessary part of Asiatic cholera ; but only one phase in the disease,—a very fatal one it is true, still, perhaps, not much more so, than the consecutive fever, and it is not so essential a portion as that fever, since no case that lives beyond the cold stage escapes the hot one.” —Pp. 70-74.

Dr Barwell then gives three cases which had no, or a very slight, algide stage, but glided almost at once from the premonitory diarrhoea into consecutive fever. Hence, according to him, cholera is a malarious disease, of an irregular intermittent type, the cold fit being so virulent, as to form the most fatal part of the malady, and to take the most prominent place in all descriptions and all our ideas of the disease. With the algide symptoms is combined, more or less, morbid action of the stomach and bowels, which action appears an essential part of the malady, and is seldom absent.

This being the theory, the practice must be directed so to act on the circulation, that the blood may be attracted to the surface, the great internal vessels unloaded, and the heart enabled to act. Such efforts can only be produced by external remedies, since we possess no internal medicines which act primarily on the circulation. The one recommended is external warmth, by means of the hot wet-sheet. Of this he says :—

“The hot-wet-sheet, one of the most powerful instruments for imparting warmth that can be imagined, was frequently very successful in dispelling the collapse, and was so grateful to the patient, that they always spoke of it with thankful expressions. All the world knows that a sheet dipped in cold water and wrapped round the body soon causes great warmth ; and this is even the case with a cholera patient, although his body may appear to have lost all power of generating heat ; but the *hot* wet-sheet is better, as it brings at once the requisite warmth. The water, out of which the sheet is wrung, should be boiling, and therefore some implement for wringing it without scalding the hands is required. A strong piece of canvass, half a yard broad and rather more than one yard long, has, sewn across at each of its end, a strong staff, which projects at either side beyond the canvass. The sheet is placed in this, in a bucket, and boiling water poured on to saturation, then two persons turn the stick, one one way, one the other, till the canvass, being tightly twisted, squeezes the sheet sufficiently dry. The patient having been stripped, is now wrapped in this, laid on a bed or mattress, and covered closely with five or six thick blankets, and in this he is kept for one hour.

“One of the most marked effects of the hot-sheet is the disappearance of cramp. Patients will come suffering from these in a very distressful manner, and after having been in the sheet for ten minutes or a quarter of an hour, will be perfectly relieved ; the body, which before was cold, will often come out glowing and red ; the tongue, too, is often warmed. Now, during the application of this remedy, the face and tongue are the only means of judging if it be

as beneficial, as can be wished, since the hands and wrists are enclosed; the cheeks will sometimes be found to become warm in about a quarter of an hour, and the face loses its choleraic aspect in about half an hour; if these changes take place, and the tongue also become warm, we may be tolerably confident that the patient will live through the stage of collapse, and that time will be allowed for the application of other remedies,—always a great point gained. Another occasional consequence of this external warmth is the checking of the rice-water purging; true, I have never tried this remedy alone to the exclusion of internal ones, such as brandy and wine, and therefore it cannot be directly proved, that the wet sheet, and not the other means, stopped the intestinal action; but, as many patients have, after the application, never had another rice-water dejection, although before this, they occurred every ten or twenty minutes, and as in this time, he had only taken one dose of medicine, wine, or brandy, it seems fair to conclude that the external warmth, whose action is immediate, was the cause of this sudden check to the morbid action of the intestines, particularly as the same internal means had before been so eminently unsuccessful, while warmth to the surface was neglected. One of the worst cases of collapse I ever saw, with constant purging, was also a case of the most rapid recovery, and the patient never had a drop of medicine of any description while in the hospital. The patient seemed afterwards to look upon that hour on the sheet with a mystic veneration, as though there were something magical in the whole performance.”—Pp. 111-114.

The purging should be checked by the means appropriate for choleraic diarrhoea, by mustard poultices on the abdomen, starch and opium enemata of very small bulk, but when it continues, and is accompanied by vomiting and thirst, he recommends that large quantities of water should not be allowed:—

“Everything he drinks should be cold, iced, and in the smallest quantities at a time. Two table-spoonfuls of iced water at a time, is enough for ten minutes, and then another such quantity, or rather a fragment of clear ice, about as big as a hazel-nut, may be given, and in two minutes more another such piece; when the patient, as he soon will do, finds the value of pursuing this plan, that thereby he is not made sick, and that his thirst is much better relieved, he will be content to continue the same method of his own accord. Thus he may have a teaspoonful of brandy, and half a wine-glassful of soda-water every five or ten minutes, or, which is sometimes useful, ten minims of laudanum in the same quantity of soda-water, either with or without brandy, as best may suit the stomach. Hydrocyanic acid is not generally of much benefit in this form of vomiting; but still should be tried with small quantities of soda-water; and creasote generally fails. Combined with the plan of management, however, a pill of four grains of calomel, or of the chalk and mercury, with one grain of opium, often appears beneficial; but, after all, the sheet anchor is the internal cold, the small, but frequently renewed blocks of ice, and the very small bulk of all ingesta. The great benefit derived from internal cold in this complaint, is, indeed, what we should expect, if we admit that it is this congested condition of the internal parts which is, in a great measure, the originator of the stomacho-intestinal disturbance, when ice, applied as directly as possible to the viscera, would be just the best adapted remedy for that particular state.

“There is, however, a mode of using the mineral acids, which often has a marked effect in checking both the purging and the vomiting, whether chiefly by their constringent or by their antiseptic qualities cannot be determined, though both have probably their share in the result. This acid is a mixture of the strong nitric and hydrochloric acids, three drachms of the former to five of the latter. On mixing, a quantity of chlorine is set free, and detained in the fluid, and it may be partially to this chlorine that the medicine owes its

efficacy. The dose is eight minims, and it is given with one drachm of syrup of poppies, and with one ounce of water. This medicine is to be administered as soon as the patient is wrapped in the wet sheet, and continued every hour. It often has the effect, combined with the management above described, of checking the vomiting and purging, and even where the absence of algide symptoms rendered the use of the hot sheet unnecessary, this acid was often found to be thus beneficial, and was often given to the exclusion of all other medicines whatever. Great dependence, therefore, may be placed on this combination for subduing the vomiting, if it be administered early enough in the disease, and it has the same effect upon the purging, five doses being sometimes sufficient to put an entire stop to both symptoms. When all these remedies, and the management of the iced and cold drinks fail, as they sometimes will, the vomiting runs on to another, and even more distressing, stage, which will be hereafter shortly described. One circumstance should not, however, go unnoticed, viz., the desirability, when opium in the solid form is to be administered, of giving, not the soap and opium, but simply crude opium, since the soap certainly appears to induce vomiting in this particularly irritable state of the stomach. And here it may be advisable to say a few words with regard to the use and abuse of opium in this disease. As a remedy for choleraic diarrhoea this medicine, combined with stimulating astringents, is, as has been said, extremely useful, when given in moderation; but it is vain, and indeed injurious, to attempt to cut short the disease entirely by its means; there is, in the collapsed state, and in the subsequent stages of cholera, a tendency to apathy and to heavy sleep, almost amounting to stupor, which this remedy only increases, and it, moreover, when given in excess, masks the nervous symptoms and perplexes the treatment. In the collapsed stage of cholera, while the patient is actually cold, almost pulseless, and when one sound of the heart is already lost, the medicine can only be hurtful; and, as far as my experience goes, does not subdue cramp, even when given in quantities sufficient to produce most decided and injurious effects on the nervous system. One patient, a captain of a small trader, had taken, I believe, from the medicine chest of his vessel, so large a quantity of opium, that it was impossible to say whether he died of the remedy or the disease, and yet, for the short time during which he lived under our observation, cramps in the calf of the legs were continual, the only sign of suffering being referable to that cause.

"After the collapse has entirely subsided, a small dose of opium, in the solid form and crude, has *sometimes*, however, a beneficial effect in checking the remaining disposition to vomit and to purging, and in subduing a restless state of the patient, which is occasionally present, and during which he often rolls himself about, throws off the bedclothes, and exposes his body to the cold air, which has, as before said, a most injurious effect. Laudanum is also useful in the same way, but more so as a remedy for purging than for vomiting, since a pill is often retained and allowed to act when fluids are rejected; it should be given in a small quantity of soda-water, if there be vomiting, and if, as is sometimes the case, it excite this action, either solid opium may be substituted, or another remedy tried. The drug, however, can only be recommended in a few instances, when the collapse, passing away, leaves the patient fretful and restless, but without the pulse becoming hard, or the skin hot, and when there is no fulness of head."—Pp. 119-24.

Should the patient struggle through the algide stage, then reaction occurs and the febrile stage commences. If he pass urine within thirty-six hours after the disappearance of the algide symptoms, recovery from the consecutive fever is, according to the experience of the author, shortly established. But if the secretion be absent for three days, recovery will be longer; and if four days go by, there is great reason to dread a fatal termination. Indeed no patient can

be considered safe until he have passed urine. Again, if within the sixth day after the entire disappearance of collapse, the febrile symptoms do not begin somewhat to abate, or if the oppression and lowness of spirits become greater and the pulse weaker, the probability is that the patient sinks into a far worse state by gradual and slow degrees; this constitutes, according to the author, choleraic typhus, and closely resembles ordinary typhus with brain congestion, and its treatment is the same. Wines, light nourishment, leeches to the head, in to the shaven scalp, effervescing saline draughts, and small doses of chloride of potass.

The chapter on the morbid anatomy of cholera, translated from the paper of Messrs Reinhardt and Leubeuscher, is the best account of it yet published.

We shall not venture to criticise either the theory or the practice of our author. The former must be allowed to be plausible, and the latter as good as any yet brought forward. Experience alone can decide on its merits. Hence we have contented ourselves with giving an analysis of this, the most recent book on cholera, and have allowed Dr Barwell to speak, to a considerable extent, for himself.

Reminiscences of a Medical Life, with Cases and Practical Illustrations. By JONATHAN TOOGOOD, L.R.C.P.L. Taunton, 1853.

It is a common belief that medical men write books at one or other of two epochs in their history. The young doctor writes from the fulness of his faith and love of medicine, and in the fulness of his leisure. The old doctor takes up the pen under similar motives, but his faith in medicine is purified from much dross, and is fortified by having had many storms and struggles to encounter; his leisure also is the *otium cum dignitate*. The book before us is by an old and accomplished practitioner, whose enthusiasm for his art has certainly never been surpassed, and whose interest in practice has in no way been abated by increasing years. It should be read by professional men as the history of a long journey, presenting numerous incidents, marvellous or instructive, with general reflections by the pilgrim during his progress.

Our author, in introducing his reminiscences to us very properly says, that he is not about to repine—quite the reverse. He has had his share of hardships and trials, involving the sacrifice sometimes of time, health, and convenience; but he gratefully adds,

“I am thankful to enjoy good health, after forty years most laborious country practice, and gratefully acknowledge that my profession has enabled me to educate, and establish in respectable positions in life, eight sons, and that it has introduced me to a large circle of acquaintances, and secured many valued friends.”—P. vi.

Dr Johnson characterises the work of our profession as a “me-

lancholy attendance on misery, a mean submission to peevishness, and a constant interruption to pleasure." Our author and fellow-traveller, in looking back on his long journey of life, devoted to the alleviation of the sufferings of his fellow-creatures, derides this churlish sentiment, and exclaims truly that

"It is accompanied with higher feelings of satisfaction, and a more enviable reward than the acquisition of wealth."—P. v.

So convinced is our author, not only of the advantages of our profession on a retrospective view, but also of the immediate and present benefits accruing from actual practice, that he has chosen the following words wherewith to close his book.

"From general and personal experience, I would recommend my professional brethren never to retire from their active duties whilst their mental faculties and physical strength remain unimpaired, from a firm conviction that those who adopt such a determination will best consult their own health and happiness."—P. 176.

We cannot enter upon the valuable and interesting collection of cases our author has met with during his life, or stop to admire all the notable men of this generation and the last, with whom he has conversed in his long journey. But we recommend this work to our readers not merely for the medical experience it records, but also for the moral qualities of the author which it exhibits, and which must commend themselves to all.

Practical Pharmaceutical Chemistry: An Explanation of Chemical and Pharmaceutical Processes, with the Methods of Testing the Purity of the Preparations. By Dr G. C. WITTSTEIN. Translated from the second German edition by Stephen Darby, Practical Pharmaceutist. 12mo. Churchill, London. 1853.

DR WITTSTEIN'S *Darstellung und Prüfung Chemischer und Pharmaceutischer Präparate*, of which Mr Darby has here given us a translation, is not a systematic work on Pharmacy, but a series of papers on individual processes, embodying the results of his own manipulations, and originally contributed to Büchner's Repertorium. As detailing the experience of a well known chemical operator, these essays will prove of interest to those who may be engaged in preparing any of the substances here treated of. A considerable number of the processes, however, refer to substances having a purely scientific interest, and the methods of preparing these are generally well known to scientific chemists, who alone are likely to occupy themselves with them. Of the more strictly pharmaceutical processes, the greater number are either introduced into our pharmacopœias or are practically known to pharmacutists, and we therefore fear that this book will not prove to be of very extended utility in

this country. In some respects, however, especially as regards the amount of product to be expected, it will be found useful as a book of occasional reference.

So far as we can judge from a comparison with the first German edition, Mr Darby's translation is tolerably faithful to the original. It cannot, however, be said to have any pretensions to elegance; and in point of punctuation it is in very many instances so deficient that a sentence has to be read over more than once, before its meaning can be deciphered. We are utterly at a loss to imagine for what reason, either technical or classical, Mr Darby should have thought fit to retain as the running titles of his translation the hideous German-Latin names of the original. It seems a very odd taste in one who was writing for the benefit of Englishmen to prefer such names as *Kalium Oxidatum Bicarmonicum*, *Kalium Oxidatum Tartaricum*, and so forth, when he has upon his own pages the more common, and certainly not less elegant, names of *Potassæ Bicarbonas*, *Potassæ Tartras*, etc.

On Diseases of the Rectum. By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, etc. 8vo. Edinburgh. 1854. Pp. 133. Third Edition.

WE deem it unnecessary to enter at any length upon the consideration of a work so well known as this treatise of Mr Syme's on diseases of the rectum. To it surgeons are indebted for correcting the improper method of operating for fistula in ano, practised sixteen years ago, when the first edition was published. The author then drew attention to the observation of M. Ribes respecting the position of the internal orifice in that disease—the necessity of including it in the incision—and the inexpediency of cutting beyond this point. He also combated the error which referred the origin of fistula to ulceration of the mucous lining of the bowel; and endeavoured to show that, when the operation was performed upon correct principles, it was a very trifling one, and rarely if ever proved ineffectual. Internal hemorrhoids were shown to be capable of being treated safely by the ligature, instead of the uncertain methods by caustics, acids, red hot irons, and the knife. The mischievous idea of considering hemorrhage from such a source to be beneficial was forcibly pointed out, and its relation to prolapsus clearly explained. The polypi, strictures, and fissures of the anus were also not only described, and their pathology made out, but a simple surgical treatment was established for their successful cure.

Whatever advantage may result from systematic works, it is to the monograph we must look as the chief means of furthering the

practice of medicine or surgery, and we would point to the one now under consideration in confirmation of this statement. Terse in expression—sound in doctrine—practical in character—and apposite in illustration.

Part Fourth.

PERISCOPE.

ANATOMY AND PHYSIOLOGY.

ON THE COLOUR OF HAIR. BY DR ALLEN DALZELL.

THE colour of the hair, which, according to Griffith, was long attributed to pigment accumulated in the cells of the medulla, depends upon one or more of three causes. First, on pigment granules; second, on diffused colouring matter impregnating the entire tissue; and third, on the presence of air spaces within the fibres of the shaft. To these might be added the nuclei of the cells themselves, which, however, where pigment granules are present, are so surrounded by them as to be scarcely, if at all, discernible. But where their isolation has been effected by boiling with moderately dilute caustic potash, they are shown as dark bodies of an elongated form.

The colour of the hair corresponds in intensity to that of the iris; as, for example, auburn with blue, and black with the darker tints. Nor are these relations at all confined to the human species, although especially remarkable in the Albino, whose choroid is destitute of pigment, and hair either very pale or entirely white.

Many observers have described the granular pigment which forms the first class of colouring matter, as if it was situated in interspaces of the fibres. I have, however, assured myself of the fact, that pigment is never lodged exteriorly in the cells, but always in some part of the interior, as may be plainly seen in the hairs of some *cervi*, where the entire cells are dry and empty, except of traces of colouring matter which adhere to their walls. Changes, during the growth of hair, often take place at regular intervals in the colour and amount of these deposits. This is seen in the hairs of many of the *Quadruman*a and *Carnivora*, to which classes it is, however, by no means confined.

In many hairs, the colour is uniform or diffused. Most animals have hairs of this kind; good examples may, however, be found in the short hairs from the face of the hair, in the tapir, and yellow bear.

Air spaces in the Shaft.—These cavities, from containing air, refract light beyond the field of the microscope, and thus, like the cells of the axis, give the idea of colour; these are best seen in white hairs. Some authors have described them as fat granules. This is inaccurate, for, on boiling with ether or turpentine, they become filled with the fluid; and even when treated in menstruum, which does not dissolve fat, they lose their refractive properties, and retain only their general outline. They are empty cavities situated in the cells of the shaft, produced, as Kölliker supposes, by the absorption of its granular pigment; for they are not found in any hair originally colourless, but only in such as have become so from some cause affecting their vitality. I examined a hair with one extremity entirely white, the other unaltered—the former part I found filled with air cells, the latter pigment cells.—*Edinburgh Phil. Journal.*

VIRCHOW. DISCOVERY OF A SUBSTANCE PRESENTING THE CHEMICAL REACTIONS OF VEGETABLE CELLULOSE IN THE HUMAN NERVOUS CENTRES.

In the human brain, Purkinje first described peculiar corpuscles formed of concentric layers, and analogous in structure to starch granules. Their origin and use were unknown. By microchemical examination, M. Virchow has made the curious discovery, that these "corpuscula amylacea" present the reactions of vegetable cellulose. When they are treated with an aqueous solution of iodine, a light bluish tint is produced, which contrasts strongly with the yellow colour of the surrounding parts; and when hydrated sulphuric acid is added, the corpuscles present the bright violet colour which forms the specific character of vegetable cellulose. The constancy of this reaction has been confirmed by repeated investigations. The corpuscles of cellulose, however, or true corpuscula amylacea, belong to the ependyma, and are found only in the superficial layers of the cerebral ventricles, in the spinal marrow, particularly the central grey substance, corresponding to the ependyma of the obliterated central canal, and in the nerves of the senses, as in the grey substance of the olfactory nerve. All other concentric corpuscles, from the pineal gland, choroid plexuses, granulations of pacchioni, or the (calcareous) plates of the spinal arachnoid, show no vegetable reaction. The discovery of cellulose in this situation has great interest in connection with the production of sugar in the body from lesion of the nervous centres, as shown in M. Bernard's experiments. M. Virchow, however, has not found cellulose in the rabbits.—*Acad. des Sciences*, Sept. 26.

Since the discovery of this substance in the human brain and spinal cord, M. Virchow had sought for it in most of the healthy and morbid tissues of the human body without success. He has, however, at length recognised it in that peculiar affection of the human spleen which consists in a kind of colloid degeneration of the Malpighian bodies, and is usually designated waxy spleen. (Wachsmiltz.) In this lesion the Malpighian bodies are transformed from the periphery to the centre into a homogeneous mass of a greyish or yellowish colour, presenting the form of grains like those of boiled sago. These grains are composed of microscopic corpuscles somewhat irregular, but quite homogeneous, which may be considered to result from the transformation of the cell contents of the splenic follicles. Treated by chemical reagents, these corpuscles are rendered pale by acetic acid, and on the addition of a little ferrocyanate of potass to the acidulated preparation, a granular precipitate is formed in the interstices of the corpuscles. Hot nitric acid produces a yellow colour, which becomes brownish on the addition of caustic ammonia from the formation of xanthoproteic acid. But in particular by the action of iodine and sulphuric acid, the bright violaceous colour of cellulose is struck with surprising promptitude, showing the similarity in composition of these waxy corpuscles with the corpora amylacea of the nervous centres. The exactness of the reaction was verified on specimens preserved in spirits. M. Virchow adds that this degeneration of the spleen is principally found in states of cachexia, and mostly in patients who have been the subjects of protracted ulcerous affections.—*Acad. des Sciences*, Dec. 5; *Gas. Med.*, Dec. 17, 1853.

VIRCHOW ON THE EXCITABILITY OF CILIA.

All known contractile substances may be excited to action by means of stimulation with mechanical, physical, and chemical agents. Valentin and Purkinje, however, discovered in cilia an exception to this rule; for they found that the only way in which their motions, when becoming weak, could be roused to renewed activity, was by means of *mechanical* irritation. The truth of this observation was doubted by Professor Sharpey; and Virchow has lately been experimenting on cilia to ascertain its accuracy. He has discovered by chance that ciliary motion is capable of being excited by two chemical agents. On adding a solution of caustic potash to a portion of epithelium from the human trachea,

in which the ciliary action—originally weak—had nearly ceased, he found that the motions of the cilia became very animated, and continued so until their tissue was destroyed by chemical corrosion. He has frequently repeated this experiment, and always with the same result. Portions of epithelium which he immersed in water till the motions of the cilia had ceased, and their structure had begun to be impaired,—recovered, by the application of the potash, all their normal phenomena. The alkaline solution needs to be very slowly and cautiously applied. When this is done, at first, a few cilia only begin to move, with an irregular jerking action; by and bye others follow, moving indeterminately and confusedly; finally, all are in full action, with gradually increasing force, moving harmoniously together in one direction, with the quick, rhythmic, lashing action peculiar to these bodies.

If, on the other hand, the alkali be applied too suddenly, or too strong, it will merely cause a brief, convulsive movement of the cilia, followed immediately by the destruction of their substance.

Virchow considers these effects to be induced by the chemical action, and not by the corrosive power of the alkali.

Caustic soda, when applied to cilia, acts precisely as the potash solution. Ammonia, on the other hand, occasions their immediate chemical decomposition. Virchow has found no other substance which acts in the same manner as these two bodies; and, considering the vast number of chemical substances tried in vain by Purkinje and Valentin, he entertains little hope of discovering any.

He considers that the excitability of cilia—demonstrated by his experiments—proves their substance to be analogous to the contractile structure of muscle.—*Virchow's Archiv. für Pathol. Anatom. and Physiol.* Bd. vi. hft. i.

VOGEL ON THE COLOUR OF URINE.

I imagine, says the author, that the colouring matter of the urine is formed by the colouring matter of the blood, and that it consists of a decomposition of blood discs. If this be the case, and it be premised that the decomposed blood-discs do not leave the body through any other channel than the urinary, it follows that we possess, in the colour of the urine, a ready means of ascertaining, in a given time, whether a large or a small quantity of blood-discs are decomposed in the animal economy. In the normal condition two to six parts of colouring-matter are voided daily, and this expresses the normal loss and reproduction of blood-discs. A chlorotic patient, or a convalescent, who voids but little colouring-matter in the urine, would have but little change and reproduction of blood-discs. Every one whose urine is high-coloured would require a constant supply of red blood corpuscles. The grounds upon which this hypothesis rests are the following:—

1. In all cases of brown or brownish-black urine, where the urine is most coloured, the direct source of the colouring matter—namely, from decomposed blood-discs, can be easily traced.

2. In all cases where there is a constant discharge of red urine with increase of the colouring matter, as in fevers, inflammation, etc., there we see, as a common result, diminution in the quantity of the blood-corpuscles, and a corresponding condition of anæmia.

3. In cases where there is noticed diminution in the production of red blood-corpuscles, with probability of their very sparing disintegration, as in many (not all) cases of anæmia and chlorosis, there do we find diminution in the excretions of the colouring-matter of the urine.—*Vogel und Nasse' Archiv.*

M. TROUSSEAU ON THE EVOLUTION OF THE TEETH IN INFANTS.

M. Trousseau affirms, that the period when the milk-teeth first appear through the gum is about the age of seven months and a half; and that they pursue the following order:—1. The two inferior median incisors; 2. The four superior incisors; 3. The inferior lateral incisors, and the first four molars; 4.

The four canine ; 5. The last four molars. M. Trousseau persists in affirming that the canines appear after the first molars. On this point he is undoubtedly correct. The order of appearance of the teeth is governed by the wants of the animal throughout the mammalian class. The soft food first given to the infant requires cutting by the incisor teeth, then grinding by the molar teeth ; but the digestive organs are not for some months fitted for the former kinds of food requiring laceration, for which the canine teeth are intended.

PATHOLOGY AND PRACTICE OF PHYSIC.

MR MARSTON'S CASE OF DIARRHŒA ADIPOSA, CONFIRMATIVE OF BERNARD'S VIEWS.

This case was that of a man, a labourer, aged 35, who had frequent purging of a very peculiar fatty-looking substance. No tenderness, swelling, or tumour, could be detected, except a slight enlargement of the liver.

“ His appetite was good, and he had a great desire for saccharine matters, fat meat, and hydro-carbons generally. Urine was passed in abnormal quantity, pale in colour, with very slight reaction on litmus : sp. gr. 1·030 : under the microscope it presented a few epithelial scales, and a number of oil globules ; on evaporating a portion, and treating it with ether, these globules were dissolved ; there was less than a normal amount of urea and lithic acid, and no albumen ; but Trommer's test, yeast, and oxide of silver, indicated the presence of sugar, and the skin was harsh and dry though he did not complain at all of these diabetic symptoms, *all* of which disappeared some time before death. Tongue was slightly furred ; gums and inside of lips were pale and flabby ; pulse 100, but it varied during treatment from 90 to 120. No cancer, phthisis, or cardiac affection, could be traced in any of his family, which had been generally healthy. The matters passed from the bowels presented a very fatty, tenacious, and peculiarly slimy appearance, deficient in bile, and altogether different from *fæces* ; on raising a portion on the point of a knife it appeared in greasy masses ; under the microscope numerous epithelial scales, with mucus and a substance laden with oil globules, in every respect similar to fat, were observed ; their fatty nature was rendered more evident by their being soluble in ether, and with liq. potassæ forming a semi-opaque gelatinous mass exactly resembling soft soap.”

The treatment consisted of abstinence from “ farinaceous and saccharine articles of food. Diaphoretics succeeded, though with difficulty, in producing diaphoresis ; the sweat had no acid reaction on litmus. Olive oil, instead of being beneficial, only increased the discharge of fatty matter and deranged the stomach. Purgatives, mercurials, and counter-irritants, with small doses of iodide of potassium and ung. iodin. to region of liver, and a variety of other remedies failing to produce relief, at last all medical treatment was discontinued. His appetite continued good, but the adipose diarrhœa (four or five stools per diem upon the average) continuing, he gradually sank, and died apparently from asthenia, after having been under medical treatment altogether thirteen to fourteen months.”

On dissection the duodenum was “ quite healthy, but the head of pancreas appeared to be converted into a hard schirroid tumour, which did not press upon the ductus communis choledochus, as in most of the similar recorded cases, whilst the body and other parts of gland were atrophied, and its duct was found perfectly obliterated and degenerated into an impervious cord.”

Mr Marston concludes some excellent observations on this case with the following remarks :—“ Mialhe, adopting Bernard's views, explains the action of the pancreatic juice upon the fats by the principle of fermentation. More recently another set of physiologists, of whom Frierichs and Lenz appear to be the most prominent, have published a series of experiments to prove that its absence in the intestine does not prevent the digestion and assimilation of fat. They experimented upon cats, and found the usual amount of fatty mat-

ter in the chyle, after ligature of the pancreatic duct. Here, then, exists a division between two leading sects of physiologists; for Bernard still adheres to his views. The question comes, does this case in any way fill up the gap? It has been advanced by Bouchardat and Sandras, that the free alkali of the juice, or serum of the blood, would be sufficient to dissolve the fat; and adopting Plattner's theory, the soda of the bile may be almost vicarious of the action of the pancreatic juice, in separating the fatty ingesta by saponification with its acids; but whether this could continue long is not evident, and what appears to be the real root of the question is, whether, under long continuance, this would not be incompatible with existence. In the case before us it is possible that the choleate of soda of the bile, and the alkalis of the serum did supply an alkali to the fat, thus saponifying it, and that the choleic acid, if Plattner's views be correct, taking albumen as its base, the resulting compound, which is the product of so highly an organised gland as the liver, on the one hand, and the chemical and antiseptic action of the gastric secretion, which is doubtless itself not a mere chemical combination, but a definite semi-organised material, on the other, might give rise to the fatty albuminous molecular base of the chyle; but that this continuing, these alkalis were not only insufficient in quantity to the digestion of the fatty ingesta, but an imperfectly organised material was formed, from which an imperfect chyle resulted.

"The presence of sugar in the urine in this case is difficult of explanation; but be it remarked that this was a transitory symptom, existing most when the patient partook largely of amylaceous matters, which are easily capable of being resolved into sugar, and ultimately ceasing altogether long prior to death. It might be that a larger than normal amount of fatty matter was presented to and absorbed by the tributary branches of the vena porta, and that through the intermediate action of the liver (if Bernard's views be correct) this was resolved into sugar and carried by the hepatic vein into the general circulation; and being more than normal in quantity, did not altogether undergo decomposition in the lungs (as it is conceived to do) but was excreted by the kidneys. Again, the deposit of fatty matter in the viscera generally may admit of a similar explanation; for, I apprehend, we must regard the fatty degeneration of the liver not as a primary affection, but as a secondary disease, resulting from the actual deposition of fat from the blood, it being found in a similar condition in almost all diseases attended by much emaciation. Perhaps the fatty ingesta in this case, not having been subjected to the vital and chemical action of the pancreatic secretion, had never reached a sufficiently organised state to undergo the necessary changes, prior to assimilation or secondary combustion.

"Looking at the case in all its bearings, it certainly appears to support Bernard's views, which I cannot consider entirely overthrown by the experiments before quoted."—*Glasgow Medical Journal*.

[This is a valuable addition to the six cases of Bright, Elliotson, Lloyd, and Gould, and supports from the result of pathological observation, the experimental researches of Bernard, with regard to the functions of the Pancreas. In a Memoir published in the last number of the Prague *Vierteljahrsschrift* by Dr Eisenmann, a seventh case is quoted by Lussanna in the *Giornale Veneto di Sc. Med. T. ii. 766*, and an eighth case of his own is given. In the two last instances, however, the individuals recovered, the principal symptom having been discharge of fatty matters by stool.]

SULPHATE OF MANGANESE IN HYPERTROPHY OF THE SPLEEN.

Prof. Gintrac recommends this salt as a substitute for and adjuvant of chalybeate remedies, for improving the condition of the blood in anæmic patients. He relates an instance of ascites, where cedema, with great splenic enlargement, formed the sequelæ of intermittent fever, in which $1\frac{1}{4}$ grs. (0.10 gramm) of this drug, given twice daily in the form of pill, produced a complete cure.—*L'Union Medicale*, lxi. 1853.

HYPERTROPHY OF THE SPLEEN.

Professor Christiansen of Copenhagen considers splenic enlargement to be caused by such mechanical obstacles as impede the return of the venous blood to the heart, and by such dyscrasic states of the blood as give a tendency to hyperæmia. He has frequently met with it in cases where there was hypertrophy of the right ventricle of the heart. In these circumstances, he says, so called "infarctions," or capillary apoplexies, are not uncommon, presenting, in its substance, clots which, at first, are firm with well-defined borders, and afterwards become decolourized and softened. Hypertrophy of the spleen also occurs in cases where there exists any impediment to the circulation through the vena cava; where there is constriction or impermeability of the vena portæ: and where there has been suppression of the menstrual or hemorrhoidal evacuations. In the blood-diseases as typhus, cholera, pyæmia, and delirium tremens, he says the organ is frequently not only enlarged, but also altered in structure. He has never seen enlargement from intermittent fever, as this disease is extremely rare in Copenhagen, but he believes it to be due to the repeated hyperæmic condition of the organ. He has frequently observed splenic hypertrophy occurring in cases of Bright's disease, so that the organ weighed from 3xvi. to 3xx. Its condition, in these cases, resembles that observed in it by Rokitansky after intermittent; i. e. it is so hard and brittle that it can easily be cut into thin slices or broken into fragments. It presents on section a coarse granular structure, nodules the size of pepper-corns being embedded in its substance: it is also of a bluish red or dark violet colour, which becomes bright red on exposure to the air. Its form is somewhat changed, the inner border being broader and firmer than ordinary. The fibrous capsule is not firmer than usual, and there are no morbid adhesions to the peritoneum, although these conditions frequently are found in the enlargement following intermittent. The author thinks this condition of the spleen arises from a deposition of albumen in the substance of the organ; and surmises that, after the absorption of the watery elements, the albumen remains behind in the Malpighian bodies, in a solid form, occasioning, by their dilatation, the granular structure alluded to.

Prof. Christiansen has found many pathological lesions of the thoracic and abdominal viscera existing in connection with enlarged spleen. Thus he has found the inferior and posterior parts of the lungs infiltrated with dark-coloured blood; imbibition of the heart, especially of the inner wall of the right ventricle, and accumulations within it of grumous blood; distensions of the veins surrounding the Malpighian pyramids of the kidneys; sanguineous effusions into the peritoneal cavity, and into the external cellular tissues. In *all* cases he found blood extravasated into the intestinal canal, resulting from a diphtheritic inflammation of the mucous membrane, which had caused, during life, bloody evacuations with tormina and tenesmus. As regards *treatment*, he admits that very little is known. He has seen one case do well under the use of *Plumbi Acetas*. Where the patient's strength will bear them, strong counter-irritants, as moxa, caustic, and the actual cautery, may be tried. In the hypertrophy following intermittent, quinine seems to him the best remedy for the restoration of the general health.—*Schmidt's Jahrbücher*, Bd. 79, p. 300.

DR E. H. EOKER OF UTRECHT, ON THE ENLARGEMENT OF THE BLOOD-VESSELS IN CEREBRAL INFLAMMATIONS, AND MENTAL ALIENATION.

The cerebral substance assumes a marbled appearance when its minute blood-vessels, which contain little or no blood, become distended with that fluid. These vessels are so small, (measuring from 0.0005—0.001 mill.), that in health only one or two blood-globules occasionally pass through them; and so numerous are they, that when, in congestion, they are filled with blood, they nearly obscure the whitish colour of the cerebral matter by their rosy colour. Nor is this sanguineous distension confined to them alone, it occurs also in the larger vessels, so that when congestion is present, the whole of the brain is enve-

loped in a fine vascular net-work. If the congestion proceed to inflammation, the meshes of the net-work become more closely approximated, and in the vessels themselves irregular enlargements occur. These enlargements are often so great that the diameter of the vessels is six-fold increased; while, in some places, the swellings are almost aneurismal in their character.

Ecker records in detail the examinations of the capillary vessels of the brain in three lunatics, who died maniacal, and in whom chronic meningitis had previously existed, accompanied by more or less imbecility. In all of these cases he found the vessels greatly enlarged; and he has tabulated their measurements, showing that they greatly exceed those found in the normal condition of the parts,—measuring from 0.275 to 0.310 mmtr., while in health they average only about 0.152 mill. The capillary vessels were largest and most numerous in the corpus striatum, and their enlargement was more conspicuous in the cerebellum than in the cortical substance of the brain. He observed that, in their expansion, their texture became atrophied, and that the tubular nerve fibres of the cerebrum were compressed by the vascular enlargements, just as happens in the case of an apoplectic extravasation.

It may not be uninteresting to the reader to mention the method adopted by Schröder van der Kolk, for the examination of the cerebral vessels. Having cut a very thin slice, (1 mill.) of the cerebral substance, before its vessels have become empty, he spreads it upon a piece of glass, and allows it to dry for a day or two. As it is then capable of being cut without tearing, he carefully thins it still further till it is about $\frac{1}{2}$ mill. in thickness, and transparent when placed under the microscope. It is then moistened with Canada balsam, and enclosed between glasses in the usual manner.—*Deutsche Klinik*. 26. 1853.

BIGNON. TUBERCULAR PERFORATION OF THE STOMACH IN A BOY—FATAL HÆMATEMESIS.

At the Hôpital des Enfants, a boy æt. 6 $\frac{1}{2}$, extremely emaciated and scrofulous, was under treatment for extensive ulcerations in the neck, and died suddenly from vomiting of blood. At the autopsy, numerous tubercular deposits were found, particularly around the ulcers in the neck, and in the peritoneum. The stomach was filled with blood which had escaped from a circular perforation, about the size of a groat, placed at the large tuberosity on the great curvature, exactly in the situation of the gastro-epiploic vessels. The erosion of these vessels could not be discovered, although doubtless the source of the hemorrhage. The perforation was not surrounded by inflammation or ulceration: close beside it a crude tubercle was found under the mucous membrane, and another tubercle, surrounded by sanguineous infiltration and tubercular granulations, occurred on the posterior surface of the stomach. From the specimens it appeared that the tubercles were first developed under the peritoneum, and destroyed the coats of the stomach progressively from without inwards. Examples of tubercle and tubercular perforation of the stomach in children, are among the rarest facts in pathology.—*Gaz. des Hôpitaux*, Sept. 20.

VIGLA. CEREBRAL COMPLICATIONS IN ACUTE ARTICULAR RHEUMATISM.

These important complications have been hitherto mostly overlooked in pathological treatises: they are not, however, extremely rare; for in three months M. Vigla has met with 5 cases, 2 of which recovered and 3 proved fatal. Out of 65 cases, this complication was observed in the proportion of 1 in 13. The cerebral affection is perhaps the most important and dangerous complication in rheumatic fever. It varies, however, in character and intensity in different cases. The different kinds have been classified as follows by M. Vigla:—

1st. Simple delirium, similar to the sympathetic or nervous delirium which occurs in many acute febrile diseases, whether of idiopathic or traumatic origin; in short, *rheumatism complicated with delirium*.

2d. Delirium, accompanied by most of the symptoms, and probably also the lesions of meningitis, or the *rheumatic meningitis* of authors.

3d. Sudden and unexpected ataxic condition, quickly succeeded by fatal collapse or coma, the *rheumatic apoplexy* of Stoll and some other authors.—*Arch. Gen. de Medicine*.

BOUCHUT. CUTANEOUS PULMONARY FISTULÆ.

The author gives this name to perforations of the lung, which communicate, by means of a fistulous tract, with the subcutaneous cellular tissue of the thorax, and secondarily with the external air, when the skin is destroyed by ulceration. From several rare cases which he has observed, M. Bouchut draws the conclusions:—

1st. We must admit the existence of pulmonary fistulæ.

2d. Some of them are completely open on the surface of the skin where it is in contact with the lung adhering to the thoracic walls.

3d. Sometimes the fistula, open externally, communicates first with the pleural cavity, which is filled with air and pus, and consecutively with the perforated lung, which is more or less distant from the thoracic walls.

4th. Other pulmonary fistulæ are incomplete, opening under the skin, and forming soft elastic fluctuating tumours: these are subcutaneous pulmonary fistulæ.

5th. All these fistulæ are the results either of abscess caused by foreign bodies, or of empyema, or of pulmonary gangrene, abscess, hydatids, or tubercles.

6th. The cutaneous pulmonary fistulæ are characterised by the fistulous opening of the skin, through which air is continually escaping during expiration and cough.

7th. Subcutaneous pulmonary fistulæ are characterised by a soft, elastic, fluctuating tumour, more or less reducible, with gurgling under the finger or to the ear. The tumour, sometimes dilated during inspiration, is particularly expansive in expiration and cough.

8th. The subcutaneous fistulæ, forming a tumour, may be easily confounded with intra-thoracic abscesses, and herniæ of the lung.

9th. The cutaneous pulmonary fistulæ may give rise to general emphysema of the cellular tissue.

10th. These fistulæ form generally a dangerous complication of pulmonary disease; but sometimes they are a salutary measure instituted by nature to bring about a cure.—*Gaz. des Hôp.*, Oct. 27.

SURGERY.

ON INJECTIONS OF PERCHLORIDE OF IRON APPLIED TO THE TREATMENT OF ANEURISMS.

Since the meeting of the French Academy of Medicine at which M. Malgaigne's paper on the above subject was read, several discussions have taken place on the same question, and some additional facts have been published. We propose at present to give a short account of what took place at the meetings of the Academy, as well as to lay before our readers the substance of the most important communications which have been laid before the profession.

Discussions in the Academy of Medicine.—M. Malgaigne's paper was read, it will be remembered, on the 8th November, but the subsequent discussion occupied the greater part of the three following meetings.

M. Velpeau stated that having observed that in certain vessels coagulation of the blood had taken place around portions of membrane floating in them, the idea had long ago occurred to him to attempt the cure of aneurisms by the introduction of a foreign body. Accordingly he tried acupuncture, but his experiments were unsuccessful both on animals and man. With a view to make this method more effectual, M. Pravaz added galvanism to the acupuncture.

The results were not equal to those obtained by the ligature; out of twenty cases there were twelve failures, several deaths, eight cures. M. Pravaz afterwards thought of producing coagulation of the blood by chemical re-agents, and after several trials decided upon the perchloride of iron. M. Velpeau considered it impossible to arrive at present at a definite conclusion. Some of the facts were not very satisfactory. For example, M. Malgaigne's case was very singular; the injection of ten drops had produced instantaneously gangrene of the forearm. But gangrene does not come on so rapidly; so that in this case it was probably due to some other cause than the perchloride. Besides, this liquid had frequently been injected in larger quantity without producing such a result. It was a mistake to suppose the perchloride a caustic, it was even doubtful if it could produce inflammation, as in the cases where this occurred it might have been owing to other causes, such as the mode of operating on the state of the individual. If it could be proved that it did produce inflammation, this method ought undoubtedly to be rejected. An important question was as to the nature of the clot produced. Clots may be of two kinds, *homeomorphous* or *heteromorphous*. The first is a normal clot and can be absorbed, though sometimes with difficulty. The second consists of blood not only coagulated but altered and transformed; and it is doubtful if it can be absorbed. It would be interesting to know whether the clot formed by the perchloride can be absorbed; in M. Velpeau's own case it could not. If this be the general rule the method must be abandoned. On the whole, M. Velpeau considered that the perchloride did not at present supersede the ligature; still the ligature had its disadvantages, exposure to inflammation, gangrene and secondary hemorrhage; and although at present the method by injection was not an effectual one still it might become more perfect.

M. Leblanc had taken part in the experiments performed on animals at the veterinary college with a view of testing the safeness of injections of the perchloride. This was shown to be almost complete. Only slight fever and anorexia had been produced. It was a mistake to suppose it a caustic; it might become so if long kept, as a certain quantity of perchloric acid might be formed, but this might be easily neutralised by the addition of a little hydrated peroxide of iron, which would combine with the acid. M. Leblanc insisted on the necessity of applying pressure for some time above and below the point operated on, otherwise the clot formed would be detached, and washed away by the current of the blood. M. Leblanc concluded by reading a letter addressed to M. Debout by M. Valette of Lyons, giving an account of a successful case. (This case we give farther on).

M. Langier thought that this question had been brought forward too soon. There were not yet enough of facts; every day would produce more; and a conclusion formed at present might soon be required to be modified. He considered that the accidents in the cases described were not due to the perchloride, for in that case, they ought to have occurred in connection with the degree of concentration of the liquid, or with the quantity employed. This was not the case; the bad effects were quite independent of these points: there must therefore be some other cause. It was probably this, in most of the unsuccessful cases the operation had been repeated several times. But M. Langier considered that every aneurismal sac, which was punctured several times, was necessarily doomed to inflammation; this was also true of encysted tumors, as well as of aneurisms, treated by acupuncture. He thought if the operation were once performed, he was safe; but if repeated, it would bring on inflammation. The method was only applicable to aneurisms containing liquid blood; as to the poisoning of the blood by the perchloride, this was an unfounded hypothesis. On the whole, M. Langier thought it better to wait before coming to a decision.

M. Gerdy considered that the absolute merits of the questions could not be treated of for want of materials. M. Malgaigne was perfectly right in bringing forward the facts which had already occurred, and in drawing the attention of surgeons to a method which, in so short a time, had produced such disastrous results.

M. Malgaigne said that a new element had been introduced by the successful case of *M. Valette*, where a cure had been obtained without accident and by one injection. This case was a great triumph for the partisans of *Pravaz*. But the operation had been performed in a manner contrary to all the rules of *Pravaz*, much more of the perchloride than recommended by him had been injected. *M. Velpeau* says that we have not enough of cases, and that the instantaneous production of gangrene by injection of the perchloride is improbable. No doubt it is improbable; such a result was never seen till this mode of treatment was adopted. But to what can it be ascribed if not to this agent? In the case of *M. Jobert*, gangrene was also produced. In several cases every imaginable precaution had been taken, but still the operations failed. If these facts do not testify against the operation of what use is logic? *M. Langier* says the bad effects do not depend on the degree of concentration or the quantity likely; but is not this the most complete condemnation of the method. The accidents which have occurred must then have been owing to the idiosyncrasy of the patients, as others have borne a larger quantity without bad effects. On the whole, the method of *Pravaz* is detestable; in some aneurisms it cannot be employed, and in small ones the ligature gives such successful results that it would be folly to give it up. *M. Malgaigne* did not say that the ligature was the last advance in the treatment of aneurism, some of our neighbours across the channel were surprised that we have not abandoned it and tried compression. To conclude, said *M. Malgaigne*, I am quite willing to adopt another mode of operating than the ligature, but provided it be better than the system we have been considering, and that it do not furnish out of eleven cases, four deaths, and five or six failures. I declare boldly that I look upon these surgeons as rash and imprudent, who, under the present circumstances, attempt the treatment of aneurisms by injections of the perchloride of iron.

M. Roux considered that this method did not merit the importance attached to it, and that it could not be put in comparison with the means already at the disposal of the surgeon. Supposing that the perchloride possessed all the advantages ascribed to it, that the clot could be absorbed, that the mode of operating were established, that all the necessary precautions were known, even then we should have nothing superior or even comparable to the method of *Hunter*. The results obtained by the latter method were excellent. Out of eighty-four cases of aneurism, *M. Roux* had operated seventy times by the method of *Hunter*; out of these seventy cases there had been only three cases of gangrene, three of inflammatory accidents, and five of secondary hemorrhage. Even in the case of varicose veins, *M. Roux* looked upon the ligature of the veins as preferable to injection of the perchloride. On the whole, *M. Roux* did not deny that this method might cure aneurisms, still he recommended surgeons not to abandon a sure and long tried operation, for one which was uncertain and had already produced deplorable results.

[No opinion was expressed by the academy as a body, but the general feeling appeared to be, that though no doubt coagulation of the blood could be caused by the perchloride, still it had not answered the expectations of its supporters, and that farther experiments should be tried on animals before it was applied to the human subject.]

False Aneurism Cured by Injection of the Perchloride of Iron.—*Louis Etienne Hugonnet*, æt. 30, was admitted into the Hôtel-Dieu of Lyons, under the care of *M. Valette*, on the 14th July 1853, for a false aneurism, the result of a bleeding two months before. The tumor of the size of a nut, was situated in the bend of the right arm. Pulsations synchronous with the heart were very strong; they ceased on the application of pressure to the trachial artery. On the 21st of July, *M. Valette* having determined to adopt the method of *Pravaz*, injected fifteen drops of perchloride of iron at 30° pressure had previously been applied over the trachial by a tourniquet as well as by the fingers of an assistant. Another assistant compressed the arteries of the forearm. Considerable pain was felt by the patient at the moment of the injection, and continued to

a certain extent during the whole day. The pressure in the trachial was maintained for an hour, that over the forearm for ten minutes after the injection. In the evening the points of the fingers were cold, pulsation could be felt in the ulnar, but had completely disappeared from the radial. Next day the tumour was handled for the first time since the operation; it felt hard and the blood appeared completely coagulated, pulsation could only be felt at the inner part. From this time the case went on most favourably. The tumor gradually diminished in size, and it was found that the pulsation felt at the inner part had been due to the ulnar artery. When last seen in the month of October, the patient's cure was complete. M. Valette recommends the adoption of the following precautions, to attention to which he ascribes his own success.

1. The degree of concentration of the perchloride should be 30°.
2. The quantity to be injected is important. To obtain the coagulation of about one centimetre of blood 12 to 15 deep are required.
3. The injecting syringe must be well made; that of Charriere leaves nothing to be desired.
4. The aneurism must be isolated by compression, for coagulation of the blood is not instantaneous.

Varicose Veins cured by Injection of the Perchloride.—On the 12th of October last, M. Follin presented to the Parisian Chirurgical Society an individual in whom he had produced obliteration of the saphena vein by the method of M. Pravaz. The subject of the operation was a man 52 years of age, who had suffered for many years from a varicose ulcer of the right leg. The saphena was much dilated throughout the greater part of its extent. On the 12th of August, M. Follin injected the perchloride of iron into the vein at two points, one above, the other below the knee. Coagulation of the blood took place immediately, the vessel became obliterated, and the ulcer healed in eight days. Up to this time the cure has been permanent.

The Effect of different Reagents in producing Coagulation of the Blood.—M. Burin Dubriessen addressed to the same society the result of his experiments on the coagulation of the blood by different reagents. The experiments were performed on blood coming out of a vein and retained in a liquid state by means of hot water, with which the vessel in which the blood was received was surrounded. Without entering into all the details of M. Burin's experiments, it will be sufficient to give the general results at which he arrived. All the persalts of iron have a powerful action in coagulating the blood. Thus, the perchloride, the persulphate, and the perntrate appear to be equally effectual. On the other hand, the protosalts seem to have no action whatever. But of all the salts tried, that whose action appeared most energetic was the perchloride of iron and manganese. A variety of other substances was tried, as solutions of chloride of zinc, of bichloride of mercury, of alum, etc.; but none were so energetic as the salts first mentioned.

The society before which these experiments were detailed did not agree with M. Dubuissen in considering that the employment of the perchloride entailed no danger of inflammation; and they also found that, however carefully prepared, it very rapidly became decomposed, part of the iron falling down, while the liquid became acid, and consequently more irritating.

Experiments on Horses with the Perchloride of Iron.—MM. Leblane and Debout have performed some additional experiments on horses by injecting the perchloride into the zygomatic and external plantar arteries. The general result of these experiments has been, that the injection of a few drops of the solution produced coagulation of the blood, and in general obliteration of the artery, without causing any serious danger.

[The perchloride has also been used as a hæmostatic in cases of oozing from stumps and bleeding from leech-bites, and has proved highly efficacious. There seems to be no doubt of its being capable of curing some aneurisms, but the advantages held out by its partisans seem more than counterbalanced by the attendant dangers.]

DR HILTY ON THE FORMATION OF INNER CALLUS.

Dr Ulrich Hilty, in his inaugural dissertation, has described some experiments made with the view of investigating the formation of callus within the medullary canal, in the union of fractured bones. For this purpose he inserted pegs of ivory and silver wire into the tibia in cats and rabbits; and in every case he found the foreign body surrounded by a deposit of inner callus, distinguishable from the surrounding bone by its white colour. When placed under the microscope it exhibited numerous vascular canals, communicating with those of the old bone; while in the bony mass itself, there existed a turbid hyaline substance, and round elongated bone corpuscles, with long and tortuous radiating canaliculi.

Dr Hilty found the deposition of the inner callus to begin thus:—Immediately after the irritation of the bony substance, by the foreign body, the medulla became redder and firmer, and its vessels were distended with blood. Shortly thereafter, at the injured part, there was deposited, between the inner surface of the bone and the medulla, a greater or less amount of exudation, at first of a serous character, but afterwards of a gelatinous consistence. This gradually increased, until it either surrounded or bridged across the peg. The exudation was conical in shape; thickest near the peg, and tapering away towards the periphery. It began first to solidify at the periphery,—some traces of cartilage being discoverable there in two or three days;—and the smooth bony surface lying beneath became somewhat roughened and uneven. The cartilaginous formation afterwards gradually extended to the other parts of the exudation, and this process was generally completed by the fourth or sixth day. During this time, and in the same manner, ossification was also progressing, often with such rapidity that after eight or ten days the exudation was surrounded by a bony capsule, while periosteal callus, thrown out synchronously with this, was commonly, in the same space of time, merely cartilaginous in its structure.

This author describes with great minuteness the histological changes observed during these processes. These we cannot notice at any length from our limited space. The exudation was at first a yellowish homogeneous mass, in which were seen fat cells, altered blood-corpuscles, and some elementary fibres of connective tissue. Nucleated cartilage cells were formed in this, appearing first as dark spots or clear vesicles which become surrounded with an investing membrane. Calcareous granules were gradually deposited in these till ossification was complete.

From these investigations, as well as from his own experience in diseases of bone, Professor Meyer of Zurich, deduces the following results. He considers 1st, That in the union of fractured bones, callus is thrown out simultaneously by the endosteum and the periosteum, but that the inner callus is of little importance as regards the healing process.

2dly. That what he terms “sclerosis” of bone, *i.e.*, an obliteration of the medullary canal by hard osseous structure,—is caused by hyperæmia of the endosteum, just as exostosis results from this condition of the periosteum.

3dly. That the thickening of the cancelli, occurring when the areolar texture of the articular extremities of bones is exposed, is caused by an increased osseous deposition, excited by the mechanical irritation of the endosteum.

4thly. That in this manner also are formed the compact lamellæ, lining the *foveæ glandulares*, formed in the cranial bones by the compression of the pachionian bodies.

5thly. That in the case of malignant periosteal growths compressing the interior of the bones, the hardening of the contiguous osseous structure, and the obliteration of the medullary canal by bony stroma, are caused by the implication of the endosteum, and are analogous to what occurs, when the periosteum is affected by bony tumors growing outwards.

6thly. That the osseous capsule which envelopes abscesses, occurring in the cancellous texture, is analogous to the external bone growth, (*osteophytbildung*) so common in the vicinity of ulcers. He observes that it frequently happens to healthy bones that their structure becomes absorbed externally, while fresh

osseous formation is actively going on in their centres,—just as, in the process of their development, absorption occurs internally,—to form the medullary canal,—while new bone is being simultaneously deposited on their exterior. This fact he considers explanatory of the condition of many flat bones,—as those of the cranium or the scapula, and the ileum,—which in their perfect condition, contain more or less spongy substance,—and which, in old age, become often thin as paper, while at the same time, they consist wholly of compact osseous tissue.—*Henle und Pfeuffer's Zeitschrift, Bd. III., Hft. 2.*

CASE OF WOUND OF THE ABDOMEN AND INTESTINES TERMINATING FAVOURABLY.

BY DR LUCAS CORONEL Y DIAZ.

On the 25th September 1838, Dr Diaz was called to see a soldier, 23 years of age, who had been wounded shortly before. On examination there was found between the anterior and superior iliac spine of the left side and the umbilicus a wound directed from above downwards, and from without inwards, traversing the whole thickness of the abdominal wall, and giving outlet to a loop of intestine, partly covered with omentum, the wound bleeding freely, and partially covered by clots. Cold fomentations having been employed with a view both of stopping the hemorrhage and of allowing the state of the gut to be seen, it was ascertained that the portion of the intestinal loop which corresponded to the anterior and inferior angle of the wound had been divided in a longitudinal direction for the space of about six lines. Dr Diaz having had the patient conveyed to the hospital, determined to practise the intestinal suture, not being willing to expose the patient to the infirmity of an artificial anus. The operation was performed in the following manner:—The surgeon seizing, with the thumb and index finger of the left hand, the serous coat of the intestine, succeeded in turning the edges of the wound inwards towards the cavity of the intestine, and in bringing them in contact. Five points of suture were then applied, so that when the wound healed the threads might fall into the cavity of the intestine, and not into the peritoneum. The taxis was then carefully applied, and the intestine returned into the abdomen. The edges of the abdominal wound were then brought together by four points of interrupted suture and three slips of adhesive plaister. On the following day there were symptoms of considerable reaction, pulse hard and full, tongue dry and loaded, some pain and tenderness of the abdomen. The patient was bled to 6 oz., and 18 leeches were applied to the abdomen with some relief to the symptoms. On the 5th day after the operation some puriform matter were passed by the anus; the fever and pain in the abdomen were much diminished. The opium which had been given up to this time was ordered to be discontinued. The next morning an emollient injection was given, which produced two stools. The external wound was examined for the first time, and was found covered with healthy pus, and united in all its extent except at the anterior and inferior angles. On the 13th day after the operation the external wound was almost entirely cicatrised. On the 30th day after the operation, the patient left the hospital, and a fortnight afterwards he returned to his regimental duty.

The patient in this case had taken no food for six hours before the time when he was wounded; he had always enjoyed excellent health, and assistance was afforded him very promptly. To these circumstances Dr Diaz attributes much of the favourable result.—*Gaceta Medica de Madrid, September 1853, in L'Union Médicale.*

DEVELOPMENT OF WORMS IN THE EAR OF A MANIAC, ACCOMPANIED BY VIOLENT OLITIS.

BY DR A. BELLETI.

The subject of this observation was a woman 32 years of age, an inmate of the Royal Lunatic Asylum at Palermo, who had been seized with mania, which had gradually lapsed into a species of dementia. Shortly after she was attacked by most acute pain in the left ear, the orifice of which, on examination, was found to be red, swollen, painful and almost closed up. There were also symptoms of general febrile reaction. At this time, the

attention of M. Belletti was directed to a foreign body which had made its appearance at the orifice of the ear; this was of a pearl white colour, smooth, convex and adherent to the neighbouring parts. As it was found impossible to extract this body, an incision was made into it with the effect of giving exit to a large number of worms, of a yellow colour, and three lines in length. During the following three days injections were made into the auditory canal, and a few worms continued to be discharged. A probe was then passed into the meatus and impinged upon a hard elastic body. By using the probe as a lever, a worm was extracted six times the size of the others, but in other respects closely resembling them. These worms were characterised by M. Caleaca, Professor of Natural History at the university of Palermo, as being the larvæ of a coleopterous insect, of a yellow colour, with symmetrical feet, and which, had they remained in the auditory canal, would have become perfect coleopters, of the tribe *claricornes*, and probably of the genus *Nicrophorus dermestes*, which generally inhabit the skin of carcasses in a state of putrid fermentation.

The patient recovered entirely from the disease of the ear, but some months afterwards she died of apoplexy; and, on examination of the head, the sinuses of the dura mater, and the meningeal and cerebral vessels were found gorged with blood, and the cerebral substance softened, on the left side the membrana tympani no longer existed, and the malleolus had also disappeared.

M. Beletti accounts for the development of worms in this situation by the supposition that his patient had introduced into her ear a piece of putrid animal matter, in which one of these insects had deposited its ova.—*Giornale psichiatrico de Pisani*, and *L'Union Médicale*.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXII., 1853-54.

November 16, 1853.—Professor SIMPSON, President, in the Chair.

I. PEDUNCULATED EXOSTOSIS OF THE LONG BONES. BY PROFESSOR SYME.

This paper is inserted in the present Number.

Mr Lister read an account of the structure and examination of one of these exostoses, which is also inserted in the present Number.

Professor Miller said, that he could not concur in the sweeping condemnation of Sir Astley Cooper. The author of the paper had certainly included a great deal too many things in the term Exostosis. He did not think that Sir Astley had called the ordinary exostosis, tipped with its transitional cartilage, a "Cartilaginous Exostosis," although, under that term, he had improperly included the osteosarcoma and enchondroma of modern pathologists,—the one essentially, the other occasionally and partially, consisting of cartilaginous structure.

Neither could Professor Miller agree to Mr Syme's proposed classification of tumours of bone,—solid, hollow, or spicular: it was both theoretically and practically erroneous. *Solid*, for example, would include osseous, osteosarcomatous and osteocephalomatous, formations,—all differing widely, pathologically, as well as in regard to treatment; the first simple, and often requiring no treatment at all; the second simple, at least in the first instance, requiring and admitting of removal by operation; the third malignant, and seldom, comparatively, admitting of operative interference. The hollow tumours, again, would include the expansion of bone under chronic abscess, as well as its hollow growth in osteocystoma (the Spina Ventosa of old authors),—conditions also differing essentially, in both nature and treatment.

Mr Liston had certainly, at one time, amputated much too freely for simple exostosis of the distal phalanx of the great toe; and to this the University Museum was indebted for a very beautiful series of specimens of that affection. Latterly, he had taken to excision of the exostosis alone; except in those cases in which the growth threatened to be obstinately reproduced, and in which removal of the phalanx may still be expedient.

Professor Miller, as a teacher of surgery, and as author of a "Students' Book," had been always ready to acknowledge Mr Syme's understood claim to priority in this country, as to *successful excision* of the upper jaw. But at the same time he, with equal readiness, acknowledged Mr Lizars' merit, as having *first proposed* the operation; a merit not slight—much in the same way as Sir R. Murchison may be supposed to have some merit in having, from purely scientific knowledge, pointed out where Australian gold was to be found; even in comparison with the man who dug up the first nugget.

Professor Miller also objected to Mr Syme's general statement, that solid cartilaginous tumours of bone always required removal of the entire bone, or part of the bone, from which they sprung. Of Enchondroma, there were two recognised varieties: And one of these—the least common certainly—being external to the bone, admitted of being dissected away, leaving the bone entire.

As to Mr Syme's alleged discovery of an investing cyst to the common "pedunculated exostosis" of long bones, that part of their pathology was well known already. He (Prof. M.) had described it in his "Book for Students"—his Principles of Surgery; and had taught in his class these practical circumstances in connection with it:—1. That it obviously facilitated removal of the growth at its neck; 2. That, after removal by operation, this adventitious serous cyst was apt to inflame untowardly; 3. That, in consequence of a blow or other injury, an inflammatory process was apt to be kindled in the parts, whereby serous accumulation might take place within the cyst, imparting to the tumour a simulation of the rapid growth, pain, and elastic feel of malignant formations; and that so, serious error of diagnosis might accrue.

At the same time, he must protest against this membrane being called *synovial*. The cartilaginous investment of the growing exostosis (for it is found only in those in process of growth) is not *articular* cartilage, lined by a *synovial* membrane, but *transitional* cartilage, invested by something periosteal, and condensation of surrounding areolar tissue into an adventitious cyst, of *serous* character.

Dr Bennett remarked that the distinction sought to be established by Mr Syme did not appear to be so much founded on the structure of the tumours as on their situation. The structure of the external exostosis, according to Mr Liston, was at first cartilaginous, and the cartilage was afterwards transformed into bone. There was every reason to believe that the tumours originating in the interior of the bone were formed in the same manner. Hence the situation of these tumours was of more importance practically, than pathologically. He could not agree with Mr Miller's views as to the malignant diseases of bone, believing that the distinctions he had sought to establish, as guides to surgical interference, were not founded upon a just appreciation of the nature of these growths.

Dr Dunsmure exhibited a calculus he had removed by lithotomy from a man, the nucleus of which was composed of a piece of woollen cloth. The case was as follows:—

II. CASE OF LITHOTOMY. NUCLEUS OF THE STONE A PIECE OF THE PATIENT'S DRAWERS OR TROUSERS.

William Howell, æt. 53, a bookbinder, admitted into the Royal Infirmary 18th October 1853, states that he enjoyed good health till the commencement of his present illness in 1842. Having slipped his foot while stepping from the Bass Rock into a fishing boat, he fell backwards on the "thowe pin," which entered the perinæum at the posterior margin of the right side of the anus. Although much hurt, he was able to raise himself, and take his seat in the boat, without acquainting his companions of the accident. Very little hemorrhage occurred,

and when the boat landed, he walked home, a distance of a quarter of a mile, without assistance. He was confined to bed for three weeks with severe inflammatory symptoms. A large abscess formed in the perinæum, which burst externally, and in three days after this he observed his urine to flow through the opening. He gradually regained his strength, and in about five months from the date of the accident the perinæal wound closed. Shortly afterwards he first observed the stream of urine to be suddenly interrupted, as if by some foreign body. Upon examination of the bladder at this time by a sound, no stone could be detected. About two and a half years ago he was again sounded, when a calculus was discovered. He refused, however, to submit to an operation; and it was only on his sufferings becoming greatly aggravated, that he again applied for relief. The operation of lithotomy was performed on the 4th of November 1853 by Dr Dunsmure, and a stone extracted about two inches long, one and a half broad, and three-fourths of an inch thick. Upon making a section of the calculus, it was found that it consisted of phosphates, was very brittle, and contained in its centre an oval cavity, about an inch long and a half of an inch broad, within which was a portion of woollen cloth of the size of a small filbert nut. The cavity was considerably larger than the cloth, which had apparently shrunk, after the phosphates had been deposited upon it, by the urine passing off through the walls by exosmosis. The patient made an excellent recovery, no bad symptom having occurred after the operation.

December 7th, 1853.—PROFESSOR SIMPSON, President, in the Chair.

I. REMARKS ON THE MINERAL WATERS OF HOMBURG. BY W. SCOTT, M.D.,
EDINBURGH.

Dr Scott was induced to bring this subject before the Society, on account of the slight acquaintance possessed by practitioners generally with the virtues of the Homburg springs, and from the circumstance that a recent residence of some months there had enabled him personally to investigate and observe their effects in certain diseases.

Dr Scott then gave an account of the village of Homburg, and of the principal springs which existed there, such as the Elizabethen, the Kaiser, the Stahl, the Sauer, and the Bad Brunnen. The waters contained variable proportions of the muriate of soda, muriate of lime, and carbonate of iron. The Kaiserbrunnen was considered the strongest saline, from its possessing much more of muriate of soda in its composition. The Stahl-brunnen, on the other hand, was the strongest chalybeate.

The season extended from the middle of May to September; and during this period, from the sheltered situation, the temperature was remarkably regular. The time for taking the waters was from 6 to 9 A.M., when the grounds presented a scene of great gaiety, from the numerous promenaders, who walked about to the enlivening strain of an excellent band. The baths were generally taken three hours after breakfast. In the dietary, which *Dr Scott* gave at length, he recommended the avoidance of vegetables, as potatoes and fruits, while animal food should be as digestible as possible, consisting of plain roast and boiled meat.

Occasionally disastrous effects were found to follow the use of the waters. He referred at length to the case of the late member for Peterborough, whose death had occurred in July last. This gentleman had long been suffering from a disturbed state of the bowels, and a variety of treatment was had recourse to in vain. He tried the waters; but they produced a spasmodic affection of the bowels, which led to a fatal issue. The case too of the late Duke of Nassau might also be mentioned, where death suddenly took place after using the bath. As to the so-called *Bad-krisis*, with its disagreeable train of symptoms, he thought it might be entirely obviated by giving some mild alterative, or suspending for a time the use of the waters.

In his own experience, he had found the waters of Homburg especially useful in all forms of biliary derangement, and particularly in the dyspepsia resulting from too close attention to business; although, no doubt, a good deal of the benefit obtained was due to the change of scene and mode of living, etc. Also in rheumatism and gout; in hepatic diseases, although perhaps here Homburg was surpassed by some of the thermal springs of Bohemia; in female complaints, in which, next to dyspepsia, the most benefit was obtained; and finally, in chronic catarrhal diseases, and in glandular and hæmorrhoidal affections. As to the affections of the skin, the various forms of herpes and impetigo were found especially tractable. Generally speaking, the waters were contra-indicated in all acute and febrile diseases, in organic diseases of the heart and lungs, and in cases with tendency to congestion of the head. The waters should not be used in cases of irritability of the stomach, where it depended on disease of the kidney. In conclusion, Dr Scott mentioned that Artesian wells were being bored, which would add greatly to the present supply; and recommended to the notice of the Society the late work of Frederic Müller on the waters of Homburg. The waters of Homburg contained no bromine, and thus differed from those of Kreuznach.

Professor Syme had found that the use of mineral waters generally aggravated hæmorrhoidal affections. As to the case of sudden death mentioned by Dr Scott, he attached little importance to its having occurred after taking the bath; as patients at these mineral springs were so continually going in and out of the baths, it was no wonder that death should overtake them in one or other of these processes, just as it did other people in their beds. He also considered Dr Scott to labour under a popular error, in supposing meat to be most digestible when roasted and boiled. Stewed meats and French cookery generally, he believed to be preferable.

Professor Simpson, after inquiring of Dr Scott as to the differences subsisting between our own Scotch mineral waters and those of Homburg, stated that in this country the chief ingredient was muriate of lime. He had latterly seen good reason to adopt Sir Robert Carswell's views with regard to fibrous tumours of the uterus, viz., that they were merely a nodose collection of the ordinary tissue of the uterus, and liable to go backwards, or undergo a species of involution, just as took place in the normal uterus after delivery. If this were the case, the beneficial effects of the waters of Kreuznach were easily explained. Cases, however, he had seen, which had returned to this country without the slightest benefit; and in such the sustained use of bromide of potassium was followed by marked diminution in the size of the tumours; in one case, fully one half. This salt is more potent than the iodide of potassium, and seems to be useful as a tonic as well as a deobstruent. In two cases in his practice it produced salivation. Dr Locock had stated to him that several of his cases had been much improved by the Kreuznach waters. But in a case which had occurred lately in Dr Simpson's practice, no good had followed the use of the waters, though the medical attendant at the place thought otherwise.

II. CONTAGIOUS NATURE OF CHOLERA. BY DR TRAILL, OF ARBROATH.

This paper presented a well drawn-up history of all the cases of cholera which had occurred in Arbroath during the recent visitation of the epidemic. The cases in all amounted to twenty-six, of which fifteen died, eight recovered, and three were still under treatment. Of the twenty-six, four cases were ascertained to have been imported; in thirteen, communication with infected individuals could be traced; but in nine no communication with infected persons could be determined. In five of the cases, cholera was only fully developed after removal to the House of Refuge; but all the cases were affected with diarrhœa. Of the nurses, one died; and all suffered from diarrhœa. Five of the patients were of dissipated habits; two remarkably so: the remainder consisted of children and of persons reputed sober. In the town itself there is a general want of good dwelling-places for the labouring classes. The water, which is obtained from

wells, is abundant, and the soil rests on dry gravel. The sanitary condition of the town, which is above par, does not explain the occurrence of the disease.

This statement was tested by reference to a neighbouring locality in the town, in nearly precisely similar circumstances as regards sanitary condition, and containing 130 inhabitants. Of these, not one took the disease. And a more striking instance was cited in the case of the row of six cottages, where the disease first made its appearance. In all, the drainage was imperfect; but the cottages to the west were more favourably placed than the others. In the four centre cottages, there were twenty-seven persons. In the two end ones, twelve. Of the twenty-seven, all but one escaped, and that was an infant; whereas of the twelve inhabiting the end cottages, six took cholera, four choleraic diarrhoea, and two suffered from diarrhoea. Dr Traill did not undervalue the importance of sanitary reform, but he confessed his conviction, supported by his late experience, that cholera was propagated by contagion.

Professor Simpson called on Dr Tait of Dunse to favour the Society with an account of the recent epidemic in that town.

Dr Tait stated, that of the ten cases which had occurred he had only seen eight. Between the first and second cases no communication could be traced, but the houses were contiguous. The drainage was bad. Case No. 3 lived about 100 yards from No. 2; he was a railway-guard, and had communication with Newcastle in the discharge of his duties; he suffered from diarrhoea for a day or so, and was drunk the day previous to his seizure. No. 4 was the wife of No. 3. Nos. 5 and 6, the Knoxes, father and daughter, lived in the middle floor of the same house as Nos. 3 and 4, who were waited on by the daughter, and had some clothes washed by her. One old woman who resided on the same floor with Nos. 5 and 6 had no communication with persons labouring under cholera, and escaped: the same is true of three people who occupied the ground floor. No. 7 lived several hundred yards away from the last cases. He was the sexton, and had, on several occasions, assisted the nurse in waiting on those affected. No. 8 was the late Dr Drysdale, who attended all the cases; but on the day of his seizure was more than usually exposed, as for three hours he had to officiate as nurse, and from the extreme jactitation of the patients, was brought into closer contact with them. No. 9 was a woman who came to attend upon the Knoxes, and was of drunken habits. No. 10 was the wife of No. 7; she assisted occasionally in nursing; she died of the consecutive fever. Case No 1 was of a delicate frame of body; while case No. 2 was of intemperate habits. The drainage in the neighbourhood of both their houses was very imperfect. Such facts no doubt gave a strong predisposition. He had remarked the immense number of midges which loaded the atmosphere during the prevalence of the epidemic. In Dunse, the average duration of the disease was from twenty-four to twenty-eight hours. The whole duration of the epidemic was seventeen days, and the number of cases of diarrhoea was twenty.

Dr Lowe had also observed an unusual number of midges which seemed to be of different kinds in different epidemics. Shortly after the commencement of the epidemic in 1848, he had observed that the ordinary midges were replaced by Aphides.

Dr W. T. Gairdner said that in the last epidemic, he obtained reports from practitioners in various parts of the country; and that he had at one time been engaged in making a summary of these reports. In the course of his study of these reports, he was at first inclined to the opinion that cholera was not contagious; but before he came to the last of them, he was convinced that cholera, although not so communicable as typhus, was still strongly contagious, and that the contagion followed the track of typhus, and could often be traced to the same houses and in the same order; and that sometimes these visitations were not in the worst-drained or most unhealthy parts of the town, just as was also observed in the case of typhus. The second fact to be noted was, that in a large number of cases, the disease could be ascertained to spread after importation, not before it; sometimes a series of cases could be traced to occur, evidently owing to importation, although the importation itself could not be satisfactorily made out. For

example, in Stirling the cholera was seen to close in from all sides, before the town itself was attacked. In Selkirk, owing to its limited communication, there was supposed to be no importation of the disease; but this was only a negative fact. In Dumfries, during the epidemic of 1832, all the medical men were opposed to the doctrine of contagion, and in one of the reports of 1848 it was stated, that medical men and nurses were not more subject to cholera than others, and that cholera cases were distributed with impunity among the other cases in the hospital. Now the true state of matters was, that great care was taken in the isolation of cholera patients in the general hospital, and that in 1832 no cholera cases were admitted. In 1848 cholera cases were admitted, and one nurse out of five died. Several of the nurses were affected with diarrhoea, which was quite general among the patients,—one medical man out of seven died, and of twelve special medical men, two were attacked, one of whom died. The reports of Dr Hamilton of Falkirk, and of Dr Hood of Kilmarnock, showed strongly the contagious nature of cholera; also that of Dr Craig of Kilwinning, who was attached to the Eglinton Iron Works, and who attended most of the cases occurring among the miners, who were very apt to crowd into the attacked houses. When the epidemic spread in this place, outside the miner's circle, it attacked first the grave-digger, then the father of the grave-digger, whose residence was isolated from his son's,—then the sister-in-law, then her two children, then the mother, two married sisters, and then the husband of the one and the son of the other. This greater number of cases occurred in the healthy part of the locality. He would have cited many other instances, but he thought enough had been said to warrant the opinion he had been induced to take up.

Mr Syme alluded especially to the circumstance occurring at the quarantine station at Queensferry during the first epidemic, as favouring the contagious character of the disease. Although not contagious in the mass, it appeared to be so in detail.

Dr Simpson gave a detailed account of the facts connected with the arrival of the smack *Trusty* at Leith, and its subsequent stay at Queensferry, at the time of the first epidemic of cholera, to which *Mr Syme* had alluded. He could not but regard the crew of the *Trusty* as foci of contagion. He alluded to the difficulty of tracing cases, and stated, that *Dr Simson* of York found that it radiated from York in various directions, but, at Knaresborough he could not trace it to contagion where two men were attacked by the disease at different ends of the town. Now it appeared that a traveller from Leeds, had stopped at the hotel at Knaresborough, had there had diarrhoea, which passed into cholera, of which he died; and, that the landlady fearing lest her house might be injured in its trade, had caused him to be privately buried in the dead of night, and that the two men who were the first victims, had assisted at the removal of the body. It thus appeared how easily the importation of the disease might escape notice. Even in the small-pox, *Dr Gregory* could not trace the source of contagion in one half the number of cases.

Dr Begbie had not seen, heard, or read anything to prove the contagious nature of the disease. He did not think it necessary to establish its importation, for he considered this only coincident with its epidemic visitation. In 1832 he had had charge of the Castle Hill Cholera Hospital during six months, and nothing he had then seen had exhibited its contagious character; not one medical officer, student, or nurse took the disease. The only fatal case that occurred among medical men in Edinburgh, was that of *Mr Geo. White*, who was very timid, and would not attend a cholera patient. He did not therefore think that contagion was the usual way in which cholera spread, as was also evinced in its visitation of Dumfries, where it was remarkably sudden in its appearance and departure.

Dr MacLagan thought that cholera was not uniformly conveyed by contagion, but that it might become contagious. He considered that medical men had got no further yet, than the opinion expressed by the Edinburgh board of health that cholera was not usually contagious, but that it may become so.

Dr Myrtle had charge of the quarantine house in 1848. The cases admitted into this house were of the kind likely to suffer from the disease, poor miserable people, yet scarcely one case of its occurrence happened among them. In the instance of a family named Forsythe, with whom he came in contact after their removal from the house in which the disease had appeared, the mother and the woman who washed their clothes had died; yet after their removal to a crowded house in Nicolson Square, no one died there. In another case in the west end of the town, in a large house where a patient died, there was no cholera or diarrhoea in the house afterwards. His opinion was that cholera may be contagious, but that it is not so much so as generally thought.

Dr Bennett considered that if, as had been said, cholera were contagious in detail it would also be contagious in the mass. It would be contrary to all the known laws of contagion to suppose that when diluted it had more power than when concentrated. Yet such seemed to be the tenor of the arguments employed during the evening. In small towns and villages, as in Arbroath, where all knew each other its contagious properties were readily made out, but in large cities like London and Paris, or in India, where it existed on a large scale, it was considered to be non-contagious. The great argument in his opinion against the contagious nature of cholera had been hitherto overlooked, namely, its progress from east to west. By contagion was generally meant the spreading of a disease by touch; by infection its extension by means of a poison in the atmosphere; typhus and other fevers were communicated in both these modes, they were both infectious and contagious. Cholera, however, commenced in Calcutta, and proceeded regularly in a north-westward direction through Europe. Russia had called out her enormous armies to form the most perfect cordon, with a view of arresting its progress, but in vain. He was at a loss to understand how, if cholera was propagated by human intercourse, its remarkable progress in one direction across Europe could be explained.

Dr Scott perfectly agreed with what *Dr Bennett* had said as to the propagation of cholera in the mass. And in further proof referred to the well-known case of the ship in Bombay harbour, in which cholera had broken out after a violent land-squall; as also to the fact of cholera having suddenly appeared in a vessel in the Bay of Bengal many days' sail from the land, and then with equal rapidity having died away.

Dr Simpson, with regard to *Dr Bennett's* argument, thought that the same thing occurred in the case of small-pox and other epidemics.

Dr W. T. Gairdner thought that *Dr Bennett's* arguments required a much more energetic refutation. He did not believe that it invariably proceeded from east to west, for it also travelled from India into China, which was not in a westward direction. In 1817 it went from India to Siam and to the Punjaub, and it went also to Persia, but still in the lines of human intercourse. It always attacked seaports first in those islands which it visited, and it radiated from these in all directions.

Dr Bennett asked why then did it attack Edinburgh before Glasgow?

Dr Simpson replied that it was evidently because Edinburgh had more eastward communication, and of course was therefore more exposed than Glasgow.

Dr Lowe stated that cholera reached Liverpool before Chester, in which latter town the first case seen was that of a jockey who had come from Liverpool. The disease seemed to spread in the house in which he died, having proved fatal to the medical man, to the nurse, and to the washerwoman.

Dr Williamson read a letter from a medical friend in Newcastle, which tended to show the contagious nature of cholera during the late epidemic there; and also that dogs were capable of receiving the disease from eating the matter vomited by a patient. He also related from his own experience in Leith, some cases which went to prove its contagious properties; and stated in addition that *Dr Knox* had recorded the fact of a person who swallowed some of the matter deposited by a species of midge, dying very soon after of cholera.

Dr Alexander Wood believed that in certain circumstances, and under certain

conditions, cholera did assume contagious properties, whether by passing through the human body or otherwise, he could not say ; but there was clear evidence that the contact of a person suffering from cholera, would produce that disease in a healthy individual. He was surprised at Dr Bennett's affirming that it proceeded only from east to west. India was the hot-bed of cholera, from which it spread. Countries lying to the westward must needs receive it from the east. There was no further evidence than this adduced by Dr B. against its contagious nature. The mass of evidence hitherto brought forward, seemed to show that cholera was more contagious here than in its birth-place. A friend of his had charge of troops which were stationed on the two banks of a river ; the cavalry on one side, the infantry on another ; but the cavalry hospital was on the infantry's side of the river. On this side cholera was prevalent, but it had not yet appeared on the other. The first who took it on the infantry's side, were the sentinels of the cavalry hospital. There was a small village, some miles distant, at which the clothes of the soldiers were washed, and in this the disease broke out most severely. The reason that cholera was seen to be more readily propagated by contagion in small than in large towns, was because they were small, and its contagious route could be more easily traced. The conclusion he would come to was that it can be, but is not necessarily propagated by contagion, just as scarlatina and small-pox are not always traceable to contagion. With regard to Dr Begbie's statement of his experience at Castlehill Hospital, he observed, that the mother of one of the surgeons of that hospital, who had been visited by her son, died of cholera in a locality where no cholera existed.

Dr Andrew Wood thought that the non-contagionists had not had the best of the argument that evening. Had Dr Myrtle's cases of diarrhoea been left where they were first found, he believed that they would have passed into cholera. In the case of a ship's crew, related by Dr Scott, the influence was certainly not telluric, and he believed that they had acquired it on shore, and that the time which had elapsed between their leaving the shore and the breaking out of the complaint, was the period of incubation. [Dr Scott having asked how long the incubation in cholera lasted ?] He would tell Dr Scott what he meant by the period of incubation. Just as there was a period of incubation in scarlatina and in small-pox, he recognised the same in cholera, and should the period between communication and the occurrence of the disease be protracted for weeks he would still call it the period of incubation. At the last epidemic the cholera and the general hospitals stood very near each other, both were subject to the same telluric influence, and comparison would easily show in which hospital the greater proportion of nurses died.

Dr Simpson said that in the cholera hospital one out of five nurses died ; while in the general hospital there was only one ; and that this nurse was the only one who had communication with the cholera hospital. He added, that although at first the disease travelled from east to west, in its subsequent progress it proceeded southward through Spain and Portugal.

Dr Begbie said that the case mentioned by Dr Wood did not occur during the six months that he had charge of the hospital. He added, that during the panic of cholera many persons found lying in the streets were sent to the cholera hospital, put into warm beds formerly occupied by cholera patients, and thus most fully exposed to the influence of contagion, and yet in no instance had he known these people to be affected with cholera.

Dr Bennett begged leave to re-state, as a well-established fact (notwithstanding the surprise with which the statement had been received by one member), that cholera, unlike small-pox or any other known contagious disease, advanced in a continuous wave from east to west across Europe, attacking the east coast of England and Scotland first, subsequently the western coast, then Ireland ; and from thence proceeded further westward, across the Atlantic to America. What was more remarkable, it left distinct intervals, and then re-appeared, and had now followed three successive times the same progress from east to west, subsequently dipping southward towards France and Spain. This undoubted fact

he considered altogether opposed to the notion of cholera being propagated by human intercourse. Those whose minds are already biassed, could easily believe it to be contagious; yet it had been shown this evening that in a small town, Dr Traill, although on the look out for facts in favour of contagion in the limited epidemic at Arbroath, could not avowedly discover cases of its propagation by contagion, in the large proportion of nine to thirteen of the whole amount. Experiments on dogs had been brought forward by contagionists in support of their views. Now human contagion was scarcely ever communicable to the lower animals, indeed he only knew of one exception, and that was determined recently in France, namely, the virus of chancre to certain species of monkeys. The poison of small-pox was no exception, for small-pox introduced into cows became cow-pox. In conclusion, he would cite a fact which had come under his own observation, and which would serve to show upon what slender foundation some contagionists built their arguments. A patient in his clinical ward, during the last epidemic, had taken cholera which, on the most careful examination, could not be traced to contagion. At last it was remembered that Dr Robertson had brought to him (Dr B.), for microscopical investigation, a small test tube containing the dejections of a cholera patient, and this tube had been carried into the nurse's room adjoining the wards. This was thought amply sufficient, by certain contagionists, to account for the occurrence of cholera in the patient referred to.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

SESSION 1853-54.

MEETING I.—*November 19, 1853.*—Professor BENNETT, President, in the Chair.

The President said that, in commencing the present session, he would sincerely congratulate the Society on the position it occupied. Its objects were now well understood, and the principles on which it was founded had been adopted by similar institutions abroad. He had observed that the Medical Society of London had commenced a series of Physiological meetings, which indicated the desire generally expressed, if not always carried out, of founding the practice of medicine on a scientific basis. The Biological Society of Paris, and the Physico-Medical Society of Wurzburg, were also actively engaged in prosecuting medical science with enlarged and sound views of what was most wanted at the present epoch; and he had no doubt that the gentlemen he had now the honour of addressing would continue to exert their energies in the same cause, and, by their communications, render the meetings of the Edinburgh Society as interesting as heretofore.

I. ON THE LOGICAL RELATION OF PHYSIOLOGY TO THE PHYSICAL SCIENCES.

The following paper was read by the Secretary from *Mr G. Gore*, of Birmingham:—

A consideration of nearly every fact in physiology impresses us with the idea that various physical forces, as chemical affinity, electricity, heat, motion, etc., are acting in and form a portion of nearly every physiological action, and we naturally and reasonably conclude that, in such cases, these forces are, in some way or other, necessary to the production or existence of physiological phenomena, and that from their constant and intimate connection there exists some logical relation between the physical and vital forces.

The points of physiological science which are logically connected with the physical sciences are probably nearly as numerous as the facts of the science itself, because nearly every vital action is attended by some manifestation of the physical forces; but we will limit our attention to a few of the most obvi-

ous and important, together with a few remarks upon the influence of the logical order of knowledge upon medical studies.

In the first place, all physiological actions are logically dependent upon the following conditions:—1st. Upon the presence of a material substance for them to be manifested in ; 2d, Upon a certain degree of complexity of composition of the material substance ; 3d, Upon a certain degree of complexity of structural arrangement of its elements ; and 4th, Upon the exertion of certain forces upon or within it.

With regard to their logical dependence upon the presence of material substances, we find no evidence, either by experiment or observation, in the whole science of physiology, of their existence, independent of matter ; but by chemical analysis we find that in every instance where vital action is manifested, carbon and other substances are always present, and we therefore conclude that those substances are essential to vital action. We further find that out of about sixty-two elementary substances found in nature, about sixteen or eighteen have been detected in vital structures ; and, of these, about seven or eight, viz., oxygen, hydrogen, nitrogen, carbon, sulphur, phosphorus, lime, and iron, are essential to the life of the particular structures in which they are found.

The logical necessity of a certain degree of complexity of composition of the material substance is evident from the fact that vital actions are never manifested in substances of simple composition, such as those in which the physical forces alone operate, but only in those containing a much larger number of elementary substances.

The logical dependence of physiological actions upon a certain degree of complexity of the structural arrangement of the material substance is clearly shown by the fact that vital forces are never found except in *organised* substances formed from germ cells, which possess a structural arrangement far more complex than any to be found in inorganic matter.

And the further logical necessity of the exertion of certain forces upon the material structure, to the existence of physiological actions, is shown by the fact that no organised structure appears to manifest vital action without an attendant exertion of mental or physical forces within it.

In the second place, physiological actions are logically more complex than physical ones. They are more complex, considered in reference to the *material substances* in which they are found, because they take place in substances which are composed of a greater number of different kinds of matter, and of more complicated structural arrangements than those in which physical forces alone operate. They are also more complex, considered in reference to the *forces* exerted in them, because they consist not only of actions of the vital forces, but also of physical forces superadded and combined with them. This coincidence of the degree of complexity of physiological actions, with the degree of complexity of the material structures in which they are found, is but an illustration of the general principle in physical science, that the complexity of a phenomenon, and of the force or forces which operate in it, are the same in degree, and logically dependent upon the complexity of the material substance in which it is manifested.

In the third place it is evident, from a study of physiology, that vital and physical actions are often logically connected together as cause and effect ; that, in some instances, vital actions produce physical effects, as in the production of heat by repeated muscular contraction, or of motion, by reflex action of nervous force, etc. ; and that, in other instances, physical actions produce vital effects, as in the production of vital and nervous force by the assimilation of food, for we may remark that any material substance conveyed into a living organism carries with it all the physical forces inherent in it ; and we may consider vital actions to be, in many cases, results of the action of physical forces upon organised structures.

There is no doubt but that many vital actions are produced by other causes

than the physical forces. The influence of mind, for instance, is a frequent cause of them ; but as our object is to speak of the logical relation of the physical forces to physiology, and not of the mental or other forces to it, we must omit the consideration of those other influences.

As in some instances vital forces produce physical effects, and in other instances physical forces produce vital effects, it is evident that neither the vital or physical forces can *abstractedly* be said to be the cause of each other, but that each may in its turn, under different circumstances, be the cause of the other ; and as effects are always equal to the causes that produce them, and action and reaction are equal, it is also evident that vital actions are equivalent to those actions of the physical forces which produce them.

All physical phenomena may be viewed as logical results either of the laws of extension, or of the laws of motion, statical phenomena being the results of the former, and dynamical phenomena results of the latter ; consequently, all the physical properties of bodies, whether organic or inorganic, are logical results of their mathematical properties. Statical phenomena include only the simple existence of matter, of different magnitudes, numbers, forms, and positions, together with those attendant manifestations in which *motion* or *action* forms no necessary element. Dynamical phenomena include only those in which *action* or *force* is superadded to matter ; therefore the science of anatomy, in so far as it relates only to the magnitudes, numbers, forms, positions, and other statical phenomena of organised structures, is a purely statical science ; and physiology, in so far as it relates only to the *material* conditions of life, as the presence of suitable substances, food, air, water, etc., in living organisms, is also a static science, but in so far as it takes cognisance of the *actions* of physical, vital, and mental forces in them it is a dynamical science.

The science of physiology being logically dependent upon a material structure, and based upon anatomy, includes a knowledge of all the *material substances* which compose organic structures, viz., hydrogen, carbon, oxygen, nitrogen, sulphur, phosphorus, lime, iron, and the various other substances which are present in living structures, either for mechanical, chemical, or other purposes ; their structural dispositions into bone, muscle, nerve, membrane, blood, etc. ; also, the magnitudes, forms, and positions of every organised being. In addition, to this, it also includes a knowledge of all the *forces, actions, or dynamic conditions* of those substances, which are present in vital structures ; thus it requires a knowledge of the laws of motion in order to understand the mechanical functions of the various bones, muscles, arteries, veins, and air-vessels ; their adaptation to support weight, resist pressure, bear extension, flexion, compression, expansion, etc. ; also, to be able to understand the mechanical processes of respiration, circulation, and absorption ; together with the influence of mechanical agents upon the different organs ; it further requires a knowledge of the laws of heat and light in order to be able to explain the various thermic and optical functions of different parts ; the production of heat in respiration, digestion, oxidation, and by muscular and nervous action ; also, the endurance of heat and cold, and maintenance of temperature by organised beings ; the effects of heat and cold upon respiration, exhalation, absorption, evaporation, temperature of organs, reparative process, intensity of vital action, and duration of life in plants and animals ; also, the influence of light upon germination, vitality, exhalation, and upon the absorption of carbon from the air by plants ; the evolution of light by glow-worms and other animals ; together with the functions of the optic nerves, ganglions, and their appendages. It requires a knowledge of the laws of electricity in order to be able to understand the various electric functions of organs ; the production of electricity by nervous force in various fishes ; the development of nervous force and muscular action by electricity ; the evolution of heat by electrolysis in organised structures ; and the numerous effects of electricity upon the functions of digestion, nutrition, secretion, etc. A knowledge of physiology also requires

a knowledge of chemical affinity, in order to understand the chemical functions of all those substances which compose organised structures, or are present in them ; also, the production of heat by chemical action in respiration, digestion, and the various oxidating processes ; and the dependence of the purification of the blood, the solution of food, etc., upon chemical action. It also manifestly requires a knowledge of the laws of the vital and nervous forces, in order to understand and explain those immense number of facts which are the peculiar objects of physiological study, the phenomena of germination, cell growth, progressive development of tissues and organs, digestion, absorption, nutrition, secretion, repetition of parts, incident and reflex action, sensation, motility, sleep, dormant vitality, death, and all other vital phenomena. It is evident, therefore, that a comprehensive knowledge of physiology is intimately dependent upon a knowledge of matter, and of the various forces within it; and that in order to better understand physiological action we must understand the mutual logical relations of the physical and vital forces.

This can only be accomplished by learning the various facts, laws, and applications of the vital and physical forces in the order of their natural dependence upon each other, and according to the general logical order which regulates the acquisition and use of all kinds of human knowledge.

A knowledge of this order informs us that there is one method of *acquiring* knowledge and another of *applying* knowledge, and that the *study* of physiology belongs to the former, and its *practical application* belongs to the latter, also that the order of *acquiring* physiological knowledge is the opposite of the order in which it should be *arranged* in the mind for reference and use.

It also teaches us that the study of physiology should be both inductive and deductive—inductive by a gradual acquisition and passing from the smallest and most particular facts or ideas in the subject to those facts or ideas which have the largest extent of signification, and which we term laws or principles ; and deductive by applying a knowledge of those laws or principles to the explanation of particular physiological facts. It further teaches us that the study of each of the physical sciences should in like manner also be inductive and deductive, and that the physical and vital sciences collectively should be studied inductively in the order of their inductive logical dependence upon each other, and deductively in the order of their mutual deductive dependence, in order that a correct knowledge may be obtained both of the class of instances in which vital forces produce physical effects, and of those in which physical forces produce vital effects ; also, that a mental facility may be acquired of applying the principles of vital science to the explanation of physical phenomena occurring in living structures, and of the principles of physical science to the explanation of vital phenomena.

It further informs us that the order in which the various physiological functions should be *inductively* studied is the opposite of the order in which they are developed ; the development of living structures and of their corresponding functions proceeds in every plant and animal from the *general* to the *special*, from the *simple* to the *complex* ; thus, in man, physiological development commences with cell-growth, and is completed by the development of sensation and motility ; therefore the *inductive* study of human physiology, which proceeds from *particulars* to *generals*, should commence with those nervous functions, and ascend through the whole series to that of cell formation.

It teaches us that the basis of all physiological knowledge must be an intimate acquaintance with facts, and that the only safe course of arriving at a knowledge of principles is by classing those facts under general and more general expressions by the exclusion of their differences and retention of their similarities, and that a knowledge of those principles is but a knowledge of the abstract portions of the minute facts which compose them.

It impresses upon us the necessity of using extensive observation in the collection of new physiological facts, because, according to logical rules, the more complicated a phenomenon and its conditions are, the more numerous must be

the observations required to confirm it. It also informs us that physiological phenomena are less calculable and more dependent upon circumstances than physical actions, because the equations of the former are more numerous than those of the latter.

It informs us that the conclusions drawn from a superficial examination of physiology are often opposite to those formed from its deeper study, because those influences in phenomena which are the most minute and apparently the most insignificant are often the embodiments of the most general and comprehensive laws, and form the basis upon which the whole phenomena rest; thus the whole existence of physiological actions logically depends upon that minute and ever-present influence termed cell-growth.

It teaches the young student to be content in reading and studying the facts, laws, and practical applications discovered and invented by others; it indicates to the teacher the duty of developing new facts and principles by experiment, observation, and study; to the medical practitioner it also indicates the duty of making new applications of physiological principles to human welfare; and it informs us all that we should not only read and study to *know* facts and principles, but also to *apply* them; not merely to become *learned*, but also to become *useful*.

It shows us that the greatest perfection of knowledge in physiology consists in our being able to apply it to the correct explanation of the most minute fact and the production of the most minute result in the phenomena of life, and that though correct medical practice *may* result from only an *empirical* knowledge of physiology it *must* result if a complete *scientific* knowledge of it is acquired and correctly applied.

It further informs us that in physiology, as in nearly all other branches of knowledge, man's mind is in a transition state, passing from a state of empirical practice to a practice according to scientific principles, and that the present state of medical practice is a mixture of the empirical and scientific, and that one of the duties of every medical man, if he aim high in his profession, is to assist in removing the science of physiology from the region of empiricism and uncertainty to that of science and principle, by the development of new facts and laws; at the same time it informs us that we must be guided by effects until we can obtain a knowledge of causes, that we must practise empirically or from simple observation of the effects of medicines until we can practise scientifically from a knowledge of physiological principles and of the action of the physical forces through the medium of medicines upon the vital functions.

II. STRUCTURE OF THE TORBANEHILL MINERAL AS COMPARED WITH THAT OF COAL.

Professor Bennett exhibited under the microscope various sections of the Torbanehill mineral and of coal, and described the structure of each. He proposed that a committee of the Society should be appointed to examine these and other sections, and to report upon whether the mineral was composed of vegetable structure, as had been asserted. This was agreed to, and Drs Bennett, Cobbold, Sanders, and Messrs Barlow and Kirk, were appointed members of the committee.

III. PECULIARITIES OF THE PELVIS IN THE COW.

Mr Barlow exhibited a preparation which showed certain peculiarities of the ligaments and articulations of the cow's pelvis at the time of parturition. He observed that in the virgin and non-pregnant state, the sacro-sciatic ligament was so tense, large, and superficially placed, that its substance and insertions could be readily traced under the skin in the living animal. As the period of gestation advanced, and especially during the two weeks prior to parturition, the ligament seemed to relax so as to leave a large furrow visible externally between the sacrum and os innominatum on each side. On examining the apparently relaxed ligament in cows dying close on the time of parturition, it was found, as in the present instance, to be much thicker in substance and of

much greater length than in the non-pregnant animal, and there could be no doubt of this additional material and elongation being the cause of its apparent falling in or relaxation before parturition. In the non-pregnant state the sacro-iliac articulations were secured by a material closely resembling ordinary intervertebral substance, and the amount of their motion was very limited. Shortly before, and at the time of parturition, however, the opposing surfaces of these bones became smooth, and were covered by a lubricating fluid, and the ligaments externally surrounding their articular surfaces were relaxed, so that a considerable amount of motion was thus ensured. The great elongation of the sacro-sciatic ligaments combined with the free sacro-iliac articular motion, added upwards of two inches to the vertical (sacro pubic) diameter of the pelvis, and also increased every diameter of its outlet.

Dr Matthews Duncan observed that Mr Zaglas, a late and much-lamented member of this Society, had demonstrated that in man there was always a certain amount of motion of the sacrum upon its transverse axis, that bone moving upon the two ilia, its co-apex being either elevated or depressed, and its base consequently moving in an opposite direction.

These motions take place under ordinary circumstances; but we know that about the time of parturition in woman, the ligaments of the sacro-iliac and pubic joints are swollen and softer than usual, and will therefore admit of this motion being more extended in exactly the same way as happens in so much greater a proportional degree in the cow. In the latter animal the ligaments relaxed are the sacro-sciatic and sacro-iliac; but in woman these joints, and in addition, the pubic articulation, underwent the change referred to. In some cases this relaxation was to a great extent, so as even to constitute it a disease from its injurious influence upon the function of progression.

Dr Duncan would merely notice to the Society how in woman the enlargement of the brim of the pelvis and diminution of the outlet in the first stage of labour by this motion of the sacrum; and in the second stage the diminution of the brim and enlargement of the outlet might take place. These matters were foreign to the subject before the Society. He would only add that the position generally assumed by the parturient female, in the commencement of labour corresponded with the enlarged state of the pelvic brim, while the position, and the abdominal muscular efforts of the second stage corresponded with and produced the enlarged state of the outlet. The action, especially of the two recti muscles of the abdomen upon the pubes, would tend to displace it upwards on the sacrum, and produce the tilting backwards of the apex of that bone.

Mr Barlow exhibited the structure of the ligament under the microscope which, in the opinion of the members, consisted of white areolar mingled with a small amount of yellow elastic fibrous tissue.

IV. CANCER OF THE LIVER.

Dr W. T. Gairdner exhibited a specimen of cancerous infiltration of the liver, a portion of which was very soft, gradually assuming towards its centre a straw-yellow colour, so as to resemble pus. On demonstration under the microscope this portion was found to consist of cells in various stages of disintegration.

Dr Bennett observed that the change in the cancerous structure now exhibited to the Society, constituted, he thought, the commencement of what was called the reticulum in cancer.

V. FIBRO-NUCLEATED TUMOUR.

Mr Joseph Lister demonstrated to the Society, under the microscope, the structure of a tumour, which to the eye presented a cerebriform appearance, and was soft and pulpy to the feel. It consisted chiefly of a multitude of oval free nuclei, of uniform size, resembling those of fusiform fibro-plastic corpuscles. Similar nuclei were also seen imbedded in a soft, indistinctly fibrous stroma. Here and there appeared a fusiform corpuscle with similar nucleus; and a very sparing amount of true white fibrous tissue was also pre-

sent. The tumour, which was of the size of a small cocoa-nut, had been removed by Mr Syme from the front of the forearm of a man fifty-one years of age. It lay in the interspaces between the muscles, some of which were expanded over it. It had produced symptoms for two years: viz., during the first nine months an extremely limited tender spot, the seat of lancinating pain on pressure, but otherwise causing no inconvenience; then a small tumour appeared, which grew at first slowly, but during the last few months with great rapidity, becoming at the same time the seat of spontaneous pain.

Dr Bennett, on examining the demonstration, declared it to belong to that class of tumours which he had denominated "fibro-nucleated."

December 3, 1853.—*Dr BENNETT*, President, in the Chair.

WOOLLEN TEXTURE A NUCLEUS FOR A URINARY CALCULUS.

1. *Dr Bennett* made a demonstration of wool, found in the centre of a urinary calculus. The calculus was extracted by *Dr Dunsmure* from a man who was severely hurt in the perinæum some years ago, while getting into a boat. The woolly substance was probably part of the trousers of the man, driven into the wound at the time the accident occurred.—(See *Report of Medico-Chirurgical Society*, p. 72).

DISEASED HEART WITH UNUSUAL SOUNDS.

2. *Dr Bennett* showed a heart, removed from a patient who had recently died in one of his (*Dr B.*'s) wards in the Royal Infirmary. There was hypertrophy, with great dilatation of the left ventricle, without any very well marked valvular disease. The physical signs of disease, on the other hand, were well marked during life; there was tumultuous action and a sound synchronous with the impulse, which was dull at the apex, clearer above, and not propagated along the large arteries. There was no blowing, and no second sound could be heard.

Dr Haldane and *Dr W. T. Gairdner* believed the valves to be diseased to a greater extent than *Dr Bennett* had admitted. *Dr G.* considered that two of the valves were more particularly affected, and one of these to a greater extent than the other. *Dr G.* was surprised that the second sound should have been absent in the pulmonary artery.

OBJECTIONS TO THE PRESENT THEORY OF THE CARDIAC SOUNDS.

3. *Mr Cartwright* read the following paper on the sounds of the heart, and observed that the case just presented to the Society was, he considered, confirmatory of the truth of his theory:—

"The following remarks refer solely to the impulse of the heart, and are principally extracts from my letters in the *Lancet*, December 20, 1851, and October 23, November 20, and December 18, 1852:—

"1st *Objection*, It may in the first place be objected to the systolic theory, that there is a great discrepancy of opinions among the systolists themselves; whilst Professor Kivisch and his followers go so far as to assert, that no tilting or turning up of the apex of the heart can possibly occur in the living body, seeing that the heart in its natural position lies in close opposition to the walls of the chest.

"2d, *Dr C. J. B. Williams* in his 5th Observation says, 'at each contraction of the heart the finger felt the tension and elongation of the ventricles, and also something like a brief shock, coinciding exactly with the first sound.' A most excellent description of the dilatations of the heart; but I presume, no reliance can be placed on experiments where it is asserted that the heart was elongated during its contraction.

"3d, *Skoda* makes the following statement about a child a few days old, and in which the sternum was deficient, but the skin entire: 'By applying the hand one could easily perceive that the heart was vertically placed, and moved with each systole downwards and forwards, with each diastole upwards and

backwards. The impulse was felt with each systole of the heart, immediately above the insertion of the diaphragm; with each diastole, on the contrary, as high as the second rib, if the fingers were sunk sufficiently deep towards the spinal column. The impulse of the diastole was just as strong as the impulse of the systole. On placing two fingers in such a manner, so that with the systole the lower, with the diastole the upper, finger felt the impulse, it was found that the heart during each systole glided about an inch downwards.'

"I have seen the naked heart, the pericardium having been opened, in five rabbits, in two dogs, and in two asses, and in each instance the heart descended during the dilatation, and ascended during the contraction; I am therefore forced to conclude, that Professor Skoda, in his admirable and most accurate description of what he felt with his hand, misapplied the terms systole and diastole.

"4th, Dr Albers of Bonn states, that the heart must elongate itself several lines, so as to produce the impulse in the fifth intercostal space; as the heart neither elongates itself nor descends during its contraction, but both elongates itself and descends during the dilatation, I am again forced to conclude, that the impulse is produced by the dilatation.

"On visiting Vienna in April 1852, I saw a young man in whose pulmonary artery could be felt the peculiar movement of the semilunar valves; on examination there was a distinct interval between the impulse of the heart and the pulsation of the pulmonary artery.

"In October 1852, with the assistance of Mr Blaikie, surgeon, I examined two dogs under the influence of chloroform, and the pericardium having been opened, we both heard the first sound during the diastole; on placing the stethoscope on the base of the heart during its contracting, my head was raised, but I heard not the least sound; the base then contracted, and the stethoscope instantly slipped off; I repeated the experiment three times: on again placing the instrument at the base, I again heard the first sound during the diastole, passing away, as it were, towards the apex.

"On pressing the lungs together with the hands, so that the heart was as nearly as possible in its natural position, it appeared evident that the impulse could only occur during the diastole; for then the apex portion became enlarged, shooting downwards and forwards with considerable force; whilst during the systole the same part ascended upwards and backwards, and the upper or anterior surface of the apex appeared to sink, to recede from the eye, or from the parietes of the chest; and not to swell upwards and outwards, as it ought to do according to the theory of Professor Kivisch.

"I am also of opinion, that the pathological phenomena, especially of stricture of the mitral valve, are more satisfactorily explained by the diastolic than by the systolic theory.

"From the above considerations I conclude, that the impulse occurs during the diastole."

Dr Cobbold had recently performed some experiments on rabbits, and, so far as facts were concerned, could corroborate what Mr Cartwright stated.

Dr W. T. Gairdner referred to "Hope's Experiments," and considered them as conclusive proof that the present received views regarding the cause of the sounds of the heart are correct. He considered it necessary that actual experiments should be submitted to the Society by any one who brought before it such statements as had been made by Mr Cartwright in opposition to the common opinion.

Dr Bennett observed that no discussion could be useful, unless the fact on which the communication rested could be in the first instance established. But as the question had been raised, it would be worth while to attempt its solution by experiment.

FUNGIOUS GROWTH IN WOOD.

5. *Mr. Kirk* showed some specimens of diseased wood, with demonstrations

under the microscope, taken from the inside of an ash tree, the exterior of which was sound. The large pitted ducts were filled with a fungus, apparently a penicilium, and the case appeared to be, in all respects, like one described by Mr Quekett, in the Transactions of the Microscopical Society of London.— (*Quarterly Journal of Microscopical Science*. No. III. P. 72.)

HENLE'S FENESTRATED ARTERIAL COAT.

6. *Dr Drummond* exhibited a demonstration, under the microscope, of the fenestrated coat (of Henle) taken from the inner coat of the aorta, which exhibited many points of resemblance to the membrana propria, or basement membrane.

CANCER OF BONE.

7. *Mr Lister* exhibited the lower part of a femur, affected with carcinomatous disease, removed by Mr Syme from a boy about 16 years old. The bone had been sawn through longitudinally, and presented a good illustration of the different arrangements, which osseous material may assume in connection with cancer of bone. The shaft immediately adjacent to the morbid growth was on one side simply thickened and indurated, the compact tissue being denser there than at any other part of the bone, so that the Haversian canals visible to the naked eye were extremely few and minute. On the opposite aspect of the bone (also close to the cancerous mass), cancellated tissue had been formed external to the original compact tissue, which still remained, and outside this loose texture was another layer of compact tissue; and thus there existed at this part two layers of compact tissue, with intervening tubercular structure. In the morbid growth itself the bone also assumed different arrangements at different parts. In some lobes of the tumour it was in the form of spicula, radiating from the centre towards the circumference, which, however, it did not reach; while in other lobes it existed at the surface only, being expanded into a thin superficial shell. Finally, in other parts, the malignant growth contained no bone, but seemed to have caused simple absorption of the osseous material that had stood in the way of its progress from the interior to the surface of the bone, from which it sprouted in fungous masses.

This tumour also presented a point of interest regarding the diagnosis of such diseases; for while some parts were soft, and some, indeed, consisted of cysts containing serum or blood, yet one large lobe was of very firm consistence, like schirrus or osteosarcoma, and would, doubtless, have been confounded with the latter, had it not been for the co-existence of the other softer portions.

Dr Bennett observed that many of the so-called osteo-sarcomatous tumours of surgeons were in point of fact cancerous.

BIOGRAPHICAL SKETCH OF M. ORFILA.

The eulogy of Professor Orfila, so long a European authority on Toxicology and Legal Medicine, was pronounced by M. Dubois d'Amiens, at the annual meeting of the Imperial Academy of Medicine. From this source we condense the following short notice of his active and brilliant career:—

Mattheo-José-Bonaventure Orfila was born on the 24th April 1787, at Mahon, in the Island of Minorca, where his family were respectable tradespeople. His early education was derived from priests, who taught him a little Latin and Greek, Rhetoric, and Music, of which last he was passionately fond: from two foreign priests he learned both English and French about the same time, and at the age of fourteen he studied mathematics. Being intended for a sailor by his father, he was sent a voyage to Egypt at the age of fifteen, and remained for three months in the Bay of Alexandria. On his return to Mahon, however, he commenced the study of science under a German teacher, who instructed him for

two years in mathematical and physical science, and in natural history; and at the end of this period he was sent to the University of Valencia, when his career may be said to begin. By the regulations of this university, chemistry was taught according to ancient traditions, and the professor was restricted to the text book of Macquer, who, in 1804, after the discoveries of Lavoisier, Priestly, and Fourcroy, still maintained that air and water were elementary bodies. Orfila soon forsook the lectures, and, turning his room into a laboratory, commenced experimenting after the doctrines of Lavoisier, Bertholet, and Fourcroy, and became so enthusiastic, that for nearly a year he did not sleep for more than three or four hours each night. Soon after, a concours was held among the students of the university, at which Orfila obtained a brilliant victory,—the result of his assiduous and independent labours. After this he studied at Barcelona, where he again distinguished himself; and the junta having resolved to send a pupil to Madrid and Paris, to study chemistry in its applications to the arts, Orfila was chosen for the mission, on which he was to remain absent for four years, and have a chair of chemistry instituted for him on his return. In 1807, he left Barcelona for Madrid, but finding the instruction there insufficient, he obtained a change in the plan of his journey, and proceeded immediately to Paris. Arrived there, he was introduced to Fourcroy and Vauquelin, the latter of whom became his friend and patron—to him, indeed, he owed his personal liberty, when all the Spaniards in Paris were arrested by Order of Napoleon, on the occasion of the war with Spain in 1808. During this period Orfila gave lectures on chemistry in a private laboratory of his own; and he received the degree of Doctor in 1811. He continued in Paris after his graduation, and resumed his lectures on chemistry, numbering Béchard, Edwards, Hippolyte and Julius Cloquet, among his pupils.

It was during these lectures that an incident gave rise to his researches in toxicology, afterwards so celebrated. In his experiments on the reactions of arsenious acid, he had announced to his class that the same precipitates would be produced if the acid were mixed with alimentary substances as coffee, wine, or soup; but on demonstration, the lecturer was not a little surprised to find the colours entirely changed; the lime-water produced a violet grey instead of a white precipitate; and the ammoniated-sulphate of copper, instead of a meadow green, threw down a dark-olive coloured deposit! Orfila was at once struck with the insufficiency of the usual tests in cases of poisoning; toxicology could not then be said to exist; a new system of processes must be discovered for the chemical detection of poisons. He immediately undertook a work on toxicology, engaging with a publisher before he commenced, so great was his confidence of success. After a vast series of chemical researches, and experiments on living animals, he brought out, in rapid succession, the different volumes, and in 1815 completed his celebrated *Traité de Toxicologie*. The work met with the approbation of the Academy, and at once established the reputation of its author and marked out his position in the scientific world. M. Orfila towards the end of the same year, was elected corresponding member of the institute. About this time M. Orfila decided to remain in Paris; the Junta of Barcelona had been unable to found the chair which had been promised him, and, when he was offered the appointment of Professor of Chemistry at Madrid, he declined it unless the course of instruction was reorganised, a condition which the Spanish government rejected.

His future career, now confined to Paris, was marked by extraordinary prosperity and success. He was attached to the household of Louis XVIII., as *médecin par quartier*, soon after his election at the Institute. The same year (1815) he married Mlle. Lesueur, the daughter of the distinguished artist of that name; and his amiable qualities in society opened up to him the most distinguished "salons" of Paris, among which he particularly frequented that of the Princess de Vaudemont. In the summer of 1816 he revisited his native town, Mahon, and was received with the greatest enthusiasm by his countrymen. Finally, in 1818, after being naturalised a Frenchman, he attained the object of his ambition on being elected to the chair of Legal Medicine at the Faculty of Paris. This position,

however, he did not long retain. In 1823 the Medical School was reorganised, and Orfila was appointed in the room of Vauquelin, to the chair of Chemistry, which he held till his death in 1853, a period of thirty years. His success as a public professor, was overwhelming. The chemical class-room was insufficient for his audience; and the lectures were delivered in the large amphitheatre of the school, which was crowded to excess. During his long professorship, neither his zeal nor his popularity as a teacher, ever diminished, and his reputation is unquestioned as one of the most eloquent and attractive lecturers of the Parisian medical school. In 1831, Orfila was elected dean of the medical faculty—a post of considerable responsibility and difficulty; but which he employed for the benefit of the school, by erecting new rooms for dissection, opening the Hôpital des Cliniques, the botanical garden at the Luxembourg, the Museum of Pathological Anatomy (Musée Dupuytren); and in 1844 he obtained the consent of government to the formation of a museum of comparative anatomy within the buildings of the Medical Faculty (Musée Orfila), in imitation of the Hunterian Museum of London. Besides the offices which he held in the university, Orfila was president of the medical juries for the examination of medical officers for the army; president of the Medical Provident Association of Paris, which he founded; member of the Conseil Général des Hospices, and of the Royal Council of Public Instruction; of the Municipal Council, and of the General Council of the department of the Seine. Notwithstanding all these occupations, M. Orfila found time to compose the classical treatises on which his fame rests: the *Traité de Toxicologie Générale*, already mentioned, in which the analytical process for the detection of poisons by the *preliminary destruction of the organic matters* mixed with them, was first laid down; the *Elémens de Chimie*; the *Traité de Médecine Légale*, full of original investigations and useful results; the *Traité des Exhumations Juridiques*. Undoubtedly the most important of his discoveries was the detection of poisons after their absorption from the *primæ viæ*, in the viscera to which they had been conveyed. It was these researches which gave to Orfila's evidence immense authority in cases of criminal poisoning, and decided many of the most remarkable trials of the period; and perhaps his reputation as a professor is inferior to his celebrity as a toxicologist and medical jurist. In 1846-7 Orfila reached the summit of his fame. He revisited Spain, passing in a kind of triumphal progress through its ancient towns; and the degree of doctor was conferred on him by a special decree of Queen Isabella. He had received numerous decorations: those of knight, officer, and commander of the Legion of Honour; the order of Cruzaro, from the emperor of Brazil; of Saint Anne, from the emperor of Russia; of Leopold, from the king of the Belgians.

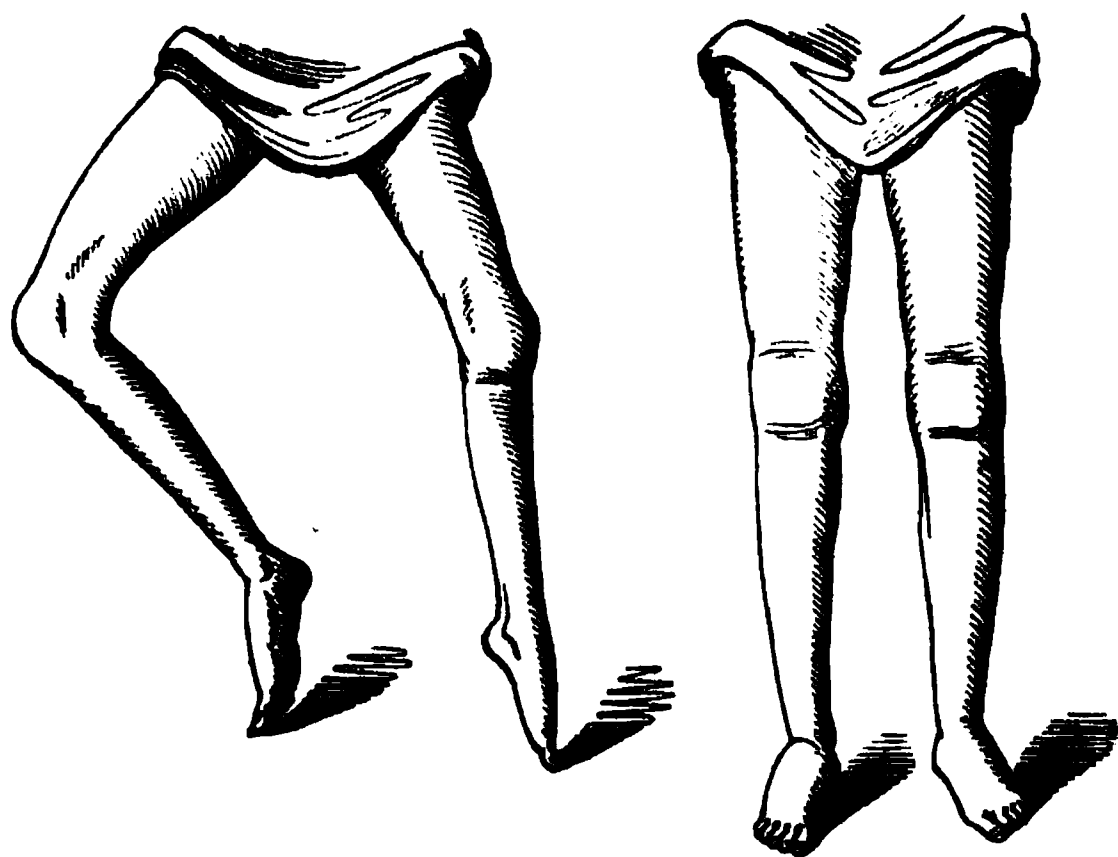
After this long course of prosperity, however, his last years were embittered by disappointment and persecution. The revolution of 1848 deprived him of the office of dean, and a strict inquiry was instituted by a commission into the affairs of the office during the period he held it. Although acquitted by the commission, he remained out of favour, and was forced to retire from his administrative appointments. He was, nevertheless, elected president of the Imperial Society of Medicine; and, concluding his career by acts of generosity, he laid aside a large part of his fortune, to the amount at least of 120,000 fr., for the foundation of prizes and donations to be awarded by the faculty, the Academy, the School of Pharmacy, and the Provident Association, etc. On the 4th of January 1853, he read this disposition to the Academy, and his death took place on the 12th of March following. Although distinguished less by the pure love of science than by the ambition of glory and applause, M. Orfila had earned his fame by discoveries of great practical importance, and a life of incessant activity; and his name is inseparably connected with the history of toxicology and legal medicine during the period in which he lived.

THE ROYAL ORTHOPÆDIC HOSPITAL.

Our attention has been directed to a handbill of this institution, and to a
NEW SERIES.—NO. XLIX. JANUARY 1854.

illustrated appendix. With regard to the handbill, its tenor and tone assimilate it to some commercial speculation, rather than to a public hospital for the relief of disease. It is certain that were any individual member of the profession to publish such a bill as is here put forth he would be scouted as a Charlatan, and why should institutions do what individuals dare not do? We are informed that "the payment of ten pounds entitles a patient to be received into an extra bed;" and from certain cases which have come to our knowledge, these "extra beds" must be a source of considerable emolument to somebody. We think this matter ought to attract the attention of the governors, and the respectable medical men attached to the hospital. We know a poor woman who, on application, was told there was no room for her lame daughter, but that, notwithstanding, she could be received on the payment of ten pounds. As this arrangement did not suit the state of her finances, she went to University College Hospital—was received there—operated upon next day—and at the end of a week walked out quite well. In another case, a gentleman in London wrote to a female in Scotland that if she wished to enter the Royal Orthopædic Hospital she would have to pay ten pounds on admission, and defray the expense of the apparatus required. The disease in this case was confirmed lateral curvature of the spine, which, in the opinion of the best surgical authorities, did not admit of beneficial treatment. Yet this institution is subscribed to as a *charity* by a long list of aldermen, members of Parliament, judges, baronets, peers, bishops and archbishops; and we are further told by the handbill that the Queen has purchased in it for his Royal Highness the Prince of Wales a perpetual right to a bed. Fortunate hospital! Happy prince!

As to the illustrated appendix, it consists of woodcut representations of wry necks, stiff arms, distorted backs, twisted thighs, knock knees, and club feet, the whole constituting a conglomeration of horrors well calculated to excite disgust or arouse sympathy. The straight limbs are placed side by side with the distorted ones, although it is not in every case easy to see which is which, or to decide as to whether the disease or the cure is most ornamental. For instance, of the limbs here represented from the "illustrated appendix," we have some difficulty in determining which we should prefer for our own use. One drawing



seems to be a study from a figurant in an attitude, and the other a sketch of universal ankylosis. Yet the "illustrated appendix" contains nearly forty of these delicate views, which some of our friends have seen lying about the drawing-room tables of the ladies they have called on. Surely at a time when so

much outcry is raised about professional proprieties, and the necessity of reform, this kind of advertising by a royal hospital, in which, too, the Prince of Wales has a permanent bed, ought to be suppressed.

HOW THEY FOUND UNIVERSITIES AT HOME AND IN THE COLONIES.

When King James the Sixth founded the University of Edinburgh in 1582, while he was a Scottish monarch, so great was the royal affection that he desired it to be called by his own name. He promised to endow it also. But, crossing the Tweed soon afterwards, he forgot his child, abandoned it to its own devices, and left it, in poverty, to the compassion of his successors. Eleven monarchs have sat on the British throne since that time; but no atonement has yet been made for the stinginess of their common progenitor.

From several private sources a miserable pittance of capital was gradually accumulated, which was lost a few years ago in the bankruptcy of the city corporation, who held it in trust-as patrons of some of the University chairs. The University was then in great extremity; for the only other source for defraying the general expenses of the institution was the common funds of the town. Parliament, however, stepped in to the aid of the city. A large sum was lent by government, at interest, and on security of the docks' revenue at Leith, for the purpose of redeeming the city debts; and in the arrangement eventually come to, the University was provided with an annual income of L.2500 *from the revenue of the Leith docks!* In this way the petty salaries of eleven professorships were restored, and the general expenses of the University, including the care and repair of extensive buildings, were scurvily provided for, at scanty cost to the government.

The same niggard parsimony has been shown in the foundation of new chairs. About the beginning of the present century, three new professorships were founded by the crown in the faculty of medicine, with an endowment of L.100 a year to each. About twenty years later, a Whig ministry improved upon the parsimony of their Tory predecessors, and founded two more medical chairs, without a farthing of endowment. More recently, a show of generosity attended the establishment of a new theological chair. But the salary attached to it was, after all, little more than what had fallen to the crown by the suppression of another professorship; and late schemes in civil polity, sanctioned by the same government, have shown that the endowment is not considered permanent, or as necessarily extending beyond the lifetime of the present holder.

At the close of the long war in 1815, there was a promise of a better state of things. The students of the College of King James the Sixth had increased to 2000; and no human ingenuity could pack so many in the pigeon-holes which served for the ancient lecture-rooms. The Government of the day, therefore, with unwonted liberality, agreed to build a new edifice, at an estimated cost of L.100,000; and a tenth of the sum was voted annually. In the end an extra sum of L.5000 was added. But there the architect was brought to a stand-still sixteen years ago—his dome unbuilt, the court-yard unpaved, and the students' reading-room, a fine hall of eighty feet in length, left in bare plaster, and even that wanting where pilasters were designed to be, and the tables constructed of rough planks, in which the archæologist still delights to recognise the primitive shelves of an extinct library. And thus do things now stand, in spite of frequent solicitations for relief. Against the building-fund may be fairly set off a sum of nearly L.40,580 which the Government has drawn from this University, since 1808, in the shape of a tax on medical knowledge—the stamp for the diploma of Doctor of Medicine. During the present year, Government has drawn L.850 from this source—being more than it has given.

On a late occasion, a desire arose on all hands to enable an aged professor to retire, after a long life spent in the service of the nation. Professor Jamieson began and finished the present magnificent Natural History Museum. He has done more than any man in Great Britain to create and diffuse in this country a taste for the cultivation of his favourite science. He has taught for a period of forty-nine years. He is a man of eighty years of age. In that long period, the

popularity of his subject might have brought him wealth, as well as fame: but he preferred to devote his fortune, as well as his life, to the advancement of natural history. Application was therefore made to Government, to enable him to retire from labours, for which his growing infirmities incapacitated him; and in September last the *Senatus Academicus* urged this measure in terms apparently irresistible. But the Minister coldly replied the other day, after three months cogitation, that Parliament had not provided any fund for the purpose. This too from a Government which imagines itself a patron of education.

Such is an example of the way to found Universities at Home. But how do they set to work in the Colonies? The Legislature of Australia determined to found a University at Sydney. AT ONCE THEY VOTED FROM THE PUBLIC REVENUE AN ANNUAL INCOME OF L.5000, AND ADDED A GIFT OF LAND OF THE VALUE OF L.150,000.

MEDICAL SECRECY IN PARIS.

The question how far a medical practitioner may be legally obliged to communicate facts, which he has learned in the course of professional duty, has for some time agitated the profession in France. There, a man who is wounded in the defence of a barricade, may be regarded as a hero or a criminal according to the political bias of his surgeon. How the latter should act has consequently become a nice question in law and morality. If a legitimist, can he conscientiously give up to justice, and by his evidence condemn, the man who has also conscientiously been endeavouring to overthrow a usurper? It is certainly difficult to reconcile the oath of Hippocrates with that of a witness. According to the former, he swears that if admitted into the confidence of a family, his eyes shall not see, nor his tongue reveal the secrets confided to him. According to the latter, he swears he will speak the truth, the whole truth, and nothing but the truth. In a severe political struggle, there may be doubts in the mind of a medical man, as to how he should act; but when anything criminal has occurred, a practitioner surely owes the same homage to the majesty of the law and of justice, as any of his fellow-citizens.

We have been led to make these observations because we find all the French journals highly commending M. Caseaux for having firmly refused to give any information with regard to a woman who was charged with criminal abortion, and who consulted him. His conduct, it is said, has led to the legal recognition of the rule, that it shall be optional on the part of a physician to communicate information to the magistrate or not, as he may judge right. But in the case of alleged criminal abortion, we think he was bound, either to exculpate or condemn the individual. But instead, he has been allowed to make the following declaration:—"I declare that I consider the facts on which I am interrogated, and which have come to my knowledge during the illness of Madame X——, as confidential facts. In consequence, I believe myself to be acting rightly in not answering the questions you think it proper to ask me." This termination of the question is considered by our *confrères* as highly honourable to M. Caseaux, and a triumphant vindication of the dignity of medicine.

THE MEDICAL FACULTY OF THE UNIVERSITY OF ATHENS.

In our last number we alluded to a lecture delivered by Professor Blackie in the University of Edinburgh, in which he maintained that Greek was in no sense a dead, but a living language, and could, of course, be much more conveniently acquired by intercourse with the living Greeks than by the usual round of scholastic appliances. We have since received from Professor Blackie a programme of the course of lectures delivered in the University of Athens, during the summer session of 1853, and the winter session of 1853-4. From this programme it appears that the professors in that institution are marshalled under four Faculties—the theological, the legal, the medical, and the philosophical,—precisely after the German model. The programme of the medical faculty is as follows:—

Special Nosology and Therapeutics five times a week. By Professor Makkas.
Obstetrics three times a week ; and Practical Obstetrics twice a week. By Professor Kostas.

Clinical Surgery five times a week. By Professors Olympios and Petsales.

Physiology five times a week. By Professor Damianos.

Clinical Medicine five times a week. By Professor Makkas.

Systematic Surgery five times a week. By Professor Olympios.

General Anatomy three times a week. By Professor Aphentoseles.

Pathological Anatomy three times a week. By Do. Do.

Hygiene four times a week ; and the History of Medicine once a week. By Professor Prinares.

Pathological Anatomy three times a week. By Mr Œginetes, a lecturer.

On Diseases of the Eye. By Professor Olympios, twice a week.

On Nervous Diseases, three times a week ; and on Syphilitic Diseases, twice a week. By Professor Benezelos.

On Comparative Anatomy and Physiology, three times a week. By Mr Busakes, lecturer.

On Toxicology, three times a week. By Professor Palles.

These lectures are for the summer session of 1853. The winter programme presents some variations, into the details of which we need not enter.

From the full course of medical education here detailed, it is clear that any young man conversant with, or desirous of learning Greek, might, without loss of time in his medical studies, pass a session at the University of Athens.

PROGRESS OF THE CHOLERA.

Since the publication of our last number, cholera has declined in frequency, everywhere in Great Britain, but still lingers in the principal places it has visited. In Edinburgh, four cases have entered the Hospital—three of which have died. In Dundee, Arbroath, Dalry, Ardrie, and a few other places in Scotland, there have also been a few cases. The disease in Ireland has been nowhere severe ; and in England the cases have greatly decreased. In Belgium there have been a small number of deaths from cholera ; but in France they have been comparatively numerous. In Paris, the visitation system has been adopted, and found to be most useful. The latest reports (20th December) is—total deaths, 616. Of these, 312 have occurred in the Parisian Hospitals ; 224 in the town, and 224 in the rural districts.

VARIETIES.

THE EDINBURGH UNIVERSITY ANATOMICAL CLASS.—The Anatomical Class in the University this session, which is ably conducted by Dr Struthers in the absence of Prof. Goodsir, is the largest which has been known there for many years, and numbers between four and five hundred students. This, though partly accounted for by the junction of the University and extra-academical classes, is also dependent on the great increase of first and second years students, to which we alluded in our last number. We are happy to announce that Professor Goodsir is rapidly recovering his health, and is expected to resume his duties next summer.

POPULATION AND MEDICAL STATISTICS OF THE UNITED STATES.—The census of the United States, for 1853, informs us, that among the population, there are of whites, 19,553,068 ; of free coloured people, 434,495 ; of slaves, 3,204,313—making a total of 23,191,876. Of the deaf and dumb, there are, whites, 9,136 ; free coloured, 136 ; slaves, 531. Of blind—whites, 7,978 ; free coloured, 429 ; slaves, 1387. Of insane—whites, 14,972 ; free coloured, 311 slaves, 327. Of idiots—whites, 14,257 ; free coloured, 348 ; slaves, 1,182.

MEDICAL REFORM.—Much speculation exists as to how far medical reform will be affected by the opinions of Lord Palmerston. Lord John Russell's parliamentary representation bill is said to be the cause of his late retirement. We sincerely hope that Lord Palmerston's opposition to that measure is not directed to the clause which gives representatives to the London and to the Northern Universities. We have been told that the graduates of Glasgow have assembled in large numbers to agitate their claims. Those of Edinburgh should not be backward at this crisis.

HONORARY INSPECTOR OF NUISANCES.—The Town-Council of Birmingham have informed the public, through the medium of advertisements, that they are prepared to receive communications from gentlemen of the medical profession, to fill the office of Medical Sanitary Inspector, which is *honorary*. A Worcestershire surgeon observes, of this degrading proposition—"It will take an acute reasoner to convince me, that any medical man is better able to give, say L.100 worth of services, for the benefit of hundreds of wealthy merchants—than that hundreds of wealthy merchants are able to contribute, say L.100 for his services."—*Med. Times and Gaz.* The system of gratuitous medical attendance is gradually becoming so great an evil, that it must ultimately work its own cure.

NEW SCIENTIFIC BIOGRAPHIES. NEWTON AND DALTON.—Two biographies, for which the world has long and anxiously waited, are about at length to be published. Sir David Brewster has in the press *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, drawn up from very full materials, including manuscripts of great interest, hitherto unpublished.

Dr William Charles Henry, formerly of Manchester, has nearly completed a *Life of Dr John Dalton*, which will be published early in next year by the Cavendish Society. Dr Henry was a favourite pupil and attached friend of Dalton's, who named him his literary executor, and left him by will his papers. Dalton died in 1844, but owing to the fault of another of his executors, the papers bequeathed to Dr Henry were not delivered to him till last summer, since which he has lost no time in carrying out his task as Dalton's biographer. It is due to Dr Henry to mention these circumstances, for he has generally been supposed to have received the Dalton MSS. in 1844, and has been unjustly blamed for delaying to publish a life, which he had not, in truth, till recently the means of writing. From what we know of Dr Henry, and of the peculiar advantages which he enjoyed for knowing Dalton, we are assured that the forthcoming life will be a most welcome addition to our too scanty stock of scientific biographies.

THE PROJECTED STATUE TO DESAULT.—An attempt to erect a statue to the celebrated surgeon Desault was made by the Town of Lure, in Franche Comté, where he was born. Subscription papers were distributed throughout France. A great folio book was opened at the office of the Gazette Médicale in Paris, to receive contributions, but only one person inserted his name. This was a Monsieur A. L. Boblet, and his subscription amounted to two francs.—*Gaz. Médicale.* All honour to M. Boblet!

MEDICAL VERBOSITY.—The following observations by Dr Samuel Jackson made at the Philadelphia County Medical Society are worth preserving:—"In a celebrated medical journal, we have this circuitous way of saying that a certain medicine was probably useful in rheumatism: 'the disease was cured in eleven days; and lemon juice, if it was not the principal remedy, certainly exerted an important influence toward the production of that end.' What think you, gentlemen, of *producing* or *leading forward* an end or a cure? One might suppose that the writer was a cobbler, and that he was talking about the *producing* or the *pulling forward* of his waxed-end. And then he has lemon-juice *making an exertion* and *exerting an influence*.

"Why should a writer say, 'I had recourse to a medicine,' if he had not previously used it in the same disease? This word means a running backward. The simple English word *to give*, is often supplanted by the Latin word *to exhibit*; that is, to make a show of the medicine. A shopkeeper *exhibits* his goods, a physician *gives* or *orders* his medicine. Celsus took nearly all his ideas from the Greeks, but he did not copy their words. I believe he never uses the word *exhibere*, but *dare et uti*. Sometimes he says *adhibere*, but this does not mean *to make a show*; moreover, it is pure Latin. His own language was sufficient for him, except in the mere naming of diseases; and hence one reason that his style and manner are universally approved."—*Boston Journal*, p. 452.

MATRICULATIONS OF MEDICAL STUDENTS IN PARIS.—The number of Students inscribed in the Faculty of Medicine in Paris this year amounts to 1065, of whom 287 are new. Last year the students numbered 1434, of whom 480 were new. This great diminution is attributed to the difficulties of many families in the country, dependent on the insufficiency and bad quality of the harvest.—*Gaz. des Hopitaux*.

SMALL-POX IN THE SANDWICH ISLANDS.—The Sandwich Islanders are apparently being exterminated by the small-pox. At the end of last July, there had been 2886 cases and 1027 deaths.—*Gaz. Médicale*.

PRIZE FOR THE CURE OF CHOLERA.—The Academy of Sciences of Paris has received the legacy of 100,000 francs (L.4000) left it by a generous philanthropist, M. Bréant, as a recompense for him who shall discover a remedy for cholera. As this is not likely to be soon awarded, it is proposed to give the interest (from L.150 to L.200) annually for the most useful discovery connected with cholera. During the first Empire, a similar sum was left for the curer of croup, which has not yet been awarded. The new legacy will only serve to swell that large sum which the Academy annually distributes in the form of prizes.—*Gaz. Médicale*.

SURGEONS IN AMERICA PROTECTED BY CONTRACT.—Dr Josiah Crosby, an eminent surgeon of Manchester, N. H., has refused to dress a fractured limb unless the patient would place himself under bonds not to prosecute in the event the limb should not be perfect. This is the only safe course to be followed. The rage for obtaining money from surgeons in the United States for unsuccessful cases, is only paralleled by the suits against railroad companies. Juries invariably assess heavy damages against both, whenever an opportunity occurs. A professor of surgery in one of the oldest and most respectable schools of medicine in New England, has recently been assessed eight hundred dollars by a jury, for alleged mal-practice. Similar cases are of late becoming so common, that blank forms of bonds should be kept on sale, ready to be executed before a justice of the peace, whenever an individual sends for a surgeon.—*Boston Journal*, p. 465.

DEVOTION OF FRENCH MEDICAL STUDENTS.—A notice having been placed in the court of the 'Ecole de Médecine of Paris, that students might be called upon to assist the authorities during the approaching epidemic of cholera, no less than 500 inscribed their names the first day, as ready for that purpose.—*Gaz. des Hopitaux*. We may observe, that the Edinburgh medical students have also eagerly supplied extra assistance to numerous towns in Scotland and the north of England, where their services have been required.

PERPETUAL THIRST.—A Mr James Webb of Fairhaven, Massachusetts, from early infancy consumed a surprising quantity of water. At present, three gallons of water is rather a short daily allowance for him, and it would be impossible, it seems, for him to live through a night with less than a pailful.

With this immense amount of cold water daily poured into the stomach, Mr Webb is now 58 years old, and is in good health and spirits.—*Boston Journal*. Does our contemporary give this as an authentic fact, or is it what we understand in this country by an Americanism?

PUBLICATIONS RECEIVED.

- Hand-Book of Chemistry, Theoretical, Practical and Technical.** By F. A. Abel, and C. L. Bloxham, with a preface by Dr Hofman. London. 1854. 8vo. Pp. 724.
- The Pathology and Treatment of Stricture of the Urethra, both in the Male and Female.** By Henry Thompson, F.R.C.S., M.B., Lond., etc. London. 1854. 8vo. Pp. 424.
- On Fatty Degeneration.** By the late W. F. Barlow, F.R.C.S. London. 1853. Small 8vo. Pp. 92.
- Surgical Anatomy.** By Joseph Maclise, F.R.C.S. Second Edition. Fasciculi I. and II. Folio.
- Some Remarks on the Medical Evidence given at the trial of William Burke Kirwan.** By John L. Field, M.D. With a Commentary thereon by Frederic C. Skey, F.R.S. London. 1853. 8vo. Pp. 15.
- Third Annual Report of the Medical Superintendent of the Manchester Royal Lunatic Hospital.** Manchester. 1853. 8vo. Pp. 32.
- Remarks on Hysteria in connection with Hydrophobia, and other Convulsive Affections.** By John Dalziel, M.D. Edinburgh. 1853. 8vo. Pp. 32.
- De la Cataracte, Mémoire couronné par l'institut médical de Valence (Espagne).** Par J. Leport. Paris. 1852. Small 8vo. Pp. 48.
- Small Pox and Alcohol two Poisons: with Remarks on the Registrar-General's Reports of Deaths occasioned by them.** By John Chadwick, M.D., Bury. 12mo. Pp. 11.
- On the Local Application of the Vapour of Chloroform, etc.** By S. L. Hardy, M.D. Dublin. 1853. 8vo. Pp. 15.
- Additional Experiments on the Excitability of Paralysed and Healthy Limbs by the Galvanic Current.** By R. B. Todd, M.D., F.R.S., etc. London. 1853. 8vo. Pp. 23.
- On the relation between Therapeutics and Pathology, with especial reference to some forms of Cardiac, Renal, and Nervous Diseases.** By George Johnson, M.D., etc. London. 1853. Small 8vo. Pp. 113.
- Observations on the Muscular Tissue of the Skin, and on the Contractile Tissue of the Iris.** By Joseph Lister, M.B. London.
- The Irish School of Medicine as it is, and as it ought to be. An Address, etc., by Thomas S. Holland, M.D.** Cork. 1853. 8vo. Pp. 23.
- Clinical Lectures on Pulmonary Consumption.** By Theophilus Thompson, M.D., F.R.S., etc. London. 1854. 8vo. Pp. 211.
- The Anatomy and Diseases of the Prostrate Gland.** By John Adams, Surgeon to the London Hospital, etc. Second Edition. London. 1853. 8vo. Pp. 178.
- The Nature of Cholera Investigated, with a Supplementary Chapter on Treatment, etc.** By John George French, F.R.C.S., etc. Second Edition. London. 1854. 8vo. Pp. 152.
- The Diseases of the Heart and the Aorta.** By William Stokes, Regius Professor of Physick in the University of Dublin, etc., etc. Dublin. 1854. 8vo. Pp. 689.
- Veterinary Medicines, their Actions and Uses.** By Finlay Dun, V.S., etc. Edinburgh. 1854. Small 8vo. Pp. 412.
- On the Educational Uses of Museums.** By Edward Forbes, F.R.S., etc. London. 1853. Royal 8vo. Pp. 19.
- Hooper's Physician's Vade Mecum.** Fourth Edition. By Dr Guy of King's College, London. 1854. Small 8vo.
- Druitt's Surgeon's Vade Mecum. A Manual of Modern Surgery.** Sixth Edition. London. Foolsap.
- Band of Hope Almanac.** 1854.

TO CORRESPONDENTS.

The papers of Drs Pinkerton and Lownds will be inserted next month.
Mr Davies case has been received.

We are compelled to delay the list of exchange Journals until our February number. The last number of the Dublin Quarterly Journal has not come to hand.

In consequence of several heavy postages having been paid on isolated American Journals, our publishers have received strict injunctions to refuse all communications whatever that are not prepaid.

It will be our invariable rule to acknowledge all the publications received during the month, so that publishers and donors of books may, from our list, ascertain whether they have come to hand.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Contributions to Obstetric Pathology and Practice.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—(Continued from last Vol., p. 50.)

NO. XIII. CASES OF TETANUS FOLLOWING LESIONS OF THE UTERUS, ABORTION, AND PARTURITION.

DR ROBERT REID, in his work "On the Nature and Treatment of Tetanus, etc.," states that "injuries or derangements of *internal organs* do not appear to have much tendency to produce this affection. Indeed (he adds), I have not been able as yet to find any case recorded of true tetanus which arose from *internal injury*, whether in the alimentary canal or elsewhere."—(p. 52.)

The uterus is certainly an "internal organ," in the sense in which that expression is used by Dr Reid; and internal injuries or lesions of it, both in the unimpregnated and puerperal states are, without doubt, sometimes followed by tetanus in an acute and fatal form. This fearful complication is fortunately a rare occurrence in midwifery practice. But the following series of cases is calculated to show that traumatic tetanus does supervene occasionally as a secondary obstetrical disease, in the same way as all medical authorities acknowledge it to supervene occasionally—and still more frequently—as a secondary surgical disease.

Before, however, detailing the proofs which I wish to adduce of this last remark, let me premise that in ancient times tetanus was usually considered and described (for example, by Aretaeus) as a disease more common among females than males. Modern statistics, however, have amply shown the reverse to be true.

Out of 128 cases of traumatic tetanus collected by Mr Curling, 112 were males, and 16 only were females.¹ Out of 221 cases collected in an excellent paper by Professor Lawrie of Glasgow, 185 of the patients belonged to the male sex, and 36 to the female sex.²

The Registrar-General's official Reports afford a still greater amount of evidence on the relative liability of the two sexes to

¹ Treatise on Tetanus. Table in Appendix.

² Glasgow Medical Journal for October 1853, p. 352.

tetanus. The number of fatal cases of tetanus which have occurred annually in England and Wales during the last fifteen or sixteen years has varied from about 120 to 150; in 1847 as many as 165 died of this disease. The number of deaths from tetanus, etc., has, unfortunately, not been published for every year of the Reports; in some of the years those occurring in the metropolis are not given; and occasionally the data as to the sexes of the patients cannot be discovered in the official returns. But from 1837 to 1842 I find 629 deaths from tetanus entered in the reports, with the numbers of each sex stated; and from 1847 to 1849 there are 430 deaths from tetanus similarly specified. Out of these 1069 fatal cases of tetanus, 829 belonged to the male sex, and 240 to the female sex. In other words, out of every 100 of these cases 77 per cent. were males, and 23 per cent. females. Or the males attacked with fatal tetanus were to the females in the proportion of nearly 4 to 1.

In how many of the above 240 instances of fatal tetanus in the female sex, the uterus was the seat of traumatic irritation or injury we have no means of judging. Doubtless, the proportion was but small. The following cases, however, will tend to prove that this dreadful malady may follow—(1) lesions of the unimpregnated uterus; as well as the lesions left in the uterus and maternal canals—(2) by abortion; and (3) by parturition at the full time:—

I.—TETANUS AFTER LESION OF THE UNIMPREGNATED UTERUS.

Lesions and injuries of the unimpregnated uterus seem to have little tendency to produce traumatic tetanus. But the following instance affords a remarkable example of this formidable complication:—

CASE I.—A widow, æt. 35, who had been suffering for many months under severe menorrhagia, came from a considerable distance for advice. The uterus was large and heavy, and evidently contained some roundish morbid mass either in its cavity or in its walls. The use, for a few hours, of a single sponge tent opened up the os uteri, and allowed a polypus to be felt in the interior of the uterus. Menstruation supervened in a profuse degree; and any further local treatment was in the meantime suspended. In a few days uterine contractions came on. They were severe in character, and gradually pushed down a large cellular polypus from the uterus into the vagina. No special operation was required to detach it, as the expulsive efforts of the uterus had, in a great measure, spontaneously broken up the adhesions of the tumor with the interior of the uterine cavity; and at last the polypous mass was found so loose as to be easily removed from the vagina by slight traction with the fingers. It proved to be a cellular or loose-fibrous polypus, of the size of the closed fist. After the separation of this mass the patient

felt comparatively well. She was free from fever and local pain, and appeared for a time quite convalescent. On the ninth evening, however, after the polypus was detached, she sent for me, to complain of some stiff and strange feelings about her face. At the time of my visit she was sitting up drinking tea. She herself remarked that she was almost unable to swallow it, from pain and difficulty in opening her mouth, which she had first noticed a few hours previously. My suspicions of tetanus were not at the time excited. I believed, with the patient herself, that she was threatened with an attack of cynanche. But early next morning all the symptoms of the disease were becoming far too marked to leave any doubts of its nature. The muscles of the neck were affected with tetanic spasms, and the jaws fixed. Deglutition was impossible. During the day, Professor Syme saw the patient along with me. In despite of all the usual remedies, the tetanic paroxysms increased in extent, in frequency, and in severity. She bore the terrible spasms with wonderful equanimity, and her mind seemed to remain entire up to the very last. She died, exhausted by the disease, about fifty-five hours after the first symptoms of tetanus were observed.

On making a post-mortem examination, no special lesions were found in any organ examined, with the exception of the uterus. And there the principal, or, indeed, only morbid appearance, consisted of the shaggy and projecting attachment of the uterine extremity of the ruptured pedicle of the polypus, marking the spot where it had adhered to the interior of the body of the uterus.

I do not remember of having heard or read of any analogous case in which a lesion of the unimpregnated uterus was followed by traumatic tetanus. Mr Curling refers to an American case, recorded by Dr Smart, in which the disease was supposed to have been produced by an injury of the neighbouring mucous canals,—viz., “by the passage of large, rough, angular pieces of clay from the intestinal canal into the vagina.”¹ The case, however, is not an example exactly in point. A French author of the sixteenth century, Martinus Akakia,² states—apparently in the way of a general observation, from one or two individual cases—the occurrence of trismus, general tetanus, and opisthotonos, running on to a fatal termination by the fourth day, as an occasional sequence of menorrhagia, whether that menorrhagia, or hemorrhage, had been the result of diseased states of the unimpregnated uterus, or of abortion, and delivery.

II.—TETANUS AFTER ABORTION.

Several of our oldest medical authorities describe abortion as one

¹ Treatise on Tetanus. Footnote, p. 197.

² De Morbis Mulieribus. Lib. I., cap. 6, “De fluxu rubro.” See Spachius’ Gynæciorum Libri, p. 759.

of the occasional causes of tetanus. "Individuals (observes Archigenes) are attacked with this disease in consequence of various causes, as from wounds, and in women from abortion (*in fœminis ex abortu*)."¹ When speaking of the causes of tetanus, Aretæus specially enumerates wounds of all kinds, with exposure to cold, and he adds that "women are sometimes, though rarely, attacked with the disease as a result of abortion."²

In none of our modern obstetric books, nor in any of the various essays or works devoted to the consideration of abortion or puerperal diseases, is any allusion made, as far as I know, to the possible supervention of tetanus after miscarriage. It is a complication, however, which does occasionally take place; and it is always one very formidable in its character, and generally very fatal in its issue. Two deaths from tetanus after abortion, occurred some years ago, almost simultaneously, in Edinburgh. I have the kind permission of Dr Alexander Wood, and Dr Malcolm, in whose practice these cases occurred, to state the following particulars regarding them :³—

CASE II.—A lady, aged about 36, and the mother of six children, after having had the catamenia obstructed for three months, was attacked on 16th Nov. 1845, with symptoms of abortion. On the 17th the os uteri was open, and large coagula were expelled. In three or four days she was so far recovered as to require no more medical care. On Nov. 23d the patient suffered under some degree of sore throat and stiffness of the jaws. Early next morning, when Dr Wood visited her, he found the rigidity of the jaws so considerable, and the difficulty of opening the mouth so great, that it was impossible to get an inspection of the throat. The pulse was 80, soft and compressible. During the day the stiffness of the jaws increased; acute pain of the spine was complained of; the abdominal muscles were rigid; and swallowing became difficult. General tetanic spasms supervened in the evening, and notwithstanding the employment of turpentine, tobacco, Indian hemp, etc., the disease continued to increase; and she died on the evening of the 26th.

CASE III.—In this case the tetanic attack was still longer in supervening after the abortion. Nearly a fortnight after an early miscarriage had been thrown off, and when the patient was considered well, she was suddenly attacked with symptoms apparently of cynanche parotidea. On the second day Dr Malcolm found his patient unable to open the jaws. On the third day the symptoms were all much aggravated; and on the fourth day she was seized with general tetanic spasms, which recurred again in two hours with in-

¹ See Aetius' Tetrabibl. II., sec. ii., cap. 39.

² De Causis Acutorum Morborum. Lib. I., cap. 6; Vol. I., p. 2; of the Latin edition in the "Medicæ Artis Principes."

³ Notes of these two cases have already appeared in the published Proceedings of the Obstetrical Society.

creased violence, and then proved fatal in the course of a few minutes. Dr J. Duncan, and one or two other medical gentlemen saw this lady in the first of these general tetanic paroxysms, during Dr Malcolm's unavoidable absence at an obstetric case.

In the following instance the ovum was at least partially separated, as shown by the attendant hæmorrhage and by a segment of it being felt protruding through the os uteri. In all probability it passed—as ova in early abortions sometimes do—when the bowels or bladder were evacuated, and unobserved by the patient's immediate attendants. If not, the case must be reckoned one of fatal tetanus, with the ovum partially separated, but not expelled. I have been favoured with the notes of it by my friend, Dr Hislop of East Linton, in whose practice the case occurred.

CASE IV.—The mother of a family, who had never aborted previously, had not menstruated for nine weeks, when she was seized with a considerable loss of blood from the vagina, which left the pulse quick and feeble. The bleeding soon ceased, but next morning Dr Hislop found the pulse at 120, the os uteri relaxed and open, and a soft mass protruding through it. He introduced a sponge into the vagina to restrain the hæmorrhage, which had recurred to a slight extent, and gave the patient some small doses of ergot of rye, to expel the ovum. The membranes were not observed in the vaginal discharge, but the convalescence of the patient went on satisfactorily, till six days subsequently when she first complained of "weakness of the lower jaw." Next morning the jaw felt stiff, and could not be opened more than half. The patient herself became anxious and alarmed. In the evening any attempt at swallowing produced a sensation of choking; and there was a copious secretion of viscid saliva. On the following day the jaw was still more fixed, swallowing became impossible, tetanic spasm began to affect the back of the neck, and subsequently they stretched to the chest, back, and extremities. By night the opisthotonos was complete, the spasms increased in severity, the pulse rose to 160, her mind, however, remaining quite clear; and she expired next morning, or on the third day after the tetanic symptoms began. The treatment principally consisted of large opiates, interrupted during the second day of the attack, till the bowels were acted on by a dose of croton oil, and by turpentine enemata.

Tetanus, when it has taken place in obstetrical practice, has apparently been repeatedly mistaken, in its earliest stages, for an attack of cynanche (as in the first case I have detailed); or even oftener, perhaps, for some irregular form of hysteria. It is only after the tonic spasms have passed to other muscles than those of the jaw and neck, that the actual presence of such a fearful and fatal malady has in general been fully realised by the mind of the medical practitioner. The

notes of the following case, obtained for me by Dr Symonds, and drawn up by a medical gentleman, a near relative of the patient, afford an instance in point. The case also offers another example of the fact, that amid the mortal and agonizing struggles of the patient, the reason and intellect often remain clear and unclouded up to the last fatal spasm,—the disease being originally and truly an affection of the reflex spinal system and not of the brain.

CASE V.—A lady, aged 41, of a delicate and nervous temperament, and the mother of six children, miscarried at an early period of pregnancy (17th Nov. 1849). So much hemorrhage occurred that it was necessary to use the tampon. During the following week she suffered severely from the effects of loss of blood, and was especially troubled with palpitation, headache, and a sensation in the throat resembling the hysterical globus. On the morning of Nov. 25th, or seven days subsequent to abortion, she experienced a difficulty in deglutition and a stiffness in the jaws and neck, which gradually increased and became paroxysmal in their character, leaving eventually no doubt of the existence of true tetanus, though it had been hoped previously that hysteria, with the addition of “a stiff neck” from catching cold, would explain all. The existence of complete trismus subsequently formed a leading feature throughout. Deglutition, too, was much impeded at an early period, and soon became almost impracticable. The muscles of the neck, chest, and abdomen, were fully affected with the tetanic spasms; but not those of the back or extremities. Towards the termination of the case the larynx became involved, and the paroxysms of strangulation, several of which occurred before death, were agonizing in the extreme. The spasmodic exacerbations, irrespective of these laryngeal paroxysms, were very severe. About sixty hours from the commencement of the tetanic symptoms, death relieved the poor patient from her sufferings in a paroxysm of strangulation. The mind and reason were unaffected to the very last.

There is a case of fatal tetanus after abortion, mentioned by Velpeau in his *Essay on Puerperal Convulsions* (Observation 21, p. 232) where the first symptoms of the disease were overlooked as unimportant. The details given by Velpeau are as follows:—

CASE VI.—At the commencement of the year 1834, a young woman entered as a patient at the Hôpital Cochin, in consequence of a miscarriage. After some little time she was seized with trismus, to which little attention was paid. It was thought even that she complained without reason, and that she suffered less than she professed. A change in the service about this time occurring, she was in some sort forgotten. Nevertheless, these convulsive movements spread little by little, although slowly, to all the other parts of the body. Several bleedings were employed, but nothing was

able to arrest the march of these accidents, and the unhappy woman died in a complete state of opisthotonos. No manifest lesion was found upon the dead body. The uterus, the brain, the spinal cord, and the viscera, appeared in a normal condition.

The symptoms of tetanus are liable to considerable variations in different cases. The following case of fatal tetanus connected with abortion, for the notes of which I am indebted to a very accurate observer, Dr Ritchie of Glasgow, is remarkable as one of the rare instances in which the spasmodic paroxysms affected, during one period of the disease, the muscles that open the lower jaw, instead of (as usual) the muscles that close it.

CASE VII.—Mrs ———, aged 40 ; the mother of seven children ; eccentric and nervous ; when in the third month of pregnancy was exposed to sudden mental agitation. On the instant she had a flow of blood from the vagina. This ceased on lying down, when the os uteri was found shut but the cervix bulky. Next day, on removing a tampon which had been left at the previous visit in the vagina, it was found that the os uteri could easily admit the finger, that the membranes were tense, and that there was no bleeding. The plug was re-introduced, and, soon afterwards, the membranes gave way, the rupture being accompanied by a loud noise and a discharge of watery blood and some clots. The tampon was again had recourse to, but about ten in the evening, much suffering being caused by it, the patient withdrew it. This was succeeded by profuse hemorrhage and alarming faintings till two A.M. There was no evidence of the expulsion of the ovum excepting the appearance of some shreds of decidua membrane, although it was possible that such might have been passed unseen in the clots.

From this day the discharge ceased, the os uteri closed, and there was every symptom of convalescence except that the neck of the womb continued to be bulky. Some clots passed from the vagina on the 11th day ; and next day, while about to chastise a child, she was seized with locked jaw, succeeded by a sense of suffocation in the throat, and, two days after, by tetanic spasms or cramps in the muscles of the neck and back, and by acute shooting pains in the articulations of the lower jaw.

She was put on a course of calomel and opium with croton oil. The mouth became sore on the 3d day of the tetanic attack, and dysentery also set in, apparently from the oil. The spasms of the neck and face continued, although rather less intensely. Some fetid clots escaped from the uterus. She was now put on 25 drops of laudanum every three hours, the former medicine being omitted. On the 4th and 5th day of the disease the tetanic spasms ceased, and the dysentery also had disappeared. The opium was discontinued.

But during the course of the 6th day after the commencement of

the tetanus, she was again accidentally excited, and in consequence seized with general rigidity of the muscles of the body. The mouth was thrown violently and widely open, the face became livid, spasmodic movements of the limbs succeeded, with complete insensibility, lasting for about half an hour. These paroxysms were succeeded by intervals in which the jaw became firmly closed, and she was affected by convulsive jerking movements, occasioned apparently by contraction of the extensor muscles of the back.

During the ensuing twenty-four hours she suffered from about four or five of the first mentioned seizures; and in the intervals her head was violently drawn backwards every few minutes, occasioning a sensation at the throat as if she were about to be strangled. The severe fits usually began with a sense of constriction at the chest and of a want of air. She died at last in one of these fits, on the evening of the 7th day from the first commencement of the tetanic symptoms.

In some of the preceding instances of tetanus following early miscarriage, there was nothing particular in the phenomena of the abortion, and nothing special in the treatment which the cases required. In fact, in several of the examples adduced (as in Nos. II., III., and VI.), no medical or obstetric treatment seems to have been called for during or after the miscarriages—the cases being in all respects simple and uncomplicated. But in others (as in Nos. IV., V., and VII.), there was one special complication present, viz., hemorrhage,—and one special and effective mode of arresting it adopted, viz., plugging of the vagina. I do not allude to these instances as any certain evidence that either this special symptom or special treatment were connected, in the way of causation, with the subsequent occurrence of the tetanus. The vagina is daily plugged by obstetric practitioners to restrain hemorrhage, both from the unimpregnated and pregnant uterus, without any such evil consequences following; but we have so far a certain amount of proof, in these instances, that tetanus—after abortion—is more particularly liable to supervene, either where the uterine surface is in such a state of injury and lesion as to lead to the protracted continuance of hemorrhage, or, where the maternal canals have been irritated by the tampon, when used as a mechanical means to diminish and arrest that hemorrhage. Dr Adams of Lanark has furnished us with notes of a case of fatal tetanus after abortion that occurred in his practice when he was settled in Glasgow, and which affords an illustration of the disease supervening upon the arrestment of the attendant degree of puerperal flooding. The patient was, in Dr Adams' absence, attended by Mr Crossken, who has drawn up notes of the symptoms and post-mortem examination.

CASE VIII.—Mrs —— the mother of several children, had a miscarriage at the third month. A slight degree of hemorrhage

followed. On the eighth day after the abortion, this discharge suddenly ceased, and a feeling of stiffness soon after supervened in the masseter muscles. Next day the jaws were quite locked, and the head was bent backwards with tetanic spasms, the muscles of the jaw and neck being fixed and rigid. The eye-balls were also sunk and the eyelids partially closed. The patient, however, was able to speak; but the deglutition of fluids was impossible. The pulse was 72; the bowels constipated and flatulent; and the urinary secretion natural. There was no discharge from the uterus, and no uneasiness or pain in that region. The pulse betimes became weaker, and the tetanic paroxysms more and more severe and frequent; and she sank and died in about seventy hours after the appearance of the first symptoms of tetanus.

The body was examined by Mr Crossken and Dr Fleming about thirty-six hours after death, and as the morbid appearances were in some respects peculiar, I will state them in Mr Crossken's own words. "The uterus," he reports, "was about the ordinary size. Its substance and internal lining membrane were emphysematous throughout, full of air vesicles, and crepitating under the fingers. In fact it was like a piece of lung, and resembled it also by floating in water. There was, however," he adds, "no appearance whatever of decomposition."

In surgical pathology inquiries have been repeatedly instituted, with the view of ascertaining if there was any kind of settled connection between the existing state of the wound and the occurrence of tetanus, but without much success. For surgeons seem generally agreed upon the fact, that while the tetanic disease very frequently supervenes when the external wound appears in all respects perfectly healthy, in about an equal proportion of other instances, it comes on when the wound is unhealthy, or inflamed, or sloughing. In some of the preceding examples of obstetrical tetanus supervening after miscarriage, the lesion or wound, left on the interior of the uterus by the abortion, seems to have been in a "healthy state," as far as could be ascertained; in others it was so far unhealthy, as to have been a source of morbid sanguineous oozing and hemorrhage. In none of the examples of obstetrical tetanus, included in this or in the next division, does there appear to have been any tendency to inflammation of the uterus, as an antecedent to the occurrence of tetanus.

III.—TETANUS AFTER PARTURITION.

Puerperal tetanus, when following parturition at or near the full time of pregnancy, seems to be guided, in regard to the period of its supervention, by the same laws as regulate the occurrence of the

disease after abortion, or after surgical operations and injuries. Under all of these conditions the tetanic attack usually does not commence till about a week after the occurrence of the exciting obstetrical or surgical lesion. According to some statistics published by Romberg,¹ in more than a half of all instances of surgical tetanus—or in 112 out of 208 cases collected by him—the attacks set in between the 3d and 10th days after the receipt of the injury, or the occurrence of the operation. The same period includes the dates at which the tetanic seizure usually took place in the examples of obstetrical tetanus which I have already detailed, as well as in those which I have collected under the present head. In the following instance, which occurred in the practice of my friend, Dr Lyall of Dundee, the tetanic symptoms began on the sixth day after delivery at the full time, and when there was apparently nothing unusual in the labour itself, or in the puerperal state of the patient, to excite the disease.

CASE IX.—A patient, aged 25, was delivered of her first child at the full term of utero-gestation, without any unusual complication, the labour being easy, and only of seven hours' duration. On the sixth day after her accouchement, stiffness of the muscles of the back came on, followed by symptoms of trismus, which daily increased. On the third day of the attack, on attempting to swallow, strong opisthotonos at length supervened, the head being drawn back between the shoulders. The tetanic symptoms went on increasing, till the patient sunk on the sixth day of the disease (the twelfth from delivery). The treatment consisted principally of large opiates; but they failed either in procuring sleep or in relieving the tetanic spasms. Venesection also, and turpentine in the form of enema, were tried. The child (a female) was living, and afterwards thrived well. "There was (adds Dr Lyall) no perineal laceration or other injury to be ascertained."

In the following case of puerperal tetanus, reported to me by Dr Lever of Guy's Hospital, in addition to the usual lesions left by delivery in the interior of the uterus, there existed also, as possible traumatic causes of the disease, lesions or injuries of the maternal passages and perineum:—

CASE X.—A mother, about seventy-two hours after delivery, was attacked with tetanus. The disease presented all its most characteristic symptoms. In this patient laceration of the vagina and perineum had occurred during the process of labour. Opium was given in large quantities; but the disease proved fatal in three days. The child also died, affected with trismus and jaundice.

I have already alluded to the circumstance that an unusual de-

¹ Manual of Nervous Diseases, vol. ii., p. 105.

gree of hemorrhage, and the use (sometimes prolonged) of the vaginal plug, seem to have been precedents to a considerable number of the instances in which tetanus appeared in connection with early abortion. In the two following cases of tetanus after parturition at the full time, the same or similar causes were also antecedent to the occurrence of the fatal attacks of this disease. For the notes of the first example I am indebted to Dr Mackinlay of Barrhead; the second has already been placed on record by Dr Storrer of Boston.

CASE XI.—A mother, aged 27, was delivered, at the full time of pregnancy, of her second child, on the 2d February 1852. She made for three or four week a fair recovery; but suddenly after some physical over-exertion and much mental disquietude, she was attacked with secondary uterine hemorrhage on the 27th February. The flooding was so great as to require the vagina to be plugged by a surgeon, who was called in. He left the plug in the vagina for two or three days. On the 5th March, stiffness about the face and neck came on. Early next morning the trismus was very marked; and she continued to show, in an increasing degree, all the characteristic symptoms of tetanus, up to the 8th March, when death put an end to her sufferings. The tetanic spasms principally showed the opisthotonic form.

CASE XII.—Mrs C——, aged 28, and the mother of two children, was delivered at the full time of an infant that weighed eight pounds. The umbilical cord broke off near its origin in endeavouring to extract the placenta. After some unsuccessful attempts to detach the afterbirth, it was considered proper to desist from further efforts. The attendant hemorrhage was slight. During the five following days the pulse remained good, and the patient free from fever or uterine pain. Towards the commencement of the sixth, a fragment of placenta was removed from the vagina, and after the use of ergot two other placental masses were expelled,—decomposing and offensive in smell. On the seventh day, the pulse was, for the first time, above 100, small and wiry; and the patient complained of pain in the head, considerable stiffness of the jaws, and a difficulty of swallowing. These symptoms rapidly increased during the day, and at night the tip of the tongue could scarcely be protruded between the teeth. The muscles of the neck and jaws had also become much more painful; the respiration was laborious; and, at irregular intervals, tetanic spasms were present. Next day (the eighth after the birth of the child), the muscles of the face were so rigid, that the jaws could not be separated in the slightest degree. The merest touch seemed to distress the patient, and to hasten on the spasmodic attacks, which recurred every few minutes. The head was retroverted upon the pillow; and so firmly contracted were the muscles of the neck, that when the hand was placed behind her occiput, the whole body was brought forward, the neck not being flexed in the

slightest degree. When the spasms were present, the patient's sufferings appeared to be extreme. The paroxysms increased in frequency until about midnight of this—the eighth day after parturition—when she sunk, exhausted by opisthotonos. Throughout, there were not any symptoms of uterine or peritoneal inflammation.¹

External injuries and lesions form certainly, the most common sources of tetanus. And next in frequency as an exciting cause of the disease, authors usually reckon exposure to cold, or rather to currents of cold and damp air—especially if the person has immediately before this exposure, been overheated or perspiring. Wounds may lead to tetanus without any exposure to cold; and exposure to cold sometimes leads to tetanus, without the presence in the body of any appreciable lesion. But though these two sources of tetanus may each confessedly lead to the production of the disease quite independently of the other, their *combined* action far more frequently leads to this result. Most of our surgical authorities, who have seen much of tetanus in practice, agree in stating that in surgical subjects the disease has very often been observed to occur immediately after those bearing surgical lesions upon their body have been accidentally exposed to cold currents, or to sudden changes of temperature. The same fact seems also to hold good with regard to puerperal tetanus. In one of the cases already detailed (Case II.), Dr Wood found that, a few hours before the patient was attacked with tetanus, she had thrown her window open, and dressed her hair, exposed to the draught of cold air from without. In the three following cases of puerperal tetanus, all of which occurred in the practice of one physician (Dr Aubinais of Nantes²), sudden exposure to cold seems to have been instrumental in lighting up the tetanic attack.

CASE XII.—In a primiparous patient, the milk-fever was high on the fourth day, when the woman (æ. 30) crossed a wet floor, with her feet naked, and while her skin was covered with perspiration. She was speedily seized with prolonged shivering, abdominal pain, and suppression of the lochia; and gradually the tetanic condition, commencing with difficult deglutition, markedly prevailed, inducing a slight degree of emprosthotonos. She died seven days after the tetanic symptoms began.

CASE XIII.—A woman (æ. 34), while in a state of perspiration, exposed herself to wet and cold four days after her confinement, with the effect of suppressing the lochia. Tetanic symptoms appeared twenty-four hours subsequently, and soon became strongly developed. The disease obstinately resisted the most active treatment until the tenth day, when it began to yield, and the patient ultimately recover-

¹ American Journal of Medical Science, for January 1842.—P. 97.

² Revue Medico-Chirurgicale. Tom. V., p. 149. British and Foreign Medical Review, for July 1849, p. 296.

ed. She was bled six times, 100 leeches were applied, and musk and valerian were afterwards given.

CASE XIV.—A poor woman, æt. 28, having suffered from an attack of eclampsia, for which she was bled, was delivered a few days afterwards of a seven months' child. She went on very well until the fifth day, when, having drank a large quantity of cold water, she was seized with shivering, followed by trismus and difficult deglutition. These symptoms went on to complete tetanus. On account of her enfeebled state, blisters were alone resorted to, and the lochiæ were restored by warm baths. The spasmodic symptoms did not abate for twenty days; and forty elapsed before she was quite restored.

The author of the article on Tetanus in the Dictionnaire des Sciences Medicales, M. Fournier-Pescay, mentions an instance, similar to the above, in which exposure to cold acted apparently as the more immediate excitant of an attack of this disease in a puerperal patient.

CASE XV.—A woman, sixteen days after her confinement, was for a short time exposed to cold, and cold draughts. She experienced at the time a sensation of great chilliness; and suppression of the lochia suddenly followed. Ten or twelve hours subsequent to this exposure, all the muscles of her body were powerfully contracted (*fortement contractés*). She was bled with the lancet, leeches were applied to the vulva and epigastrium, and emollient tepid drinks and baths were employed. Under their use the patient's symptoms disappeared, while the lochial discharge became re-established.¹

In a case of puerperal tetanus published from the notes of the late Mr Colles of Dublin, exposure of cold also seems to have been considered as an important link in the excitement of the attack.

CASE XVI.—A woman who had been delivered a fortnight before of her sixth child, was exposed to cold. That same evening she felt lightness of her jaws, and was ordered opium, and mercurial ointment. Next day she could open her mouth better, but during it had two or three tetanic paroxysms, and died.²

In the case which I have just cited from Mr Colles, the disease occurred in June; and there seems to be a prevalent idea in the profession that tetanus is more common in our own climate in the warm than in the cold months of the year; in summer more than in winter. But the idea is of very doubtful accuracy. In one of the Registrar's Official Reports³ there has been published a table showing the relative number of deaths produced by different special

¹ Dictionnaire des Sciences Medicales. Vol. lv., p. 15.

² Dublin Quarterly Journal, No. xxx. p. 288.

³ Ninth Annual Report, p. 151, &c.

diseases in London during the four different quarters of the year, and from 1840 to 1847 inclusive. During these seven years, 179 individuals died in the English metropolis of tetanus. Of these 179 cases, forty-nine occurred during the spring quarter of the year ending with March; thirty-seven during the summer quarter ending with June; thirty-four during the autumn quarter ending with September; and thirty-nine during the winter quarter ending with December. Such facts show, beyond cavil, how very little influence, if any, season has upon the frequency of tetanus in our own climate.

Traumatic tetanus is a disease which is generally alleged by pathologists to be more common in warm than in temperate climates, a result to which various causes may contribute, besides the mere amount of heat. And puerperal, like surgical tetanus, would appear to occur more frequently under the tropics than with us. Thus, in some remarks which Dr Christie has published on tetanus in Ceylon, he casually observes, that he had occasion to treat a case of tetanus consequent on parturition, "which (he adds) the native practitioners of Ceylon inform me is *not* an unfrequent occurrence here." Dr Christie gives the few following particulars regarding this instance of puerperal tetanus:—

CASE XVII.—A healthy woman of the Portuguese caste was, two days after delivery of her first child, seized with rigidity of the jaw, and other symptoms of tetanus. The native doctors and midwives (continues Dr Christie) immediately pronounced her incurable, and their predictions were fulfilled. She was in vain treated with mercury, opium, and the warm bath.¹

Individual examples of puerperal tetanus among the inhabitants of tropical climates are sometimes incidentally alluded to in the writings of those that have practised in these climates. Thus—

CASE XVIII.—In a communication² upon tetanus, Mr Dickenson, surgeon in Grenada, reports concisely the histories of thirty-three cases of the disease which he met with during the time he was in practice in the West Indies. One of these cases of tetanus occurred in a female after delivery. The patient—a black—was seized with the disease soon after parturition, and died on the fifth day from the commencement of the malady. The treatment consisted of blisters, mercurial friction, and laudanum.

Puerperal tetanus has occasionally been observed to supervene after cases of operative midwifery. An instance in which tetanus succeeded delivery by turning, in a case of placental presentation, has been recorded by Mr Finucane of Nenagh.³ The case is further

¹ Edinburgh Medical and Surgical Journal, vol. viii. p. 415.

² London Medical Repository, vol. i. p. 192.

³ Lancet for June 2, 1838, p. 338.

remarkable in consequence of the extreme rapidity (if the reporter's data are given correctly), with which the tetanus ran through its brief and fatal course.

CASE XIX.—A patient, in her fifth confinement, was attacked during labour with much flooding. On examining, Mr Finucane found the placenta attached to the cervix uteri, and in consequence, proceeded to extract the child, which was still-born, by the operation of turning. Four days afterwards she had a violent rigor, followed by perspiration, which returned slightly on the following day. On the evening of this (the fifth day after delivery), the patient began to complain of stiffness about the articulations of the jaw, with pain in the back part of the head and neck. Early on the following morning the lower jaw was found completely fixed, and the patient was suffering under complete opisthotonos, the body resting on the heels and occiput. Every attempt to drink excited violent spasms, and the patient died in a few hours. The tetanus ran throughout its fatal course in little more than fifteen hours.

The Cæsarean operation has, from the earliest annals of French surgery up to the present time, been performed many times in Paris. The present distinguished Professor P. Dubois has himself, I believe, operated on eight patients. But it is a remarkable fact, that not a single instance of recovery from the Cæsarean section is known to have ever occurred in the French capital. The case in which the patient survived for the longest time, at least of late years, was one operated upon in 1839, and where ultimately the woman died of tetanus, seventeen days after the performance of the Cæsarean section. The following are the principal points connected with this instance of the disease:—

CASE XX.—On the 22d January 1839, Professor Dubois extracted a living child by the Cæsarean operation from a deformed dwarf. Febrile and inflammatory symptoms speedily set in; but by the 31st these had, in a great measure, disappeared; the pulse was 110, and the patient's appetite had returned. The patient's state continued to improve still more up to the 5th February, or till the fourteenth day after the operation, when general uneasiness and stiffness in the movements of the jaw came on. The masseter muscles were soon affected with violent contractions; the poor patient was unable to drink; and the symptoms of tetanus became clearly established. The disease continued up to the 9th (the tetanus affecting principally the muscles of the jaw and neck); fits of suffocation supervened, and the patient at last sunk on the seventeenth day after delivery, and the fourth after the first commencement of the tetanic symptoms.

At the time the tetanus began, the abdominal wound was already completely healed, except at its lower angle. On dissection, the original wound in the uterus was found closed, not by the union of the edges of the cut uterus, but by their close adhesion to the ante-

rior wall of the abdomen, to the posterior wall of the bladder, and to a fold of intestine. Several small abscesses existed among the peritoneal adhesions. In the spinal marrow nothing was found except a very slight and doubtful softening at one point.¹

In the following instance of tetanus supervening in a patient who had been previously the subject both of a surgical operation and of parturition, it is difficult to say whether the fatal attack of tetanic disease belongs to the class of surgical or of obstetric cases, or pertains to either. The disease set in at an unusual distance of time after parturition, and at a still longer date after the surgical operation.

CASE XXI.—In 1809, Mülder of Gröningen excised the knee-joint of an adult female, in whom articular disease had been of long standing. Great irritative fever followed at the end of nine days; and, later, she suffered intensely from the knee, and from hectic fever. At the lapse of two months after the operation, she was delivered of twins, and ultimately she died from tetanus, seven weeks after delivery, and fifteen weeks after the operation for excision.²

Let me make one further remark before closing this list of instances of puerperal tetanus. Occasionally cases of death from tetanus after delivery are referred to in English official and tabular records, without any special details in regard to them being given. The following two or three instances are examples of this remark, and afford additional evidence of the fact that puerperal tetanus is not so very rare as the total silence of obstetric works on this subject might lead us *à priori* to suppose.

CASE XXII.—The late Dr Merriman, in his Synopsis of Difficult Parturition, has published a table (p. 339) of the causes of death in childbed, among 10,190 attended by one physician. There occurred 107 maternal deaths in this long list of deliveries. One of these puerperal deaths is noted as having been produced by "locked jaw." But no particulars are given.

CASES XXIII., XXIV.—From the Fourth Annual Report of the Registrar-General, it appears that, in the course of the year 1840, 140 of the inhabitants of England and Wales died of tetanus. The sex of the patients, in 116 of these cases, is given in the Report. Of these 116 patients, ninety-six were males, and twenty were females. The causes which led to the attacks of tetanus, in this long list of cases, are, of course, not specified. But, incidentally, it is stated in the Report (p. 224) that two of the deaths from tetanus occurred after parturition, "lock-jaw caused by childbirth" being reported in two cases

¹ Lancet, for 29th February 1840, p. 821-852.

² Wachter's Dissertatio de Articulis Extirpandis. Groningen. 1810.

in which the patients were aged "34 and 35 years." Thus, out of the twenty cases of fatal tetanus among females during that year, two at least, or 1 in 10, were cases of puerperal tetanus.

The relative frequency or rarity with which tetanus occurs in obstetrical, as compared with surgical practice, could perhaps be properly ascertained by an analysis of the official returns given in for a few years to the Registrar-general of all deaths from this disease. And certainly the investigation would form an interesting point of inquiry in puerperal pathology.

NATURE OF PUERPERAL TETANUS.

It will be granted, I believe, by all pathologists that the existence of an injury or wound upon the external parts of the body is by far the most common cause of tetanus. After abortion and parturition we have the existence, upon the interior of the uterus, of a similar state of lesion. All authorities seem now generally agreed as to the facts (1) that the human decidua is, as was maintained in the last century by Krummacher,¹ the thickened and hypertrophied mucous membrane of the uterus, (2) that the epithelial or superficial layer of it separates from its basement or outer layer in abortion and after delivery—and (3) that this separation or solution of continuity of tissue, as well as the rupture of the organic attachments of the placenta from the uterus, leaves the interior of this organ so far in the condition of an external wound, or with a new or raw surface for the time being exposed. Obstetrical tetanus has, in this respect, an exciting cause essentially similar to surgical tetanus. And perhaps the great reason why this state of lesion of the interior of the uterus does not more frequently give rise to tetanus is simply this, that the uterus is itself principally, or indeed almost entirely, supplied by nerves from the sympathetic system, while apparently, as stated by Mr Curling and other pathologists, tetanus is an affection far more easily excited by lesions of parts supplied by nerves from the cerebro-spinal system, than by lesions of parts supplied by nerves from the sympathetic system.

Tetanus is known to follow wounds very various in their degree and severity. "Whether (says Professor Wood)² the wound is trifling or severe seems to be of little consequence," as far as regards the supervention of secondary tetanus. By what pathological mechanism a wound or lesion of a part can, under any circumstances, lead on to an attack of tetanic disease, is an inquiry regarding which we as yet possess little information; and in this respect, the production of obstetrical tetanus is not more obscure than the production of surgical tetanus.

¹ *Membrana decidua*—"Proprie est, membrana uteri interna, quæ post conceptionem intumesit et crassescit, usque ad tertium circiter graviditatis mensem, etc."—See Krummacher's *Dissertatio Circa Velamenta Ovi Humani*, 1790.

² *Practice of Medicine*, vol. ii., p. 746.

The disease, when developed, essentially consists of an exalted or superexcited state of the reflex spinal system, or of some segment or portion of that system. What circumstances in midwifery or surgery might possibly, whether singly or in combination, produce this state, and so produce traumatic tetanus? In relation to this question I will venture to make one or two hypothetical remarks.

1. We have in obstetric pathology evidence almost amounting to certainty that the analogous superexcitable state of the cerebro-spinal system of nerves which gives rise to eclampsia or puerperal convulsions is generally produced by the existence of a morbid poison in the blood. And it seems not impossible that the generation of a special blood poison, at the site of the wound or elsewhere, may sometimes in the same way give rise to obstetrical and surgical tetanus. We know, indeed, that the introduction into the blood of particular vegetable poisons is capable of exciting an artificial disease quite analogous to tetanus. Brucine and strychnine have both of them, as is well known, this effect. Abundant experiments upon the lower animals, and cases of poisoning in the human body have amply proved this.¹ Tetanus is a frequent disease from wounds, etc., in the horse and lower animals. Would it not, in relation to the possible humoral origin of the disease, be worthy of trial whether the blood of an animal dying of tetanus is ever capable by transfusion of infecting another animal with the same malady? The experiment is said to have succeeded with a disease having many analogies with tetanus, viz., hydrophobia. But,

2. We know further, with regard to the tetanizing effects of strychnine and brucine, that these agents do not necessarily require to circulate in the blood in order to produce their special effects. In the lower animals, when strychnine or brucine is applied directly to the spinal cord, tetanic effects instantly follow; and in all probability, when they are introduced into the blood, they produce their tetanizing consequences, by being carried in the current of the circulation to the cord—thus toxicologically influencing it, as if they were primarily applied to it. Centric irritation, or centric morbid conditions of the cord (and, it may be, of the cerebro-spinal system), may lead on, according to these experiments, to tetanic disease, independently of any morbid state of the blood. And, if the observations of Sims, Harrison, and others, are correct as to the occasional mechanical origin of *Trismus Nascentium* from pressure and displacement of the occiput upon the medulla oblongata, we

¹ See details in the works on Toxicology. Thus, in a boy poisoned by false angustura bark (which contains both brucine and strychnine) Professor Emmert found that when "he touched the patient's arm, in order to feel his pulse, a sudden and violent *Tetanic* spasm intervened; the eyelids opened wide; the eyes projected rigidly and immoveably; the lower jaw was firmly compressed against the upper one; both lips separated from one another, so that the front teeth were exposed; the different muscles of the face were tense; the extremities were extended and rigid; and the spinal column and the head were violently drawn back."—Romberg's Manual, vol. ii., p. 129.

have, in this obstetrical instance, tetanus resulting,—as it certainly sometimes does under other circumstances,—from direct injuries, or affections of the nervous centres.

3. The appropriate and specific affection of the spinal cord, or cerebro-spinal system, constituting traumatic tetanus, would appear to be sometimes, if not always, a condition excited by some influence propagated upwards along the nerves, from the seat of the injury or wound to the central portions of the nervous system. In proof of this, we have the fact that occasionally (as in cases published by Hicks, Murray, Larrey, etc.), but not often, the artificial division of the nervous communication between the seat of the wound and the nervous centres has arrested the disease, when performed in a very early stage of the attack. What the nature of the transmitted influence may be, we have no means at present of judging. But we have analogies for the transmission itself in some of the phenomena of electrical induction and propagation. And, let me further add, that the symptoms of tetanus assimilate themselves very greatly to a rapid succession of electrical or galvanic currents transmitted peripherally from a nervous trunk or centre to the sets of muscles affected.

At the present time various physiologists are busy with recondite inquiries into the laws pertaining to the electro-motive powers of the nervous and muscular systems of the body. When our knowledge of these laws is more advanced, physicians will, perhaps, be able to deduce from them a truer and juster pathology of tetanus and other convulsive diseases.

TREATMENT OF PUERPERAL TETANUS.

According to Mr Curling, tetanus is not only less frequent in women than in men, but it is also “*less fatal* in the female than in the male sex” (p. 29).

Out, however, of the preceding list of twenty-four cases of obstetrical tetanus, only three recovered (Nos. XIII., XIV., and XV.); and these under opposite modes of management. There are two other cases on record of the successful termination of puerperal tetanus. The two instances I allude to have been reported by Dr Symonds and Dr Currie. The true tetanic character of both cases may perhaps admit of some doubt.

In his article on Tetanus, in the *Cyclopedia of Practical Medicine* (Vol. IV., p. 874), Dr Symonds of Bristol speaks of the first of these cases in the following words:—

CASE XXV.—“ We remember (says Dr Symonds) attending a young woman, who was seized with lock-jaw soon after delivery of a still-born and premature foetus; but the affection readily gave way to a turpentine injection, and we looked upon it as a mere sympathetic accident.”

The other case of recovery from puerperal tetanus, which I have

adverted to above, is mentioned by the late Dr Currie of Liverpool, in an essay on the treatment of tetanus and other convulsive diseases by the cold bath and cold effusion; which he published in the third volume of the "Memoirs of the Medical Society of London." In this essay he details six instances of tetanus. He gives the few following particulars regarding the puerperal case :—

CASE XXVI.—A poor woman, "in consequence of difficult labour, and, as she imagined, of local injury in some part of the uterus, was seized with the *spasmus cynicus*, locked jaw, and other symptoms of tetanus. She was immediately taken to the cold bath, and thrown into it, with good effects. The spasms disappeared, and though they afterwards returned in a slight degree, they gave way entirely to a second immersion."

Supposing these two instances of recovery to have been instances of genuine tetanus, it would still appear, from the evidence of all the cases which I have collated, that puerperal tetanus is nearly as fatal as surgical tetanus, for, out of the whole twenty-six examples collected, twenty-one died and five recovered; or, the disease was fatal in the proportion of four out of every five persons attacked. Dr Lawrie has calculated surgical tetanus to be fatal in seven out of every eight who are seized; but, like Mr Curling, he believes surgical tetanus in females to be less fatal than this.

In such a communication as the present, it would be out of place to dilate on all the means usually recommended for the treatment of tetanus. Let me merely remark that, in obstetrical tetanus, no kind of local treatment to the seat of the original uterine lesion could be well applied, or would probably be of any avail, if applied. And, as to *constitutional* means, perhaps the most important are,—

1st, The greatest possible quietude and isolation of the patient from all irritation, corporeal or mental, during the course, and for some time even after the resolution of the disease.

2d, The special avoidance of painful and generally impracticable attempts at opening the mouth in order to swallow; but sustaining the strength of the patient, and allaying thirst by enemata, or by fluids applied to the general surface of the body.

3d, If there is any well-grounded hope of irritating matters lodged in the bowels, acting as an exciting or aggravating cause, to sweep out the intestinal canal at the commencement of the disease with an appropriate enema.

4th, To relax the tonic spasms of the affected muscles, and diminish the exalted reflex excitability of the spinal system by sedatives, or antispasmodics; with the prospect of either directly subduing this morbid reflex excitability; or of warding off the immediate dangers of the disease, and allowing the case to pass on, from an acute and dangerous attack, to a sub-acute, and far more hopeful and tractable form of the malady.

Various sedatives and antispasmodics have been recommended to fulfil this last most vital and important indication in the treatment of tetanus—as belladonna, stramonium, hemlock, henbane, musk, camphor, Indian hemp, hydrocyanic acid, valerian, camphor, etc. Perhaps the two drugs of this class that have hitherto been most used, and most relied upon, are opium by the mouth, and tobacco by enema. But certainly we have no decided evidence of the beneficial effects of opium, even in the most heroic doses; and it seems doubtful even if this and other such medicines are readily or at all absorbed from the stomach and upper part of the intestinal canal in cases of acute tetanus. Tobacco in the form of enema has doubtlessly often acted most favourably in arresting the spasms; but it is a drug the action of which is not easily or safely kept up with that degree of constancy which is required in acute tetanus. Latterly the antispasmodic action of sulphuric ether and chloroform has been repeatedly employed to allay that exalted state of the reflex nervous system, and to relax that resulting tonic contraction of the maxillary and other muscles, which constitutes the essence of tetanus. Medical men may yet discover therapeutic agents (to be introduced into the body by inhalations or otherwise,) the action of which will be as directly anti-tetanic in their effects, as strychnine is directly tetanic in its properties; and such agents—if they were otherwise innocuous—would form the proper remedies for tetanus. Here, as elsewhere, in future medicine physicians will probably seek for therapeutic remedies in the same way, and upon the same principles, as toxicologists search for antidotes to poisons. Chloroform in sufficient doses acts as a direct sedative upon the reflex nervous system, and upon exalted muscular contractility. In consequence of this action, it affords us one of our surest and most manageable means of allaying common convulsive attacks; and it has now also, according to the reports in periodical medical literature, been repeatedly successful in the treatment of traumatic tetanus,¹ whilst it has apparently also repeatedly failed in subduing the more acute forms of the disease. Perhaps some of the failures have arisen from the patient not being kept sufficiently deeply and continuously under the action of the drug. If used in tetanus its action will require to be sustained for many hours, or oftener perhaps for many days. And there is abundant proof of the safety with which its continuous action may be kept up under proper care and watching. For instance, a few months ago I saw, with Dr Combe, a case of convulsions of the most severe and apparently hopeless kind in an infant of six weeks. The disease at once yielded, and ultimately altogether disappeared under the action of chloroform, which required to be used almost continuously for thirteen days; as much as 100 ounces of the drug being used during the period. After all tendency to convulsions

¹ See, for example, Dr Ranking's Abstract, vol. ix., p. 239 (three successful cases); British and Foreign Medical Review for 1851, p. 464 (two successful cases), etc.

at last ceased, the little patient rapidly grew, and is at the present moment a very strong, healthy child. In a case of the successful treatment of traumatic tetanus by Dr Dusch, above sixty ounces of chloroform were employed.¹ Let me close these remarks with a case of puerperal tetanus treated with chloroform.

CASE XXVII.—In the following letter, dated 20th January 1854, Professor Lawrie, of Glasgow, has kindly communicated to me the encouraging results (so far as they go) of an example of the disease which is at present under his care :—

MY DEAR DOCTOR,—If the following notes, of a case of tetanus after abortion, be of any use to you, they are at your service.

Mrs B——, a fine young woman, æt. 24, in the third month of her third pregnancy, miscarried on the 4th of this month (Jan. 1854). She lost a considerable quantity of blood, and required plugging, cold, and pressure, but was so well on *Sunday the 8th* that I ceased my attendance. On *Thursday the 12th* she complained of stiffness about the neck and lower jaw, but not suspecting the nature of her illness, she did not send for me till late on *Saturday the 14th*. I found trismus well marked—the spasm not extending beyond the neck—and the pulse nearly natural. Every attempt to swallow gave great pain, and produced a spasm in the muscles of the neck and larynx, which threatened instant suffocation. I forbade all attempts at swallowing, ordered nutritive enemata with 50 or 100 drops of laudanum every six hours, and pectra to the neck, with aconite and chloroform. There was little change till the night of *Monday the 16th*, when the pulse had risen to 120, and the spasms had greatly increased, but had hardly extended beyond the neck; deglutition was impossible. I immediately exhibited chloroform, which acted admirably, and gave instant relief. I taught her husband and mother how to use it, and she has since been more or less constantly, and nearly continuously, under its influence. *Thursday, 18th*, the pulse was 96, and she swallowed with comparative ease. To-day, *Friday, 20th*, she is not quite so well; the pulse is 108, the abdominal muscles rather tense, and the rectum will not retain the enemata. In this last occurrence I was of course prepared; and, since the 15th, she has been carefully rubbed with oil, butter, and cream. She still swallows tolerably well. My *prognosis* in this case was from the first serious, for although the pulse was natural, and the spasms had not extended beyond the neck, deglutition was nearly impossible, and suffocation was often imminent. One symptom I have forgotten, which is often one of the most distressing—a constant cough from accumulated mucus, which cannot be raised or got rid of. In two days it has disappeared. I now anticipate recovery. The *treatment* has been negative, with the exception of the chloroform, which has been most useful. I attribute the improvement on the 17th and 18th entirely to it; and if Mrs B. recovers, she will owe her life to your invaluable discovery.

Excuse great haste, and its accompanying imperfections.—Believe me always yours,

JAS. LAWRIE.

Even when not curative of tetanus, the use of chloroform has proved of no small service in relieving the sufferings and agonies of the patient. Thus, in a fatal case of puerperal tetanus already detailed in a preceding page (Case V.), the medical gentleman who drew up the case observes, “We had the just comfort of finding the paroxysms materially alleviated by chloroform, which was used dur-

¹ Ranking's Abstract, vol. xvii., p. 63.

ing the last sixteen hours of the patient's life. 'Chloroform! Chloroform!' was the poor sufferer's anxious exclamation whenever she felt the spasms impending, and comparative tranquillity speedily followed its application each time." Our practical power over the most rapid and fatal forms of tetanus perhaps amounts yet to little more than a "meditatio mortis." But even to alleviate the sufferings attendant upon such a dreadful disease (should we effect no more), and to produce a state of euthanasia, is surely an object worthy of the best directed efforts of the medical art. 'Physicians (says Bacon) do make a kind of scruple and religion to stay with the patient after the disease is deplored; whereas, in my judgment, they ought both to inquire the skill, and to give the attendances for the facilitating and the assuaging of the pains and agonies of death.'—*Montagu's Edit. of Bacon's Works*, vol. ii., p. 166.

ARTICLE II.—*Observations on the Dentition of the "Lilliputian Aztecs."* By ROBERT REID, C.D.D., Dentist to the Merchant Maiden Hospital, Edinburgh.

THE appearance in this country of those interesting little strangers called the "Aztec Lilliputians," has occasioned a good deal of speculation as to their origin, history, age, etc., in all which there is a mystery yet to be cleared up. A question having arisen as to the last mentioned point, professional opinion was sought thereon, in so far as could be formed by an examination of their physical structure, present appearance, and particularly their dentition. As this in both cases presented features of considerable interest, a few observations on it may prove worthy of being put on record.

Having taken casts of the dental apparatus in both individuals, I shall note the peculiarities in either case respectively, beginning with the boy, Maximo. In the upper jaw (see woodcut No. 1), there are altogether eleven teeth; of these, three are deciduous, namely, the left canine, and two molars right and left. The remaining eight are all permanent, and rank as follows, namely:—

- 2 Central Incisors.
- 2 Lateral Incisors.
- 2 First Bicuspid.
- 2 Molars, being the six-year old teeth.

It will be observed that the right canine is not included in either of the above tables. The space is unoccupied, and from the appearance of that portion of the jaw, there is nothing to indicate the approach of the tooth. It may also be stated that the left one remains quite firm in its socket; and as the party exercising a

No. 1.



guardianship over those diminutive beings, asserts that there has been no change in the dentition of the boy during the three years he has been under his care, it is not at all improbable that the missing tooth has been forcibly ejected from its place, the supposition being strengthened by the fact that marks of violence are to be found on the face and other parts of the body. The statement of that party, however, must be received with caution, as it is more than probable, that during so extended a period as three years, important changes may have taken place, without exciting his notice. I shall immediately offer proof on this point. The lower jaw contains only seven teeth, all of which are permanent. They stand thus:—

- 2 Central Incisors.
- 2 Lateral Incisors.
- 1 Bicuspid (being the first, right side), and
- 2 Permanent Molars (six-year old teeth).

There is a vacant space for the first left bicuspid caused by the disappearance of the remaining milk grinder, and which, it must be observed, took place during the interval betwixt the 5th of November and the 3d of December, these being the days on which I took the casts which are represented in the accompanying woodcuts, Nos. 2 and 3—the one showing the presence of the tooth in

No. 2.



No. 3.



question, and the other the depression in the gum denoting its situation. This circumstance in itself is not deserving of consideration in estimating the age of the individual, as the milk grinder at times retains its place till an advanced period in life. In a case which recently fell under my notice, the patient, then in his 49th year, applied for extraction of the same tooth, the second bicuspid on that side having not yet appeared.

The absence of the canine teeth of either denomination in the lower jaw, will be observed, and constitutes one of the most remarkable features in the case. There is no space to receive them, neither is their presence in the jaw bone indicated by protuberance either anteriorly or posteriorly. The incisors, were they placed in antagonism to the upper ones, might be found very serviceable, being strong and well grown, the laterals so much so, that, at first sight, they might be taken for canines. The opinion may therefore be hazarded, that nature has not destined the last mentioned teeth to make their appearance at all, an assumption favoured by the extra-

ordinary relative position of the jaws to each other. To this point I shall shortly return. The permanent teeth are, on the whole, well developed, neatly shaped, in fair proportion to the general structure of the individual, and at same time well placed, both as regards their growth above the gum, and the contour of the dental arch. There is, as yet, no appearance of the second molar in either jaw, but its situation is indicated by the flattened appearance of the gum and the space betwixt the first molar and the ramus.

The form of the upper arch is somewhat remarkable, being rather narrow and pointed, yet without that overlapping of the front teeth usually accompanying what is familiarly termed the rabbit shape. The relative position of the jaws to each other, that is, the bite, has already been alluded to, and deserves special notice. Irrespective

No. 4.



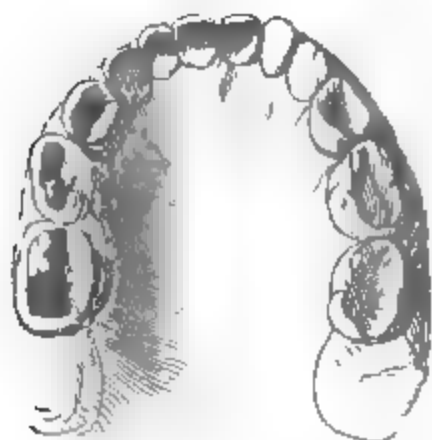
of the Assyrian lineaments marking the upper portion of Maximo's face, the retreating lower jaw gives his profile a strange hawk-like expression. Instead of the under front teeth falling immediately behind the upper, there is a clear space of $\frac{1}{10}$ ths of an inch between the two arches when shut, such as will admit of the thumb being freely passed up to touch the palate, (see woodcut No. 4). This peculiar feature might, at first sight, be regarded as a freak of nature, exhibited in a solitary individual; but the relative position of the upper and under back teeth militates against such a view, and rather favours the assumption that the peculiarity extends to the immediate family, if not to the tribe (supposing such to exist), and that for the following reasons, namely, that while the upper jaw contains eleven teeth, all arranged with due regularity, the under contains only seven, by which provision of nature the molars in either jaw antagonise and fit into each other with great exactness, the posterior edge of the last tooth in either jaw being in a line with that of its adversary. This feature my observation has not enabled me to trace in cases of dental irregularity or mal-arrangement with children in this country, such, on the contrary, tending to effect the symmetry of the opposite arch, and destroy the regularity of a close-fitting bite.

It is highly interesting to trace the means by which nature has brought about the antagonism of the four six-year old teeth with each other. While the upper jaw has ample room for the full complement of teeth from the incisors to the first molars inclusive, and which will be twelve when completed; the retreating shape of the under jaw leaves room for eight only, the remaining four, namely the canines and second bicuspid being denied a place among their fellows, and thus by their exclusion the end is accomplished, the space betwixt both dental arches at their apex answering to that

which would be required on each side to contain the teeth wanting.

We now come to the case of the girl Bartola. In her dentition there is little to note, it being in a backward state. In the lower jaw (woodcut No. 6), she has got her two six-year old teeth, and a

No. 5.



No. 6.



permanent central incisor on the left side. All the other nine are deciduous and remain firm in their sockets, with the exception of the right central incisor which is about to give way to its permanent successor. There is a peculiarity in the lateral incisors and canine teeth on either side worthy of remark. These teeth are fused together, forming one instead of two separate teeth, all attempts to pass a thin blade down between them having failed. Such may occasionally be met with in single instances, but rarely in corresponding teeth in opposite sides of the dental arch. In the upper jaw (woodcut No. 5), the only permanent teeth that have as yet made their appearance, are the six-year old, the temporary set remaining quite unbroken. A word may, however, be said as to the shape of the dental arch.

On beholding these little creatures for the first time a few months ago, I was forcibly impressed with the idea that they were not of the same race. There was nothing in the physiognomy of the girl that corresponded with the prominent nose, lustrous protruding eye, retreating brow and silky hair of the boy. Her facial angle was entirely different, her eyes small, and her hair curling and even crisp were it not carefully looked after, and parted into ringlets, all which point rather to an African than an Asiatic origin. An examination of the upper dental arch strengthened the impression first made, its breadth and roundness with a slight flattening in the centre, bearing no small resemblance to that of the negro, and which must in the course of time become more apparent when the jaw is fully developed. This point, although away from that immediately before us, is still interesting and worthy of being touched on as showing the assistance to be obtained from an examination of the dental structure in determining the race to which an individual probably belongs.

Having already mentioned that the object sought in examining the dentition of these children was, in the absence of any positive evidence on the point, to afford an opinion as to their probable age, I may shortly state the conclusion come to, and the means by which it was arrived at. The ages were judged to be about twelve years in the boy, and eight in the girl. In estimating them thus, the present stage of dentition, in either case, would scarcely have warranted it, were that to be relied on as positive evidence on the point. Other appearances were sought for and found, tending to solve the difficulty. The development of the permanent teeth, as to their height above the gums, and, in the boy, the flattened appearance of the latter, at the heel of the jaw, betokening the advent of the twelve-year-old teeth, also the marks of attrition on such of the permanent teeth as had antagonists, were significant features, and although affording apparently but scanty evidence, yet taken in conjunction with the general structural development betokening a slow physical growth in each individual, warranted the opinion hazarded as to their age. In the absence of direct and positive evidence on the point, it is satisfactory to me to learn that the above opinion is identical with that expressed by one whose authority in such matters is of the greatest weight. A friend informs me that he was present at a meeting of the Ethnological Society of London, held in July last, when an elaborate essay was delivered by Professor Owen on the physical structure, age, etc., of the Aztec children, in which he assigned to them nearly, if not exactly, the ages which my examination led me to consider as nearest the truth. I have not been able to ascertain more precisely the exact opinion of Professor Owen, or the grounds on which he formed his conclusions. His paper has not yet been published, but will, I am led to understand, appear in the Transactions of the Society in the course of the approaching year.

ARTICLE III.—*A few Observations on the Climate of Teneriffe.* By ARCHIBALD WM. PULTENEY PINKERTON, M.D., Ann. President Royal Medical Society, Edinburgh.

IN drawing attention to a few scattered facts concerning Teneriffe, I am not anxious to urge this climate in preference to others. I would not venture to say that it will be of more service than another; nor preferable to many other invalid resorts. In bringing forward Teneriffe in this paper, I would merely draw attention to the few facts gathered, and the few observations gleaned, during a two months' residence in Santa Cruz, knowing that it is only by such information, imparted by those who have had experience, that facts are elicited and advantages reaped.

The climate of Teneriffe differs from that of Madeira in several respects. That of Madeira is well known, many persons having

gained such experience as enables them to speak favourably and decidedly. And in this respect many are correct, for to them is Madeira a place of relief, and an antidote to many sufferings. But it is incumbent on all who would truthfully observe the different arrangements of nature, to note the advantages and disadvantages of every place.

The climate of Madeira is acknowledged to be to several a source of benefit, and if so, the Canaries, situated as they are in much the same locality, may be expected to differ very little.

Placed in 28° of latitude, and nearer the African coast, it follows that the atmosphere will be drier, from its nearer approach to the equator. To some this will be an advantage, to many the reverse. Lying 200 miles further south, these islands partake more generally of a tropical appearance. Excessively hot in summer, they, from their greater amount of high land, present a greater proportional degree of cold in winter. This is especially the case in the island of Teneriffe, which, owing to the Peak, presents some of the winter phenomena observable in mountainous continental districts. And in this respect it differs from Madeira, concentrating within itself the beauties of the tropics with the genial coolness of an English autumnal noon. Of the seven islands composing the group, that of Teneriffe presents the most favourable resources for invalids; for at Santa Cruz, which is the chief town, all English and other steamers stop; there all functionaries live, and commerce exists.

Lying on the southern side of the island, with somewhat of an eastern exposure, the town sometimes suffers from the African winds. But in other respects it is sheltered, for all round a low ridge of mountainous country preserves it from the northern and north-west winds. It is exposed to the sun all through the year, with only the sea breeze, if the wind be favourable, to relieve the excessive heat. In size it is inconsiderable; in invalid accommodation very scanty. Cleanliness, however, in outward show, is no fault, while taste and convenience are always consulted: the well paved streets, whitewashed tidy houses, presenting a great contrast between those of the Spaniards on the continent and those on the island. The Peak is situated near the north-western extremity, while, stretching east, runs a ledge of hill, broken here and there by ravines and defiles of no great size when seen from the sea, and of no great influence over the climate of the town which lies at their foot. Towards the south-eastern extremity lies Santa Cruz. On the western shore, immediately at the foot of the Peak, lies Orotava, while between the two, among the high lands, lies Laguna. This, during winter, is damp and dreary in the extreme; in summer it is the resort of the population of Santa Cruz. Although this latter is so damp, Santa Cruz itself is the very opposite; for except the dew and sea breeze in the evening, the atmosphere seems to abound in dryness. In summer and autumn, the foliage is burned up, the people cannot show themselves during mid-day, and everything languishes

for moisture. It is not completely hedged in by mountains, like Funchal, but, lying at the foot of a gradual declivity of no great height, the natural scenic effect is not good. Besides, the soil seems so barren, so dry, so scanty; no foliage, nothing save defiles of no great extent covered with withering grass, that expectations are destroyed and hopes disappointed. Few houses save those in the town are seen; few gardens recal pleasant recollections; all is barren. Yet the same might have been said of other parts a few years back, and patience must be practised, experience gained, ere the same beautiful plenty is seen in Teneriffe as greets the eye from Funchal harbour. At first sight, every part looks scorched and withered, save where the deep shade of the orange leaf is seen mixed with its golden apple.

The town, of no great size, looks scattered, and presents nothing save white houses, of two or three but mostly of one storey in height. White walls surround them, while the coast fortifications, being of the same colour, produce an enlivening effect. All above, on the gentle slope, is scorched dried-up pasturage, with here and there plantations of cochineal, which being trained on broad-leaved stunted cactuses of rather a pale green colour, do not add to the beauty of the scenery. This rearing of cochineal forms at present the main employment of those who possess land near Santa Cruz, so that everything gives way to this, which produces a desert-like landscape. The sun is very strong, and, meeting with no counteracting agency, shines all day on the white-washed walls and houses, producing a heat which in summer and autumn is hardly to be tolerated even by the natives. In winter even the reflection from an extensive white wall is overpowering. The streets are neatly paved, clean, and not too narrow, while the houses themselves look comparatively clean and comfortable. Along the shore, towards the Lazaret, a good carriage-way extends; while in the other direction a delightful path leads in the shore and under the cliffs which stretch towards the eastern angle. There runs round the town, on the land side, a small carriage-road, which joins a very broad one stretching out twelve or twenty miles to Orotava. Towards the north-west, on high ground of a table-land character, six miles from Santa Cruz, lies the old capital, Laguna. Almost deserted during the winter, the dreary look of the empty large houses, still retaining the ornaments and devices of bygone nobility, amidst almost total solitude, present no attractions. During the summer, however, the green suburbs, the shade of the pines and arbutus, and the cooler air, attract the sea-side population, and then the alameda and the streets of this town present a lively appearance.

Orotava, the next town of importance, lies completely on the west coast, at the foot of the Peak, which towers away to the south. This is said to possess some conveniences for invalids, and several English are now located there; but no vessel touches at that port, while the road over the island from Santa Cruz not being completed, access

is at present very inconvenient. Thus the resources in Teneriffe are what might be expected from a native industrious, intelligent people, uninfluenced by fashion or change. The roads, houses, and gardens, few though the latter be, their cultivation of land, all excel their brethren on the continent, whom they do not particularly like. There is then room and opportunity for improvement, and sufficient talent to appreciate its advantages.

Situated on an island, amid a vast extent of sea, Teneriffe cannot be so dry as a continental inland, or east coast, for the sea air surrounding must imbue with moisture all things submitted to its influence. Whether the wind come from one or other of the points of the compass the result will be the same, provided the heat be equal. But this is not so. In summer the heat overcomes the moisture, and in this particular instance has the same effect on the damp atmospheres of other seasons, even of the winter. As already said, the atmosphere is dry, in other words, the heat is sufficient to overcome the damp, and so prevent the extreme languishing feeling of other climates. Sometimes, doubtless, it may assume this latter character, it may cause languor and disinclination for exertion, only however occasionally, and under certain conditions. The variations of the wind are also great, though from its situation Santa Cruz is sheltered from the north and north-west winds. These, especially the latter, are drier and colder than the rest, and have some twenty miles of land to come over previously; in summer, warm from the heated ground even to an intolerable extent; while in winter, the rain or wet upper lands impart a damp chill, and the snow, save when it is melting, a brisk sharp feel. The north-east wind comes sweeping round the east point sometimes high, during the rainy seasons stormy, but generally it imparts an agreeable coolness to the over-heated air. The south wind comes with full force against the valley, but so seldom as to cause no uneasiness. During the winter, mists are seen at sea, some winds are felt perceptibly damp, even saturated like a November fog, occurring either when the sun is clouded, or temporarily overpowered by the superabundant moisture, or when the breeze is not stormy enough to carry it rapidly away. They occur but seldom, though the air itself, owing to the extent of surrounding sea, must always be more or less charged with similar damp particles. The east and south-east winds, originating from the African continent, when stormy, are parchingly hot and dry, producing the same effect as a sirocco. These are rare, however, and during the winter have such a distance of sea to traverse as will, to a certain extent, compensate for the dryness. Again, some winds from the same direction are similar to the others, moist and foggy, evidently showing their marine origin, though the space for such is much more circumscribed than that of the west, north or south. The north-east wind serves as the cooling evening breeze during the summer and autumn, and, independently of it, at sunset the evening sea breeze regularly performs the

same office. Here, as elsewhere, the rainy seasons are periodical, the one towards the end of autumn, the other towards the beginning of spring, or October and February. Then the rains descend, the winds blow, and every element seems in commotion; the rivers, dry for months, in the course of a few hours almost overflow their banks. For days, for a fortnight, perhaps three weeks, will such continue, green, fresh verdure taking the place of dry barren, burnt up pasturages. Sometimes their storms are perfect hurricanes, but seldom do they diminish the *real* temperature as marked by the thermometer beyond a few degrees. The slight difference can easily be made up by a warm room, though at first from the sensations experienced, one would not expect such. For it must always be remembered that the chill or heat of these warmer climates affects us in different modes and degrees according to our longer or shorter stay previously in them. One who has been several months or only weeks in one of these climates will complain of chill and discomfort when a greater stranger will be complaining of the reverse, and one accustomed to nought save sunny dry days will feel wet stormy weather more cold and disagreeable than it really thermometrically is.

That such is the case a reference to tables of the thermometrical variations will show, for then it is seen that while in the room the markings are 68° , in the rain it will be only 65° or 3° less. The same thing occurs in the case of wind, which reduces the thermometer from 69° to 66° . A drizzly wet reduces it 5° , from 64° to 59° , while a wind coming off the snow-clad shoulders of the Peak will bring it down from 63° to 55° or 8° . In all these instances the people accustomed to invariable sun will feel chilled beyond the true or actual temperature, lowered though it be. In looking over the same tables of variations, great difference will be seen to exist between the temperature of the sun's direct rays and that of the shade. In those tables the evening and other markings were made in a room in which no fire was used, where the window was constantly open during the day, and which had an exposure toward the hill side away from the sun. The direct heat of the sun in January was 99° or 100° , 102° about ten A.M., and mid-day gradually losing strength by two P.M., being then 94° , 95° . In February 19th, while the sun's temperature was 89° , that of the wind was only 61° in the shade. But this was one of those clear frosty days caused by the brisk wind coming off the snowy upper lands, and partaking of many of the characters of the wind on the east coast of Spain. By reference to the table of hygrometrical markings, it appears that on the very same day, especially toward the afternoon, it was drier than on any previous one that month, although the rainy season could not have been properly said to be over.

The two months which I spent in the island, unfortunately were the worst in the year. Wet, damp February, and a somewhat cold January, form no real or satisfactory index to the remaining parts of

the year. The first fortnight, from the beginning of January up to the 24th, was beautiful, and the nights delightful, such as gave a pretty fair notion of the previous summer and autumn months. Cloudless, sunny, sufficient air to prevent extreme heat, such, with the only exception of diminished sun's power, were the characters. The rest of the month was alternately bright and cloudy, fair and showery. Gradually the rain came down heavier and heavier, the wind more continuous and gusty, the ground more and more damp, and consequently the air more and more raw. The snow this year came down further than usual, the rains being more violent and of longer duration. Still no very great difference in the mean monthly temperature is observable, for while the mean mid-day heat of January may be stated to be about 68° , that of February is about 66° ; and between these two means occur all the heaviest weather.

The other months are such as might be expected in such a situation. From the end of February, after the rains have ceased, on to August, the heat gradually increases, in which month, having attained its maximum, the temperature more rapidly falls, till having reached its minimum degree in the month of February, it again slowly rises. The autumnal rains in October tend to cool the air, producing a more genial uniform temperature during the winter months. From this moderate temperature of the worst season in the year, much benefit will be derived by the strangers, as it is agreeable, bright, and unrelaxing. The summer heats, especially toward the months of July and August, are so excessive that every one must seek refuge in Laguna, or some elevated spot, which example can easily be followed by the invalid, or perhaps Madeira might be substituted, if not, a voyage homewards. The winds sometimes blow strongly over the island, lowering the heat; but storms of any kind are rare, and the periodical rains, seldom severe, occur at the end of months characterised by uniformity of temperature and continual brightness.

The *mean annual temperature*, as given by Sir J. Clark, is 70.9 , the degrees of temperature not being "equally distributed over the year." The temperature of so southern an island must be much warmer than others more northerly, as is shown by Santa Cruz being 7° warmer than Funchal during the summer. It is to this additional heat that the increased dryness of Teneriffe is partly owing, though it be at the expense of almost insufferable warmth. In winter, again, it is only 5° warmer, though, from its position 200 miles further south, a greater difference might have been expected.

Whether this lesser difference during winter result from the increase of moisture required in the warmer climates, or from the greater amount of high land and frequency of winds, the degree of dryness during the winter is greater, perhaps from the larger size of the island and more sheltered site of Santa Cruz. Orotava is liker Funchal in these respects, because it is more exposed to the western winds fresh from the sea, and is not protected to the west by such a

pyramid of earth as the Peak. Again, the very fact of the Peak being so large, and such a vast receptacle for snow, will aid to chill the sun's heat without necessarily overcoming it, or adding to its moisture. It will give rise to higher currents of wind, colder during the snow period, and more similar to those of the continent.

The *mean monthly temperature*, as seen by tables, would give January 68° , February 66° . In a table drawn up by Mr C. Smith, he gives 68° and 65° as the respective maximum means of the same months in Orotava. Santa Cruz, however, being more sheltered, will have the above 68° and 66° as its mean, whereas Orotava, being more exposed, has the same for its maximum mean. Seeing there is so little difference, the same mean temperature may be taken for Santa Cruz as are found in Mr Smith's table given in Sir James Clark's work on Climate, taking the maximum line as the surer index. From this table, then, as well as from natural consequences, it is seen that the monthly mean temperatures gradually rise to August, which is the hottest month. From this they fall as gradually to February, which is the coldest. This taken at 66° , presents no very great degree of cold, the wet causing more uneasiness than the cold itself. This is also the rainy month, and the first twelve days were more or less rainy, either showery during the forenoon or the afternoon, or drizzling all day. From that to the 22d it was fine, when, owing to a south-west wind, a sultry, damp, misty, and rainy afternoon occurred. Showers are marked several times afterwards, but the weather seemed to amend gradually though surely. Up to the 16th of January the weather was very fine, genial and warm, with the exception of one afternoon, when a slight sirocco was wafted across from Africa. After the 16th, however, the days became gradually more and more cloudy, raw, and the hills more wrapped in mist and showers, the winds more ungenial and chilling. Mist was seen over the sea also, and the wind which brought it towards the island felt more chilly and raw. This continued with occasional glimpses of sun and fair weather until the 23d, when rain came down, increasing in severity, until it passed away in February. This last winter, however, was peculiarly severe, and ought not, therefore, to be considered the general condition of such a season.

During this time the thermometer reached its lowest point, but gradually; from 71° it came down to 62° or 63° in January, and in February keeping very regularly at 66° or 65° at first, for towards the end of this month some considerable variation may be noticed. On the 23d, a close sultry warm day, the thermometer rose to 70° , the previous days having also been warm. Next day a north-east wind set in, and sweeping round the eastern and northern hills, not yet dried from their half-finished saturation, came down with such a degree of cold as to make the thermometer fall from 66° to 62° , or 61° at night. The monthly variation for January is therefore 9° to 8° , and for February 8° (taking these two extreme markings, which, however,

may be looked on as accidental), or more generally 3° . The difference between these two months is less, only amounting to 2° , or, according to Mr Smith's table, 3° . From this it ascends in March to 4° , in April to 5° , and so on.

The *maximum temperature* marked during this month is 71° for mid-day, and morning 67° , rarely 67° for evening.

The *minimum* is for the same times respectively, 63° , once 62° , and 58° to 60° . The monthly variation will thus be 8° to 9° .

In February, the maximum temperature marked was, for the morning, 68° ; for mid-day, 70° ; and for the evening, 66° ;—the minimums being 62° for the morning and mid-day, 61° for the evening.

The monthly variation will be 6° , 8° , and 5° respectively, showing a greater equality and consistence. But these being taken from the extreme markings, for instance, the solitary 70° of the mid-day of February 23, or the similar 62° , 61° of the 25th, a little allowance may be made. February is more equable in temperature, being the same as occurs about the middle and end of January, after the partial set in of the rains and destruction of very fine weather. When it is remembered that no sun is visible at Christmas, that often before the 24th of January it falls on the Peak, little surprise can be experienced at the rapid fall, or the extensive variation. This is given somewhat greater in the table above referred to, but on the west coast, while the accompanying figures show the little extent of the same in the subsequent months. The diurnal variation of either month thus taken, very little difference is seen between the two.

The *diurnal variation* of January ranges from 71° to 58° generally throughout the day, or is 13° .

The maximum morning marking amounts to 71° , the minimum to 62° —or gives a range of 9° . The maximum mid-day marking amounts to 71° , the minimum to 62° —giving a range of 9° . The maximum evening marking amounts to 68° , the minimum to 58° —giving a range of 10° .

Looking at each day, however, no such great difference will be found—from 71° to 67° or 4° , 64° to 60° , and 73° to 58° , or 5° being the average highest differences.

The mean temperature for the whole month will thus be 65° or $64\frac{1}{2}^{\circ}$.

The diurnal variation of February ranges from 70° to 61° , or gives a difference of 9° .

The maximum morning marking amounts to 68° , the minimum to 62° —giving a range of 6° . The maximum mid-day marking amounts to 70° , the minimum to 62° —giving a range of 8° . The maximum evening marking amounts to 65° , the minimum to 61° —giving a range of 4° .

The range for each particular day, however, is from 70° to 65° , or 66 to 61 —average, 5° for difference.

The mean temperature for the whole month will be 65° ,—the same as January.

A great difference is observable naturally between the shade and any exposed spot, either in sun, wind, or rain; but this can have no effect on an invalid, who should not be so exposed, and who here and elsewhere must expect privations and imprisonments similar to what occur at home. These, however, are not so many in winter, but must be endured, probably for thirty days or so, scattered over the winter. The last twenty days of January 1853, no one could enjoy, and the same may be said of the first half of February. Dangers exist in great number for the imprudent and the wilful, as no person can be exempt from chills caused by draughts or damp air, so readily encountered during these rainy seasons in any quarter. Still, in Teneriffe the bright sun soon removes all dulness, the clouds clear away before a strong current of air, and in a day or so everything assumes a delightful appearance.

Although a few fine days occurred in the first part of January, several were damp and sultry from sluggish currents of sea air, slowly driven along for several hours, before settling on the thirsting land. Then languor and inability or unwillingness for exertion came on, with occasional headach and heaviness. When the weather cleared up, the languor and headach disappear, and the full delights of living in such a genial climate were experienced. Another day about the same period was rendered uncomfortable from the African winds reaching the island, and causing symptoms of a very different nature from its irritating and exciting qualities.

Between these two extremes lies the mean of the climate of Teneriffe. It is drier than some and not so irritating as others. It possesses the qualities of a Continental sea-side, together with the advantages of an island situation.

The *Hygrometrical observations* taken from the markings of an old instrument are imperfect, but still, in the absence of anything better, may not be altogether undeserving attention. The mean point between *damp* and *dry* being marked zero, the needle fell to either side, according to the state of the atmosphere. It obeyed all variations very regularly, and seemed sensible to all changes. By this, then, it appears that during the first part of January, from the 12th to the afternoon of the 17th, tolerable dryness existed, the needle often stood at zero or 0, marking 1 to the damp or dry side, during the forenoon or afternoon according to the wind or atmosphere. After this no markings occur on the dry side again, all being more or less damp, varying during the day, and from day to day, the effect of the rains. On the 10th, the marking is on the damp side, from 3 to 4, a few days before rain had fallen. Snow had appeared on the Peak, and though there was no wind, a sharp feeling could be perceived, not dry but moist. Next day 5 is marked on the damp side, rain during the morning, with damp east wind and clouds. The same may be noticed for the 12th. On the 13th all cloud and rain had disappeared, and 2 degrees to the dry side are marked. Towards evening a sea-breeze set in, and the needle set at $1\frac{1}{2}$ damp. On the

16th, 2 dry is again marked during the prevalence of a land wind from Africa. Next day, from 0 the needle set at 3 damp, and though the wind was still east, it was no longer irritating, but, on the contrary, moist and damp.

During the remainder of January and February, damp markings prevail, with showers, moist or sea-winds exemplifying their influence, the last-mentioned dry marking being an exception, caused by a dry land wind.

On the 19th of January the damp markings are only 2 or $2\frac{1}{2}$, though an east wind prevailed, exemplifying the fact that persons are too apt to call days damper, rawer, or colder than they really either thermometrically or hygrometrically are, for, as in this case, though the day did feel more raw and damp; it was, in fact, neither, being only $2\frac{1}{2}$ damp, and 68° mid-day temperature.

On the 15th February only 2 damp is marked for the afternoon, arising from a brisk, lively current of air from the west, tintured with the comparative dryness of land and the chill of snow, analogous to what occur on the Continent. But though thus derived or modified, the damp was only 2, while the temperature was 62° to 61° , 10 P.M. Two days after, the snow melting, rendered it more raw and damp.

From a fortnight to three weeks is the usual continuance of the rains both during the winter and spring; several days occurring both before and after, of so broken a nature as to prevent invalids from venturing out. The rain comes down in torrents, but the ground soon dries when the sun sets in with full uninterrupted ray. In some small districts near the foot of the hills and openings of the ravines, from the accumulated vegetable debris brought down perhaps only so far, and not washed away to the sea, slight effluvia may exist. In one defile near the high lands, ague from this cause is not unfrequent, but in the town it never occurs spontaneously.

From the foregoing statements, it seems as if Teneriffe offers advantages not possessed by other places, and which would be of great service to those with whom the extremes of dryness or moisture do not agree. It is more windy than other places, but it is drier. It is not situated on the Continent, and so possesses insular advantages, combined with others derived from its high mountain land, similar to what one meets on the land. It at present possesses good roads for carriage exercise; and, owing to the native tact of the islanders, seems capable of great improvement and cultivation. There are no houses as yet fitted for invalids, save one or two, but, being a free port, Santa Cruz offers peculiar advantages for transportation of commodities.

Very warm during summer, it possesses a high land cool retreat, while the heat never diminishes to an extent which could do any harm. The English already there, are hospitable and attentive; their properties and persons well taken care of by a zealous and excellent consul. A cemetery is now given for Protestant purposes,

and by this time, or during the present winter, a clergyman will be stationed there.

The resources as to food are good. Sheep are numerous and well fed. Milk, both cow's and goat's, is easily obtained. Fish, of various sorts, abound. Fruit, the orange, banana, and pine-apple, from the African coast, is in large quantities. The wheaten bread is very fair; but Indian meal serving the poor instead of our flour, less bread is used, comparatively speaking, than with us.

But great danger will arise, if care be not taken, and that constantly, against draughts and currents of air. Nothing is more easy than to keep a room sufficiently warm, but at the same time it is as easy to let in a current of air, refreshing though it may be, that will undo all the benefits.

As yet little convenience exists for invalid exercise, but little time will elapse before sledges or carriages will be numerous, while horses and ponies are always to be had, and camels for those who prefer an elevated seat and a see-saw motion.

Santa Cruz is easily reached by steamers to the west coast of Africa and South America, touched at by many vessels bound for Australia, Cape of Good Hope, and India, supplied by regular sailing traders for its oranges and cochineal, a constant source of communication is now open. Two Spanish steamers from Cadiz also have bimonthly communication with Santa Cruz.

Lying so far south, it is about forty hours longer passage than Madeira, having on the northern shores a rather rough sea. Mosquitoes exist all through the winter, destroying the repose and ruffling the mental tranquillity of any new comer.

The climate is between that of Madeira and that of the north shores of the Mediterranean. It is less languishing than the one, less exciting than the other. In this respect it may suit some who derive no benefit from either of the above localities, though as yet no proof can be adduced. That some have been benefited is certain, as I have seen one or two cases which, after passing a winter at Santa Cruz, felt much relieved. One was a severe cardiac disease, with angina, and he derived much benefit. Several others, again, more or less threatened with chest affections, have lived for years as comfortably as they could elsewhere, nay, with decided benefit, both at Santa Cruz and Orotava. Many constitutions cannot tolerate a depressing climate, while others again could not stand an exciting one. Leucophlegmatic temperaments do not agree with excessive stimuli, and so would not do in such an exciting air. Brisk sanguine temperaments would not suffer from the climate of Teneriffe, save in the summer heats, which can easily be avoided, and they would have very few depressing days to complain of.

In bringing forward these few unconnected facts relating to a part of the island of Teneriffe only, I do so merely with a view to supply an acknowledged deficiency regarding our information of the climate of this island. The facts have pointed out the worst

features of the winter climate; but if allowance be made for severity; if comparisons be instituted with the degree of temperature previously given, and calculations made from these data, the climate will appear warm, genial, unrelaxing, and pretty uniform from month to month. If it prove less relaxing than Madeira, and less exciting than Malaga, surely good will accrue to those who claim only partial benefit from either of the former.

Without prejudice in favour of this island or against that of its more northerly neighbours, it may be said, that the former does present some points of attraction in a hygienic sense, not found in the latter. It cannot be denied, that while many recover, and that wonderfully, from almost certain and expected deadly disease, numbers perish. That this, in many instances, is more the fault of the invalid than of the climate, is allowed. Some, again, are in such a state of debility when they arrive, that scarcely anything but death can await them. These considerations apply to all invalid resorts, east or west, north or south, and do not in the least detract from the main fact that to some a change of climate is often of infinite and most satisfactory service. The cases hitherto seen and treated in these resorts confirm this truth, and give us full warrant in treating future patients, other circumstances being favourable, in a similar manner. One only requires to go and look for himself to be convinced that in Madeira people are living, and likely to live, who, had they remained in this country, would long ago have paid their debt to nature. How it is that warmer climates act in bringing about a more healthy state is not yet solved, but if we remember that it is mostly in those who are in the primary stage that such a result has occurred, some clue may be obtained. The person is taken out of a damp, cold climate, transferred to one which, while it enables him to obtain several modes of exercise during all parts of the year, at the same time, from the greater heat, and consequent less requisite respiratory process, prevents, to a certain extent, the lungs from having an over much amount of duty. In other words, the heat of the animal economy being more easily kept up by the surrounding warm air, less blood will be sent to the lungs. This may appear paradoxical, but, in a warm climate, the tissues are not in the same shrivelled state as seen in many who suffer from cold in an English climate; the skin is called on to act more vigorously, while the circulation is, though regular and steady, less violent. If a patient makes little exertion fatigue will be unknown, and food not taken in large quantity. Thus we have a sufficient nutrition without overloading the body; we have the skin called on to act; the tissues in that state so well known after exercise in a heated room or under heavy clothing; we have no rapid or violent circulation, no laboured respiration, the blood itself not requiring to be so freed from carbonaceous particles—in fact, we have almost all the requisites for rest and quietude to the lung. In climates such as Malta or Malaga, where the air is exciting, another influence will be brought to bear

on the pulmonary tissues. From personal experience, we know that the dry exciting air of Malta has caused considerable, even serious inconvenience to several who had previously benefited from the climate of Madeira, and that similar occurrences have been noticed in other Mediterranean resorts. This merely proves, however, the often-repeated assertion, that it is only to certain constitutions and temperaments that either of these localities will bring satisfactory relief. But, even in this respect, Teneriffe enjoys apparent immunity from many of the inconveniences experienced in continental or Mediterranean residences. Isolated amid vast tracts of water, unaffected by continental winds on every side like Malta, lying nearer a well-known and much-frequented resort, Teneriffe is well calculated to play an equal part with Madeira in offering every possible opportunity for the benefiting of many invalids. Warmer, though perhaps more windy, it presents a brisk and less oppressive atmosphere than is to be found in the latter; while the exciting qualities observed in Malta and other eastern towns, are held in abeyance.

The excitement of the Mediterranean climates will influence the spirits, and, through them, the circulation, so that less exercise will probably be required, while a healing effect will also be exerted locally. Then, though still very great precautions and prudence will be necessary, some lively constitutions will thus be benefited.

In Madeira, again, where the heat is more of a calming, soothing nature, the local effect will act differently, requiring the same precautions, and doubtless leading to as beneficial results. For different constitutions will be benefited by different climates, and while the leucophlegmatic and puny temperaments are fostered in the one, the lively and sanguine will do well in the other.

Teneriffe enjoys a somewhat medium climate, and so will benefit either of these constitutions, and be of service to many of those who will fairly give it a trial.

It is not to extol Teneriffe that this paper is written; but rather to point out facts, with a view of alleviating suffering, or enabling some persons to spend their short lives more comfortably, and perhaps of prolonging them, and thus aid, be it ever so little, in the great cause of humanity.

ARTICLE IV.—*Two Cases of Snake Bites occurring in Scinde.* By Dr LOWNDS, Assistant-Surgeon, H.E.I.C.S.

1st CASE.—Hydrabad, Middle Scinde, beginning of June 1852.

This occurred to a Scindee living some fourteen or sixteen miles from Hydrabad, and who was not brought into the hospital there till twelve hours after being bitten. Variety of snake unknown.

He stated that he had received the bite the previous evening on the foot, and that he had taken some medicine given by one of the Meers,

and said to be a specific ; that he began to spit blood about three or four hours after being bitten, and that this blood-spitting had continued ever since. In person he was powerful and well developed. A bandage had been tightly applied below his knee.

Symptoms.—Two small punctured wounds were distinctly visible a short distance apart, a little anterior and inferior to the internal malleolus. No bleeding from punctured wounds. Coughed a good deal, and by this coughing brought up a considerable quantity of blood, which seemed to have no tendency to coagulate. Breathing natural, except when coughing was present. Pulse about 90 beats per minute, full. Expression of countenance haggard and anxious. He distinctly stated that he felt no pain whatever.

Treatment.—I united the two punctured wounds by an incision, and placed a small cupping-glass over the incision ; this drew a little blood, but not much—(still no tendency to coagulation). I ordered stimulants of sulph. æther and spiritus ammon. aromat., to be administered in full doses, if any change should take place in the character of the pulse. At this time the patient said he would not take any medicine. This was about 5.30 A.M.

At 1 P.M. the pulse had become much weaker, and the medicines ordered were attempted to be given, but he still obstinately refused them. Spitting of blood still continued ; he gradually became weaker and weaker, still refusing anything and everything, and he swooned and died at 5 P.M., or about twenty-fours after the bite.

2d CASE.—Shekapoor, Upper Scinde, 18th Aug. 1852.

A sepoy of 2d Belooch battalion was bitten by a snake, said by the natives to be of the cobra species. He came to the hospital at about 5.30, or 6.0 A.M., next morning.

General Appearance.—Slight but well made. About 25 years of age.

Symptoms.—Marks of fangs of snake a little anterior and inferior to internal malleolus of right ankle. Wounds not bleeding at all, nor had any application been used, a ligature only having been tied round the leg a little above the ankle. Countenance natural, and at this time no expression of anxiety. No pain any where, except a slight pricking in the wound itself. Blood-spitting had commenced about three or four hours after the snake bite, and still continued. This was the only abnormal symptom ; his pulse natural and breathing regular. The blood that was spat up had no tendency to coagulate, and exactly resembled that mentioned in the former case.

Treatment.—I made several small incisions—one to connect the two fang wounds, and one on either side, where there appeared some slight laceration. A cupping-glass was exhausted and placed over the wound ; it was soon half-filled with blood without tendency to coagulation. This had scarcely been done when the officer commanding the regiment asked if I had any objection to allow two Sepoys, who said they could cure snake bites, to try to cure the

man. I at once consented, only resolving to watch the case narrowly. I merely applied caustic to stop the bleeding. These natives first applied a poultice of small leaves (species of tree unknown) to the wound, and gave the patient a dose of croton oil seed. This latter produced considerable vomiting and much purging. 6 P.M., I again saw patient; spitting of blood still continued; countenance expressed some anxiety; pulse about 96, full. Bleeding had recommenced from cut in front, probably to extent of ʒvi. or ʒvij. Still the same sort of blood. I left directions to be instantly summoned on the slightest change. 2 A.M., I was hastily called as the patient was much worse. I found on my arrival that about half an hour before he had become weaker and weaker, and then almost suddenly had become insensible. I found him perfectly insensible; extremities cold and corpse-like up to upper part of his thighs and arm-pits. No pulsation could be felt at the wrist, nor could the beating of the heart be distinguished through the parietes of the chest. The action of the heart could only be recognised by pushing the hand up behind the sternum, and then a faint thrill could alone be recognised, much resembling the cardiac thrill felt in the same way on a new-born infant. The breathing could be recognised by placing the hand on the abdomen, which was much collapsed from purging by croton seeds. Wounds in foot still continued bleeding, but not very much; spitting of blood had not occurred for about two hours before insensibility came on.

I ordered grs. v. of bicarbonate of ammonia to be at once given, and repeated in five minutes; also a large enema of mustard and water, with ʒij. of ammoniated tinct. of valerian, and a large mustard poultice to be placed on the cardiac region. Men were set to rub the extremities diligently. For the first quarter of an hour little or no change could be perceived. The enema of mustard and valerian was returned. The following mixture was then ordered:— \mathcal{R} Chloroform, ʒi. ; arrack (native spirit), ʒij. ; mist. camp. ʒij. , M. ft. M. To take ʒi. , and repeat every ten minutes.

The mustard poultice was of little benefit as the mustard was not sufficiently finely ground, being prepared on the spot. After taking two doses of chloroform mixture, some improvement was perceptible. I thought, I felt a pulsation at the wrist, intermitting and very faint at first. At this time ʒij. of ammoniated tinct. of valerian were given with warm water, as an enema. Chloroform mixture was continued, and in about two hours all dangerous symptoms passed away. Warmth returned to the extremities, and the pulse became steady and regular. The chloroform was gradually discontinued. The blood-spitting did not return. The patient remained in hospital somewhat weak for a few days, and until the wounds healed. He was then discharged quite well.

Observations.—In the first place, I must observe with reference to the incisions and the application of cupping-glasses, that they were

rather done to inspire confidence than with a hope that any portion of the poison would be extracted, so long a time having elapsed since the bites occurred, and I thought it better to wait for some decided symptoms in regard to the state of the system, than administer any medicine without a special object in view.

Again, we must be struck with the remarkable similarity, up to almost the termination, nay, their almost identity. I think that in neither case was the result at all affected by the native medicine administered. These were given at different times, and were most probably different nostrums, and yet the progress of the cases how similar. Blood-spitting, at first the only prominent symptom, supervening in each at almost the same time, the blood non-coagulable in each case. The sinking in the heart's action commencing at similar periods, and the insensibility almost at the same. The state of the blood is most remarkable, and may at least lead us to the conclusion that death threatened through the circulating system, and not through the failure of respiration.¹ (This view has been taken by some, in reference to the action of snake bites.) Whether this poisoned or deteriorated blood acted on the great nervous centres, and thus, through the medium of the brain induced failure of the heart's action, or directly by depressing the nervous and muscular energy of the heart and great vessels, we cannot precisely determine.

In neither of these cases did the disposition to sleep, which is so commonly remarked, occur.

With regard to treatment in the second case, it was purely and powerfully stimulant. The power of chloroform as a diffusible stimulant is very great, and almost instantaneous in its effect on the circulation, and it was, I think, in this case, used with advantage.

ARTICLE V.—*Extraordinary Case of Mistaken Personal Identity.*

Communicated by ALEXANDER JOHN KINLOCH, M.D., Park, Drumoak, Aberdeenshire.

I AM induced to communicate the following case of mistaken personal identity from seeing an advertisement in some of the English newspapers requesting information regarding the body of a man who was found drowned in one of the rivers in the north of Scotland some years ago. When out early one morning, walking by the banks of the river Dee, on the estate of Park, and in the parish of Drumoak, county of Aberdeen, I was hailed by two of the men whose usual mode of livelihood is the rafting of timber, to come and see the body of a man which they had just discovered in the sand of the river. The river had been much swollen for two or three days previously, and had just subsided. The body was lying slightly

¹ I mean rather leading us to fear death by syncope than asphyxia.

imbedded in sand. The hooded crows had picked out both the eyes, but decomposition had made no progress. The body was that of a man between 60 and 70 years of age, and apparently belonging to the labouring class, was of the middle height, and rather stout. A pair of spectacles and a few halfpence were all the articles found on his person; but the body presented the following peculiarities:—The left ear was wanting, as also the first finger of the left hand, both having the appearance of having been lost in early life.

The body was conveyed to a suitable place of reception, and intimation sent to the different neighbouring parishes of the circumstance; an advertisement was also sent to the local newspapers, requesting parties to come and identify the body. Various parties made their appearance for the purpose of identifying the body, but in vain, till two young women (sisters) from the opposite parish (Durrus) made their appearance, stating that their father was missing, that he was a sawyer by trade, was in the habit of leaving them for weeks at a time, not telling where he was going—that he had lost the left ear and first finger of the left hand. The clothes of the deceased were shown them, which they at once apparently recognised as the clothes worn by their father; and on being shown the body itself, they both gave vent to their feelings in the usual feminine manner. After a short time, however, the younger sister, on making a more careful examination of the body, *expressed some doubt on the point, and stated that she did not think it was her father after all.* But finally, on the persuasion of her elder sister, left the house to make preparations for the funeral, under the impression that the body was that of her father.

The usual arrangements were accordingly made for the funeral, and the friends and relatives of the deceased crossed the river from the parish of Durrus, to the parish of Drumoak, at the upper ferry or west boat, and the body was duly buried in the churchyard of Drumoak. The friends, including the two young women formerly mentioned, crossed again into the parish of Durrus by the east ferry boat or boat of Drumoak, the boatmen of which, knowing the parties well, and seeing them in mourning, asked them who they had been burying, as he had never heard of any of their friends being dead; and on being told that it was the father of the two girls, he laughed, and said it was impossible, as he himself had ferried their father across the river not more than half an hour before, and also stated that if they went into the small public-house on the road side, he had no doubt but they would find him there. They of course did not believe the boatman, but called at the small public-house, when, to their surprise and amazement, they found him regaling himself with whisky, etc. The clergyman of the parish of Drumoak, on being made aware of this extraordinary circumstance, and having doubts as to the fact of the man's being alive, wrote to the clergyman of the parish of Durrus regarding him, who told him that not only was the man alluded to alive, but had been in the church the day before.

This man has since died ; but who the person was whose body was buried in the churchyard of Drumoak has never as yet been ascertained.

The unusual circumstance of two individuals wanting the left ear and first finger of the left hand being brought into the same locality, and mistaken, renders this case a very remarkable one.

Part Second.

REVIEWS.

Clinical Reports on Continued Fever. By AUSTIN FLINT, M.D., etc. Buffalo. 1852. Pp. 390.

Lectures on the Nature and Treatment of Fever. By J. L. CORRIGAN, M.D., etc. Dublin. 1853. Pp. 104.

Der Typhus in Irland beobachtet im Sommer 1852. Von Dr JOSEPH LINDWURM. Erlangen. 1853.

Observations on the Typhus of Ireland made in the Summer of 1852. By Dr JOSEPH LINDWURM. Erlangen. 1853. Pp. 99.

IT might be supposed by any intelligent person without medical education, that the subject of continued fever was one on which all physicians must hold doctrines nearly similar. For the extreme frequency of the disease, and the fact that it has prevailed in temperate latitudes from time immemorial, may be supposed to have long drawn the attention of the medical profession to its pathology and treatment. We need not tell our readers that this has been the case, and that the number of treatises on fever is vast indeed. But far from showing anything like unanimity in doctrine or practice, this profusion of medical literature merely indicates, that on one of the most important of subjects, the most accurate observers and accomplished of physicians have arrived at very opposite conclusions, and that even at the present time opinions the most irreconcilable are held by different sections of the profession. To one point only of discrepancy we propose to direct attention in the course of these remarks. We allude to the modern attempts to separate typhus fever by nosological definition from typhoid fever and from relapsing fever. Some fifty or sixty years ago, when the "essentiality" of fever was a favourite doctrine, and before *post-mortem* examinations were conducted and registered with care in our large hospitals, such attempts would have been deemed highly heretical. But when Broussais and his school attacked this doctrine of "essentiality," and endeavoured to account for all fevers by lesions in the solids—as in

the track of the alimentary canal for example—inquiry was excited, and doubts suggested which have not yet been removed. For although Broussais did not succeed in demolishing the doctrine of “essentiality,” he at least established the fact that the vast majority of the cases known in Paris as examples of “fièvre typhoïde,” are connected with a diseased condition of Peyer’s patches and of the mesenteric glands. On the other hand, the records of most British and Irish hospitals has positively demonstrated another fact, viz., that when death follows British typhus, these morbid appearances are exceptional. It was natural for pathologists to take up the inquiry at this point, and to endeavour to ascertain if the “fièvre typhoïde” of France and the typhus of England were not two different diseases. To this investigation many have devoted themselves, and none, perhaps, with more ability than Dr Jenner of London, whom we may cite here as the most powerful advocate of the expediency of nosologically separating “typhoid” from “typhus” fever. It is, however, possible, that had Dr Jenner conducted his inquiries in another school—as in Edinburgh or Dublin—and had these been extended over several epidemics, his conclusions might have been different, or at least less confidently expressed. Certain it is, that his views have not been adopted, either here or in Ireland, by those who have for many years been most conversant with the types and morbid appearances of our fever epidemics. It is very possible, we admit, that Dr Jenner may be *right*; before, however, it can be proved that he is so, there must be renewed investigation—series of cases must be collected in different epidemics and in different localities, and from a rigorous analysis of these the truth may perhaps be elicited.

These remarks have been suggested by a perusal of the short treatise of Dr Lindwurm. In Germany, as in France, the lesions of the small intestine are so commonly observed after death from typhoid fever, that a German pathologist must naturally incline to the doctrine which attributes the fever to the glandular disease. Not so Dr Lindwurm; he has twice visited this country, and in the course of his last visit went to Dublin, for the express purpose of satisfying himself of the identity or non-identity of typhus and typhoid fevers. We find that his observations have led him to the conclusions, which he shares with the heads of the Dublin school, that the diseases are not nosologically distinct, and that the affection of the ilium is a complication dependent upon certain circumstances, such as age, mode of life, diet, exposure, peculiarity of epidemic, etc. In his remarks upon Dr Jenner’s comparative view of the points of difference between “typhoid” and “typhus,” we find, however, that Dr Lindwurm is not quite fair to the Professor. For instance, in commenting upon the forms of the eruption, to which Dr Jenner attaches great importance, he writes—“I can only admit a difference in the amount of its diffusion (*Ausbreitung*), not in its form.”—P. 96. Now it is quite notorious that Dr Jenner’s remarks apply to the differences which characterise *well-marked* cases of ordinary and

of ilio-typhus, and that he admits the existence of cases in which a discriminative diagnosis is, from the appearance of the eruption at least, impossible. There is a very marked difference between the small, circular, rose-coloured and elevated, sparse and fugacious petechiæ of typhoid fever, and the dark mulberry-coloured exanthem of ordinary typhus. We know from experience that any attentive student can very soon learn to distinguish the exquisite forms of these eruptions. But we admit that cases very frequently present themselves in which the most practised eye is unable to refer the eruption to either type.

Dr Lindwurm, at the commencement of his treatise, says:—

“In the London new Fever Hospital, the head physician, Dr Southwood Smith, showed me one day a case of typhus which had relapsed into scarlatina; and the resident physician, Dr Sankey, showed me, on the same day, two cases of fever, easily distinguishable according to his views—the one a case of simple exanthematous typhus, the other of typhoid fever, or ilio-typhus. Thus, while of two physicians to the same hospital, the one admits the passage of typhus into scarlatina, and views both diseases, typhus and scarlatina, as modifications of the *same* morbid process, the other distinguishes two specifically distinct varieties of typhus, always to be discriminated during life, and each dependent upon a special contagious principle. The first was, according to my own opinion, a case of typhus which, during convalescence, had been exposed to the contagion of scarlatina, and contracted the disease; the others were two cases of typhus, one with, and the other without, gastric complication; both had a *perfectly similar* exanthema, and when I was unable in any way to satisfy myself of difference between them, Dr Sankey remarked, that ‘it required more experience to enable one to see the difference.’ I believe, that where there is a difference, it should at least be demonstrable.”—P. 7.

Had Dr Lindwurm been acquainted with that ever-memorable production of the Board of Health, the “Report on Quarantine,” he would have been quite prepared to hear something startling on the pathology of fever, as propounded by Dr Southwood Smith. But we confess our opinion that possibly a few visits to the hospital in the company of Dr Sankey might have enabled him to distinguish between the typhus exanthem and rose-coloured spots of “dothinen-teritis.” We find, however, from the following passage, that he is consistent in professing his inability to make the distinction even after his second visit to Dublin:—

“Some modern English and American authors, especially Jenner in London, profess to distinguish most decidedly the eruption of exanthematous typhus from that of ilio-typhus, and to make the differential diagnosis, if not always, at least in the majority of cases. I cheerfully admit that in exanthematous typhus the eruption, as a general rule, is much more diffused than in ilio-typhus, hence, indeed, its name; but I must decidedly deny the existence of all the characters (except this amount of diffusion) stated to distinguish the eruptions. Between exanthematous typhus with scanty, and ilio-typhus with abundant eruption—and such cases are not rare—I most decidedly pronounce the diagnosis from the eruption alone to be impossible. I gave myself the utmost trouble to recognise, in Dublin, the distinctions laid down by Jenner, but in vain. Stokes, Kennedy, Christison, Bennett, Todd, Tweedie, etc., all unite in asserting that the exanthem affords no sure criterion.”—P. 90.

Now, it is interesting to find that the conclusions to which his

independent observations have led Dr Lindwurm, are nearly in accordance with those of Drs Corrigan and Flint. Both of these physicians have had great experience in the treatment of fever, the former in Ireland, the latter in British America. Dr Flint has collected, in the volume before us, records of his experience in three American epidemics, and has laboriously analysed the whole in a goodly octavo volume. Dr Corrigan has, into a more moderate compass, compressed a large amount of practical information, acquired in the fever hospitals of Dublin, and which, in accordance with the spirit of the age, has in part already seen the light through the pages of our weekly contemporaries. We most cordially welcome the "Lectures on the Nature and Treatment of Fever" in their present more independent form.

It is, we say, interesting to discover that three observers, whose opportunities of research have been so ample and yet so varied, should have arrived at conclusions nearly identical upon the debated pathological point to which we have already called our reader's attention. All, be it observed, recognise the existence of "dothi-enterite," of "gastro-enteric," or "typhoid" fever; and admit a certain chain of symptoms to be highly characteristic of this disease; but all seem to agree in refusing to the intestinal lesion, commonly associated with these symptoms, all claim to be considered as the *primary* disease. On the contrary, all seem to regard the changes in the Peyerian patches and mesenteric glands as constituting a mere complication in the course of typhus fever; and support their expressed opinion by cases. We regret, however, to record here our deliberate opinion that, till certain *lacunæ* in these cases are filled up, we cannot accept them as of more value in a question of doubtful pathology, than Dr Lindwurm's difference in opinion with Dr Sankey. To illustrate our meaning we extract a case from Dr Corrigan:—

"Mary Cope, ætat 22, previously in the enjoyment of the best health, was admitted into the Hardwicke Fever Hospital on the 23d January, ill of *maculated fever*. There was nothing unusual in her case. She was soon marked convalescent; when, on the 5th February, fourteen days before her death, she complained of debility and of diarrhœa; her tongue became brown and dry in the centre, but not furred; there was no tenderness of abdomen, nor tympanitis, but there was gargouillement over the cæcum; and the stools were like gruel, but neither mucous nor bloody, nor was there any tenesmus. The pulse became quicker and weaker. On the 18th February she required wine in considerable quantity; the diarrhœa became uncontrollable. On the 19th February, fourteenth day of the attack, she died.

Post-mortem Examination.—The peritoneal covering of the abdominal viscera was sound, but the ilium and a portion of the colon presented follicular enteritis in all its stages. The greatest intensity was at the ilio-cæcal valve, the entire circle of which was occupied by a depressed, jagged, greyish, irregular ulcer. In the ilium both the isolated follicles and the 'glandulæ agminatæ,' or glands of Peyer, were attacked. The sites of the affected 'glandulæ agminatæ' were marked by oval ulcers, while around and above them isolated follicles were seen in similar ulceration, but not to such a degree. In addition to these ulcerations, the isolated follicles in both the lower portion of the ilium

and the upper portion of colon presented every stage of the disease. Some follicles were just protruding under the mucous membrane, filled with a cheesy-looking purulent matter, the mucous membrane around being swollen, red, and prominent, and from the orifices of the follicles looking like depressions, those follicles bore a strong resemblance to variolous pustules. In others the matter was in such quantity that the affected follicles presented the appearance of spherical projections, attached by pedicles, and covered by mucous membrane, hard and firm to the touch, while in others, still more advanced, the follicles had gone on to ulceration, destroying the mucous membrane, and leaving only a grey slough of cellular tissue in the place of the follicle itself. The appendix vermiformis was swollen and congested, and, on being slit up, was found distended with a tenacious purulent fluid, and some of the mesenteric glands were infiltrated with purulent matter. This was a case of great rapidity and of equal severity. In most cases the disease is bounded by the ilio-cæcal valve. In another case, occurring about the same time, which ran a longer course, the lower part of the ilium was found extensively ulcerated, the ulcers presenting different appearances; some of them presenting the striking resemblance already described to the pustules of variola, others having the appearance of white cicatrices, as if traced out by a snail traversing a zigzag course along the mucous membrane; while others again, with loss of substance in the centre, but with soft pulpy areolæ, projected above the surrounding mucous surface. In this case, as is most usual, the disease was confined to the small intestine."

Now, if it were distinctly shown, first, that Mary Cope had *maculated or measly* typhus, and that she after a fortnight's illness, and, possibly, a week's convalescence, became affected with "dothinerite," and the eruption described as characteristic of that complaint, her case—especially if similar ones were appealed to—would be of extraordinary interest. We should then be forced to admit, either that ordinary typhus may pass into "dothinerite" instead of true convalescence, or that Mary Cope, after running through the whole course of a typhus fever, while under the very eyes of her hospital attendant, contracted (possibly by contagion) a new disease—dothinerite—and died of it. Dr Corrigan obviously inclines to the former supposition, but we repeat that, in support of his opinion, the case is worthless. For we know not *how long* Mary Cope had been ill of fever before her admission, on 23d January, into Hardwicke Hospital—we know nothing of the eruption except that it was "maculated"—the date of crisis, or of commencing convalescence, is not stated—but *thirteen* days after her admission there is a note of debility and diarrhoea, followed promptly by the ordinary symptoms of "dothinerite" (minus the eruption, which is not mentioned), and on the 27th day after admission by death. Dr Jenner, and those who follow the same creed, may adduce this case, as it here stands, in support of their doctrines with quite as much effect as Dr Corrigan. We allude to its imperfections now, merely to show how sound doctrine may be confounded by imperfect facts, and to stimulate the attention of our readers to the possible occurrence of similar cases in their own experience, and to the great benefit to science which may accrue from accurately observing and recording them. It is too generally thought, especially as we know, among students, that the subject of continued fever is exhausted, and its pathology perfectly

understood or at least recognised. Far from such being the case, we may say that the advances which morbid anatomy and the application of the means of physical research to the study of medicine have made of late years, render the whole subject of fever one of doubtful pathology, which can only be cleared up by the most searching analysis of materials collected by accurate and industrious observers. And in the absence of a sufficient number of *accurate* facts, but supported merely by general impressions of what we have ourselves witnessed in Edinburgh, especially during recent epidemics, it may be presumptuous in us to say, that we regard the so-called "dothinerite" as an accidental complication of typhus, and have often met with cases in which Dr Jenner's characteristics of the eruptions were in vain appealed to in order to settle the diagnosis in a doubtful case. We may add a wish, that we had some standard plates of the different forms of eruption. The attempts at illustration which we have hitherto seen, convey a very imperfect idea of what they are meant to depict, and yet there seems no sufficient reason for such uniformity of failure.

Practical Observations on Aural Surgery, and the Nature and Treatment of Diseases of the Ear, with Illustrations. By WM. R. WILDE, Fellow of the Royal College of Surgeons in Ireland, Surgeon to St Mark's Ophthalmic Hospital, etc. etc. London. 8vo. Pp. 506. John Churchill. 1853.

It is an impression, we believe, not more generally than accurately maintained by the profession, that up to the present period our knowledge of the pathology and treatment of diseases of the ear has been in the last degree unsatisfactory. The concealed position of the organ, and the minuteness and complexity of its parts go far to account for the fact; and suggest that a very peculiar as well as patient investigation and elucidation are necessarily desiderated. Good service has been effected within the present century by such men as Saunders, Kramer, Wharton Jones, Toynbee, and others; and yet we cannot but rejoice that the subject has been undertaken by the talented author of the work before us, who, acquainted with literature as well as science, his mind enlarged by foreign travel and observation, commanding, moreover, very peculiar advantages as the founder of an hospital in the Irish capital for the treatment of diseases both of the eye and the ear, now in the twelfth year of its existence, has thus acquired a large amount of skill and experience, which he freely communicates, not more to his own credit than to the benefit of the profession. The work is in some degree a revision and digest of memoirs previously published by the author, and without pretending to be a complete system of aural surgery, it sup-

plies us with a highly practical treatise concerning the most common and urgent affections of this organ.

The work is divided into seven chapters; the first being mainly occupied with a succinct BIBLIOGRAPHY, and the last, in the shape of an appendix of sixty pages, being a digest of our present knowledge concerning DEAF-DUMBNESS. Both of these are interesting and instructive; but to them our limits forbid further allusion. The five remaining chapters are severally occupied with the means of diagnosis; the statistics and nosology of ear diseases; then the diseases, 1st, of the auricle, mastoid region, and external meatus; 2d, of the membrani tympani; 3d, of the middle ear and eustachian tube; 4th, the diseases of the internal ear; concluding, 5th, with an account of otorrhœa. We cannot attempt more than glancing at a few items in their order.

No remark can be more true, than that until we know the exact nature of a complaint, our treatment can be nothing better than empirical; and to discover the real state of the different portions of the ear in their complicated pathology, is a task which cannot be successfully accomplished without a considerable amount of knowledge, and much delicate tact and ingenuity. No circumstances, moreover, can perhaps more strikingly illustrate the backward state of the art in this department—till within a very recent period—than the paucity, not to say the want of the suitable and necessary means of investigation—a desideratum which our author has done much to supply. Hence, the chapter “On the Means of Diagnosis and Application of Remedies” is, in our apprehension, one of the most useful and interesting in the volume. As to the plan pursued, we may, in a word, observe that, at the commencement of each chapter, the author supplies, 1st, a brief anatomical description of the parts concerned in the affection under consideration, and a concise account of their more remarkable malformations, and then proceeds to the etiology and the required treatment. The business way in which he proceeds in his hospital may be learned from the following sentence. Each case as it presented itself was accurately investigated in the presence of a class of advanced students and young medical men; the most prominent symptoms were noticed, the pathological condition of the parts demonstrated, and a few remarks made upon the cause of the disease, its progress and treatment. Occasionally the student under his direction conducted the examination. A shorthand writer, familiar with medical terms, was always in attendance, who recorded with accuracy what passed. These notes, when reduced to writing, and corrected by the author, were compared with the appearances presented upon the patient’s next attendance. By this means a vast amount of time and labour was saved.—The hospital registry, we need scarcely remark, is thus kept with commendable accuracy. It consists of sixteen columns, and at a glance supplies much useful information. The author, moreover, thus supplies us with a nosological arrangement of aural diseases the most com-

plete and satisfactory we have yet seen, and which might profitably be generally adopted.

Every one who would undertake the management of an aural case should be instinct with the fact, that the one great point is accuracy of diagnosis; to arrive at which it is indispensably necessary that we should be thoroughly acquainted with the best mode of conducting an examination. For this essential matter the author discards as unsatisfactory and deceptive all artificial light, and directs his patient to be placed opposite strong, direct sunlight, with the head inclined at such an angle that the sun's rays may fall directly upon the organ.

"We first then carefully observe the condition of the chonca, the external meatus, mastoid process, infra-zygomatic region, and the space immediately behind the lobe of the ear. The auricle, its various folds, its colour, its temperature in particular, its thickness as learned by feeling the helix between the fingers, and the angle which it forms posteriorly with the cranium, together with the position, size, shape, and colour of the external meatus. The upper rim of the helix should then be grasped between the finger and thumb of one hand, and drawn upwards, backwards, and outwards, while the thumb of the other hand, placed in front of the tragus, by drawing it and the integuments forwards upon the zygoma, exposes the outer third or more of the auditory canal to view. The small gorget-like instrument may often advantageously be employed in examining the external aperture. The finger should then be pushed deeply and firmly upon the moveable root of the tragus, and backward into the depression between it and the articulation of the jaw. Whilst the finger is retained in this position, the patient should be desired to open and shut the mouth, and the amount of pain or inconvenience experienced by pressure in those two different positions of the jaw is accurately noted. The middle and fore fingers should likewise be inserted deeply behind the ramus of the jaw towards the styloid process, and notice taken of the sensation there experienced."

Directions equally minute and specific follow in relation to the careful examination of the mastoid process, and parts adjacent, which should be carefully studied. And once more,—

"The auditory canal, and the external surface of the membrana tympani should be examined. To effect this, and to explore every portion of the surface of these parts, it is necessary to resort to the mechanical assistance of the speculum; first taking care to remove any impaction of wax, accumulated discharge, or other mechanical impediment which may exist, and obstruct our vision. If this obstruction is complete, and that we have reason to suppose that it is the chief cause of deafness, the employment of a syringe, and some *plain warm water*, is the best mode of removing it; but if the obstruction merely co-exists with other, and particularly with some of the inflammatory affections of the meatus or tympanal membrane, or if it be only partial, and consists of portions of detached cuticle, hairs, or scales of hardened inspissated cerumen, it is better to remove these gently with a pair of fine forceps, because the very act of syringing, even with warm water, causes in the healthy ear an increased vascularity, which will mask the actual amount of disease present. The same remark applies also to slight otorrhœa.—Having found, remarks the author, that the handles of the instruments introduced through the tubular speculum and the fingers of the operator interfere to a certain degree with the direct sun-rays, I have latterly had instruments constructed with an angle in the shaft, the utility of which is at once manifest."

We willingly introduce these extracts, not so much as exhibiting

the author's style and manner as showing the minute and finished character of his investigations. And did not our own observation strongly confirm the author's assertions as to the lamentable want of ways and means of exploration, and the required tact, we should not so long insist upon such matters; and now remark that, with not less precision and energy, the author dilates upon the means required for the more recondite examination so frequently required in cases of this class. Thus it is with respect to the use of the silver *tubular speculum*, which, he remarks, "has now become a necessary appendage to the assortment of instruments which the surgeon usually carries along with him." The best mode of ascertaining the integrity of the tympanal membrane is duly insisted upon; and whether, on the one hand, it is perforated, or, on the other, the eustachian tube is obstructed. Where there are indications of the latter condition, proceeding from accumulation of mucus, blood, pus, or other matter, then the aural surgeon must proceed, no contra-indications existing, to the exploration of the canal by the mechanical means of the pump, an elastic tube, and catheter, while the results are carefully noted by means of the stethoscope, or by the ear applied externally. This operation, we need not remark, is not unattended with difficulty, and it has been frequently stated with danger. With suitable apparatus, however, and competent knowledge, the risk is but small and the advantage is correspondingly great. The forcing-pump, its construction, and application, are minutely described. Kramers's is described as the best and most easily managed; yet our author has modified it, giving a minute description, which must be studied in the work itself. When the parts are in an inflamed and irritable state, the author remarks that the only solid instrument which he ventures to employ in exploring the eustachian passage, and that only for a short distance, is an ivory bougie, rendered flexible by having the earthy matter removed by immersion in an acid, and the point of which, for an instant at least, has been previously softened in water, so as to resemble a piece of gelatine.

In ascertaining the degree of deafness in every case a watch should be approached gradually to the ear, till it gets within the hearing point, and should be again applied to the auricle and gradually removed to a distance. The hearing should likewise be tested with the mouth open as well as shut, and it should be tried both before and after the inflation of the tympanum, as in most cases the act will produce a material difference in the amount of hearing. The watch should next be applied both behind and in front of the auricle, and to the forehead, and also placed gently between the teeth of the patient, and the amount of hearing thus obtained likewise noted. It is quite necessary, if we wish to watch the progress of a case, not only to conduct these observations with great care, but also to take a written note of the "hearing distance," the first and each subsequent time we see the patient.

The condition of the throat, the tonsils, and back of the pharynx

should next be inquired into, particularly as it respects the mucous membrane. The finger, moreover, should be introduced far into the mouth, and its point made to press firmly upwards and inwards beyond the arch of the palate, opposite the mouth of the eustachian tube, and the degree of inconvenience it produced observed.—These detached hints must be accepted as a sample of the author's instructions as to diagnosis. We subjoin a few hints as to the best means of employing the appropriate remedies.

Though most of the diseases of the organ of hearing are originally of an inflammatory character, yet it is seldom necessary to resort to general bleeding; while local depletion is urgently required by cupping as near the affected parts as possible, and also by *leeches*. This latter expedient is usually the most successful, and they should be applied immediately round, and within the edge of the external meatus, in the fossa behind the tragus, and, if necessary, in front of that prominence, in the hollow formed by depressing the jaw. From four to six leeches may be readily attached round the meatus; and in this situation, according to our author, they will produce more immediate and permanent relief than three times the number fixed behind the auricle. The application in front of the tragus is also much more effectual than upon the mastoid region. When, however, the latter locality becomes the seat of inflammatory action, they should also be applied freely to it. "I do not," states the author, "know any painful affection in which leeches applied in the manner directed produce the same amount of immediate relief as in disease of the ear. They should be had recourse to again and again, even upon the same day, and applied in numbers to relieve the paroxysm of pain, as well as to lessen the degree of redness and vascularity observable in the inflamed parts."

In chronic affections, where there is much thickening of the *membrana tympani*, our author speaks highly of the beneficial results proceeding from the *tartar emetic ointment*, applied to the mastoid region. In neuralgic cases, with pain over the side and back of the head, he has found compound camphor liniment, with extract of belladonna, one of the most useful applications; and in childhood, strong tinctures of iodine, with some iodide of potassium, applied with a camel-hair pencil. The application of heat and moisture is often particularly grateful, steaming the ear by holding it over the vapour of some very hot water placed at the bottom of a long narrow vessel, medicated with *hyosciamus*, opium, belladonna, or with the ordinary decoction of marsh-mallows, camomile, or poppy-heads. A warm lint-seed poultice, removed every two or three hours, is often also very useful.

Mercury, however, in the hands of Dr Wilde, is the medicine of all others which acts most beneficially in diseases of the ears, simply on account of its specific efficacy in arresting and controlling inflammation, and removing its products. But as there are a variety of aural affections to which it is applicable, so are there a variety of

modes of administering this powerful mineral, and a diversity of preparations each specially applicable to the particular stage of the disease, the class of symptoms, or the peculiar habits and constitution of the patient; and as a rule, the author states that the modes of exhibiting it in ocular affections will serve as a safe guide for prescribing it in diseases of the organ of hearing. In the most violent inflammations of the fibrous structures of the *membrani tympani*, the *periosteum* lining the cavity of the *tympanum*, the *eustachian tube*, or the deeper portions of the *external meatus*, and also the inflammations of the *internal ear*, when such can be diagnosed, as well as the specific inflammations of a *rheumatic* or *syphilitic* character, where actual *ptyalism* is indicated, we must introduce it quickly, and in such doses as will bring the system under its influence at once, just as we should do in inflammations of analogous tissues in the eye. In such cases, small and frequently-repeated doses of blue pill and calomel, with opium, will most speedily produce the desired effect. In the more chronic and sub-acute forms, again, of aural disease, where the mucous membrane is often extensively engaged, mercury as an alterative often proves a valuable remedy. The most efficacious form, however, in which mercury may be employed, is in the form of the *oxymuriate*. Combined with peruvian bark, according to our author, it is almost a panacea for most of the strumous inflammations in childhood and young people; and its power in controlling strumous ophthalmia, corneitis, iritis, etc., extends equally to the cure of kindred affections in the ear. "It is the best remedy I know," says Dr W., "for inducing absorption of lymph deposits in the *membrani tympani*, and in general thickening and opacity of that structure, as well as in very old cases of chronic inflammation of the membrane of the *cavitas tympani*."

It is of set purpose that we have dwelt so long upon the elementary topics brought under review; and this from the conviction, not hastily adopted, that they are those that are most likely to be useful to the profession generally. Another consideration which has also weighed with us, is the firm persuasion that Dr Wilde's volume cannot fail to be considered as a compendious, safe, and satisfactory work of reference for the practitioner, when he meets with cases of this very frequent and dangerous class of disorders to which our attention is now directed, and should therefore be introduced into our public professional libraries, as well as placed on the book-shelves of those who can indulge in the luxury of works of superior merit.

We have stated above that St Mark's Hospital is appropriated to the treatment of diseases of the eye, not less than to aural disorders; and much benefit accrues from the comparison which these two classes of disorders mutually reflect upon each other. The number of ear cases contained in the register amounts to the goodly number of 2385, which are catalogued as follows:—579 were simply cases of impaired hearing, produced by impaction of the external audi-

tory passage with cerumen ; 114 were of so-called nervous deafness ; 25 of tinnitus aurium, unaccompanied at the time by deafness, or any apparent disease ; 14 of otalgia ; 7 of deaf-dumbness, either congenital or acquired ; 2 of accidental hemorrhage from the tympanal cavity ; 7 of congenital malformation ; 20 of collapsed membrana tympani ; and 2 of tumours of the auricle ;—making, in all, 770 diseases of the ear not directly traceable to inflammation, or its effects. The number of eye cases again in the register, within the period, amounted to 11,233 ; and a few detached sentences must suffice to elucidate the interesting analogy. When discoursing on the morbid state of the membrana tympani, the author remarks :—

“ In ten instances only, of what is called nervous deafness, was the membrana tympani found natural. In 176 cases it was thickened and opaque, in whole or in part. The opacities varied as much in shade as the same form of disease presents in the cornea, from a slight nebula to that of a dense white leucoma ; and the analogy between the diseases of the two structures, the cornea and the membrana tympani, can only be appreciated by those conversant with both.—Sometimes it presented a slight opalescence, a skimmed-milk hue ; at others a general dense opacity ; and in those cases the membrane was most frequently collapsed. In some cases the opacity presented a matted appearance, like that seen on the back of the cornea in aquo-capsulitis ; in others, it appeared like mother-of-pearl. The most frequent site of isolated dense opacity was the inferior attachment of the membrane, where it presented a crescentic white band, about a line in breadth, and somewhat resembling the arcus senilis.—Again, in some instances, the whole surface of the membrana tympani was not only of a deep red, but also granular, like that of a deep raspberry, or the appearance so frequently presented in the inside of the upper eyelid ; such were cases of long-continued otorrhœa.—And once more, as it respects acute myringitis : as we denominate that form of internal ophthalmia which chiefly or primarily attacks the iris, an iritis, although in the severer kinds of that affection, several, if not all of the other textures of the eye eventually become engaged ; so in the inflammations of the membrana tympani, we must expect that sooner or later the adjoining textures will participate in the unhealthy action.”

We find, however, that our exhausted space compels us to close ere we have pointed out even an infinitesimal portion of what is calculated to inform the student, and improve the art, in this difficult department. In his chapter upon “ the wounds and injuries of the auricle,” the author inclines to the idea, that when the external auricle has been entirely removed, as with a sabre, it has, under able treatment, been united again, by union, by the first intention. In acute myringitis, when the disease, threatening and persistent, has not yielded to milder remedies, the author remarks :—“ I am now so fully convinced not only of the utility, but of the urgent necessity of employing mercury, that I do not hesitate to recommend its use in the early stages of all such affections. A pneumonia, a pericarditis, an inflammation of a large joint, or a serous cavity, or an iritis, may, it is true, get well by simple depletion, etc. ; but will any experienced practitioner of the present day risk such a case without having recourse to mercury ? ” The author’s discussion on tinnitus aurium is ingenious and suggestive, and his account of

syphilitic myringitis, of which several cases are supplied, is, we believe, for the first time brought before the British public. His descriptions, too, of otitis with partial paralysis, of otorrhœa, and polypus, are able and judicious; whilst the complete cure of deafness by an artificial membrana tympani is alike interesting and useful.

With every disposition to do justice to our author, we confess that, with our limited space, we have not succeeded according to his due, and our own wishes. His work is an admirable monograph on a most difficult and important department of our profession; and supplies—very much from original sources—a desideratum which has been long sought for. We cordially congratulate Mr Wilde on his laurels, and hope he may be long spared to pursue his honourable career, and do credit to his profession and to his country.

Surgical Anatomy. By JOSEPH MACLISE, Esq. Second Edition.
London, John Churchill. Parts I. and II.

IN noticing a work which has already found so much favour with the medical press, and the profession generally, it may seem superfluous to do more than congratulate the author on the successful result of his undertaking. But holding, as we do, that such success should stimulate to further exertion to improve the subsequent editions, we confess that we are not quite satisfied with a mere reproduction of the plates of the first edition.

Viewed generally as a series of plates of regional anatomy beautifully executed, and published at an extremely moderate price, the work was unquestionably deserving of the favour it obtained. But if its absolute value be considered as professing to give the best views of the surgical anatomy of the body, or if it be compared with what Mr Maclise had previously done in illustrating Mr Quain's *Surgical Anatomy of the Arteries*, the work had defects, and was capable of much improvement; and this we think might have been accomplished at little expense or trouble by an author possessed of the artistic and anatomical qualifications of Mr Maclise.

What we consider to be the defective points of these views are—1st, In the planning of the dissections from which the views are taken; 2d, An occasional disproportion between some of the dissected structures, as compared with the reduced size of the body in the plate. These defects are perhaps more visible in the regions of the head and neck than in any other part of the work. The former is evident in Plate III., which, but for the defective plan of the dissection, is one of the best, and certainly one of the most artistic of the series. In the view given in this plate we at once see that, from the whole of the sternum and part of the clavicles having been removed, the ribs and upper extremities, deprived of their natural connections,

fall away from the mesial line, and the clavicles are also dragged down by the weight of the arms; whilst the head being inclined backwards, the important natural relations of the parts at the root of the neck are lost. Thus, the bifurcation of the innominate artery, instead of corresponding to the sterno-clavicular articulation, or a very little above it, lies, even in the reduced sketch, at least an inch and a half above the clavicle, whilst the left innominate vein is stretched and dragged from its natural position. Now this disturbance of parts can be perfectly avoided by a plan of dissection, which we have long used for demonstrating the surgical anatomy of this region. By sawing through the clavicles very obliquely, commencing about one-half of an inch external to the clavicular attachment of the sterno-mastoid, and then making a transverse section of the sternum, about two inches below its upper margin, with very little dissection the upper part of the sternum, and the portions of the clavicles connected with it, together with the sterno-mastoid hyoid and thyroid muscles, can be reflected towards the neck, so as to expose the deep-seated parts in their exact natural relations, and they can be accurately replaced if we wish to review the more superficial relations of the region. The thoracic view can be obtained by making another transverse section of the sternum, about an inch and a half below the other, and then by dividing the costal cartilages from the fifth downwards, or even the corresponding ribs themselves, a large flap can be reflected towards the abdomen so as to expose the thoracic viscera.

The second defect, a disproportion in drawing, appears to us to exist in Fig. 3, Plate IV. and Plate VII., in which the carotid and subclavian arteries, as also the section of the mastoid muscles in Plate IV., are represented of nearly their natural size, while the drawing of the head and neck is reduced to about one-third of the natural size.

There are some points also, which though of minor importance, jar sadly with the ideas of those accustomed to practical dissection, such as the artificial look of the extremely lobulated surface of the parotid in Plate V., and the lively expression of countenance of some of the dissected subjects. Such defects are common enough in anatomical plates, but we scarcely expected them from an artist like Mr MacLise.

But while we feel bound to make these critical remarks, we do so in the spirit of kindness, and the rather, because, admiring as we do the high abilities of Mr MacLise, we wish to prevent him resting satisfied with what he has done, and to urge him to fresh exertions in a work which, from a rare combination of anatomical knowledge and artistic power, few men are so well fitted to advance.

Since writing the foregoing remarks we have received the third part of Mr MacLise's work, which contrasts most favourably with the preceding parts; and as we admire the excellence of plan and artis-

tic beauty of execution displayed in the views of the axilla, we only regret the more that the author should not have given a new and improved series of views of the regions of the head and neck in the present edition. We would strongly suggest a supplemental fasciculus, containing a view of the cervical region, different in plan from those he has already published, and views of the important relative anatomy of the parts at the angle of the jaw, and of the submaxillary and pterygoid spaces, of nearly or quite life size, as necessary to render the work complete.

The Elements of Materia Medica and Therapeutics. By JONATHAN PEREIRA, M.D., etc., etc. Third Edition, etc., Vol. II. Part II. Pp. 2316. London. 1853.

FROM comparatively small beginnings, this now great work has swelled out into a treatise of 2316 closely printed octavo pages. The name of *Elements* is no longer justly applicable. It is a system of Materia Medica, and an elaborate one—the most elaborate which has been brought out in modern times in any language. Originally designed by the author to be a text-book for students, it has become too comprehensive for that purpose, and is really now rather a work for consultation by practitioners. In this high position it will probably long remain. No work, which can be compared with it, has appeared in an English dress, or indeed in any other tongue, since the lectures of Dr Alston on Materia Medica were published in 1770; and it is not likely that any one else will be induced to undertake soon a task so herculean as the compilation of another complete system of Materia Medica,—so long at least as the branch of Therapeutics remains in its present unsatisfactory position.

It would be easy to pick out faults in the execution of so gigantic an undertaking as that which Dr Pereira eventually planned for himself. But they are faults of exuberance, rather than of deficiency or error; and these are counterbalanced by such a catalogue of excellencies, that, mourning, as all must do, the recent untimely death of the author, in the flower of his age as a medical philosopher, it would be ungenerous to dwell in the spirit of minute criticism over a production, for which medicine must long lie under a deep debt of gratitude to him.

Among other excellencies, it may be safely said, that few authors in medicine have bestowed so much pains, or exercised such impartiality, on the brief historical notices which he has introduced in the course of his description of the special articles of the materia medica, and their uses. There are few discoverers or original observers, who will not find in Dr Pereira's elements, that they have been done ample justice to in this respect.

It is with no small astonishment, therefore, that we miss this spirit

of impartiality in his editors, who have finished the last part of the second volume of his work. It seems that Dr Pereira had pretty effectively completed before his death the pharmacy of the several topics treated of in the present volume; but that much was left for his friends to edit, or compose, in the branch of Therapeutics. As a test of their mode of handling their task, we naturally turned to the most important of all therapeutic discoveries since the publication of Dr Pereira's last edition, viz., chloroform. Here we find such a depreciating statement of its services to surgery, midwifery, and the practice of physic, as we venture to say could not have been produced by any two members of the profession, of any authority in it, except among their infatuated brethren of Guy's Hospital, London. The marvellous conclusion to which they come, is, that "when every care is taken, and no obvious disease can be detected in the internal organs of the patient, it may sometimes be justifiable to recommend the inhalation of chloroform, in order to secure the patient from suffering." P. 1983. These are the very words of the editors, Drs Alfred S. Taylor and G. Owen Rees,—though the reader will scarcely believe it.

Every man, indeed, is welcome to his own opinion on such matters; and the extremely faint praise of the editors will of course go for what it is worth. But we submit that they had no right to sink the name of the discoverer of the actions, virtues, and uses of chloroform. This edition of Dr Pereira's *Materia Medica* will be referred to in all succeeding time by those who desire to obtain the most complete and precise account of the state and history of therapeutic science in the middle of the nineteenth century. What will they suppose then, when they find that in such a work, published only six years after the discovery in question, the only notice taken of a discoverer is an obscure allusion to an unknown American having ascertained, or thought, in 1831, that "chloric ether was a grateful and diffusible stimulant?" Every page of the article on the action and uses of chloroform is literally spotted with the name of Dr Snow; and an entire page is deformed with a hideous wood-cut of one of that gentleman's ridiculous engines. But throughout the whole article, occupying five pages and a half, there is positively not the most distant allusion to the Professor of Midwifery in the University of Edinburgh, except in the following line: "Dr Simpson, however, has used as much as eight fluid ounces in thirteen hours, in a case of labour." P. 1979. For anything that appears in the book, this is all, according to Dr Alfred S. Taylor and Dr G. Owen Rees, that Dr Simpson has done for chloroform.

Now, we apprehend that some explanation of so very extraordinary an exercise of editorial license is very much to be desired by the friends of these gentlemen.

Part Third.

PERISCOPE.

PHYSIOLOGY.

The Functions of the Auricles of the Heart, and the Influence of the Contractility of the Lungs and of the Respiratory Movements upon the Circulation of the Blood. By Dr SKODA. Schmidt's Jahrbücher, July 1853. (Translated by W. O. Markham, M.D.)

I.—ON THE FUNCTIONS OF THE AURICLES OF THE HEART.

In accordance with generally received opinion, the contraction of the auricles immediately precedes the ventricular systole, and so forces the blood into the ventricles. In the opinion of Baumgarten, Hamernjk, and Nega, the closure of the auriculo ventricular valves is caused by the blood which is driven into the ventricles by the contraction of the auricles.

Now the contraction of the auricle must commence either at those points where the veins enter, or at some other part of the auricle. In the first case, the blood in the auricle will be entirely separated from the blood in the veins, and a reflux of blood from the auricle prevented; the blood also in the orifices of the veins will be driven backwards, for the orifices contract simultaneously with the auricle; the veins, consequently, during the continuance of the auricular contraction will become distended and enlarged by the incessant flow of blood into them. But if the contraction of the auricles commences not at the opening of the veins, but at some other point, then will the blood be forced partly into the ventricles, and partly backwards into the veins, which again, as in the last case, will be distended by the continued flow of blood into them.

Thus each contraction of the auricles, by which blood is driven into the ventricles, will produce likewise a reflux into, and distension of the veins. Now a special consideration of the condition of the right auricle shows, that in a healthy individual, placed horizontally, and whose cervical veins are moderately filled with blood, every contraction of the auricle, which causes closure of the tricuspid valves, must of necessity produce increased distension of the cervical veins and expansion of the valves in those veins.

The objection, that the interruption to the movement of the blood in the vena cava is of too short duration to produce distension of the cervical veins, is answered by the fact, that pressure upon one of these veins made by the finger is instantly followed by its distension; the idea, also, that the backward flow of blood is too small to be propagated as far as the cervical veins, and to cause expansion of their valves, cannot be sustained when we consider that the reflux of blood into the veins, produced by the auricular contraction, must be as forcible as the current which is driven forwards into the ventricle; and that the reflux of blood into the veins of a healthy person lying in a horizontal position (when the action of the blood in the cervical veins and in the ventricle becomes equal as respects its gravity), must be sufficient, to distend the valves of the cervical veins, if the current directed forwards into the ventricles is powerful enough to cause distension of the walls of the ventricles, and closure of the tricuspid valves.

From such considerations of the mode in which contraction of the auricle is produced, it would seem, that certain phenomena observable in the cervical veins might be made available as interpreters of the condition of the right auricle, and particularly for deciding the question, whether the contraction of the right auricle causes closure of the tricuspid valves, and, inasmuch as the con-

dition of things in both sides of the heart must be considered as alike, whether the contraction of the auricles produces closure of the auriculo-ventricular valves, antecedent to the ventricular systole.

The following are the chief phenomena observable in the cervical veins under *normal* and *abnormal* conditions. *Normally*, and in the upright posture, the cervical veins are not distended. Sometimes when the skin is white and thin, the external jugular, or median vein, may be faintly seen as a thin blue streak, which is unaltered either by inspiration or expiration, or by the action of the heart; the least pressure, however, by the finger instantly produces distension of its upper portion. Occasionally the pulsation of the cervical arteries causes displacement of the surrounding soft parts, and then a vein may become visible, and at each arterial pulse, it will be seen to vibrate, or curve, or slightly swell, or even exhibit a gentle motion of its blood columns. When the exit of the air from the lungs is impeded, swelling of the cervical veins takes place, and should the skin be thin, the positions of the venous valves may be recognised by the knotty kind of swelling produced by them. Veins thus swollen do not alter their diameter during the systole and diastole of the heart. The beat of the cervical arteries may be visible, moreover, through the distended veins. In the horizontal position of the body, the cervical veins become slightly swollen, and the two above mentioned rendered visible, if the skin is not very thick; but no alteration is produced in them either by the respiratory movements, or by the actions of the heart. Sometimes, when the head lies low an undulation in them is caused by strong pulsation of the cervical arteries. It is evident that impeded respiration, and an horizontal position must cause increased distension of the cervical veins.

Under certain *abnormal* conditions of the circulating organs, the cervical veins are constantly swollen, whether the posture be erect or horizontal; or they alternately swell and sink, and manifestly in unison with the movements of the heart. Constant distension of the veins occurs when the blood accumulates *before* the right ventricle, so long as the tricuspid valves close; it may also be caused by continued pressure on the veins, as by tumours in the neck, or by pressure on the descending vena cava.

The rise and fall of the veins depending upon the movements of the heart takes place in different ways. 1. A sudden distension of the veins may attend each ventricular systole, disappearing slowly or suddenly at each diastole. 2. Sudden distension of the veins may accompany the diastole of the ventricle and their contraction also take place during the diastole, or be prolonged into the systole. 3. Sudden distension and contraction may take place during the ventricular systole, as well as during its diastole; they may indeed be three times repeated during the period of one systole and diastole. 4. Swelling of the veins consequent upon a ventricular systole may be gradual; whilst their contraction takes place suddenly, and at the beginning of the diastole.

The abnormal conditions of the respiratory apparatus which impede the entrance and exit of the air into and out of the lungs during inspiration and expiration, cause, amongst other phenomena, swelling of the cervical veins during expiration and their contraction during inspiration. It is manifest, that in cases, where certain abnormal conditions of the circulating organs exist coincidently with these abnormal states of the respiration, the swelling and contraction of the veins will accompany the movements both of the heart and of the respiratory organs, and that the distension caused by the movements of the heart will be at one time coincident with the contraction produced by inspiration, at another with the swelling produced by the expiration. These abnormal phenomena are for the most part more strongly marked in the veins on the right, than on the left side of the neck; so that some of them may be present, and to a striking degree, on the right, and at the same time entirely absent on the left side.

Now it follows, from the facts above related, that the views of Baumgarten, Hamernjk, and Nega, concerning the function of the auricles, are untenable.

Moreover, that in normal circumstances, the contraction of the right auricle does not impede the flow of blood from the vena cava, for if it did, such contraction would be rendered manifest by distension of the cervical veins. And, that during the systole of the ventricles the blood flows as rapidly in the vena cava, as during their diastole, for the level of the blood in the cervical veins—when it is visible—or their distended condition remains unchanged both during the systole and the diastole of the ventricles.

This unchanged condition of the blood's level in the veins is not comprehensible, except upon the supposition, that the blood flowing from the veins during the ventricular systole enters freely into the expanded auricle, and that during the ventricular diastole the contraction of the auricle is of such a nature as not to impede the flow of blood from the vena cava into the auricle. Consequently the auricular contraction is never complete, the auricle is merely narrowed into the form of a canal, of about the size of the openings of the vena cava; it is most vigorous at the commencement of the ventricular diastole, when the blood flows into the ventricle and meets with the least resistance; as the ventricle fills, the resistance increases and the auricle begins to dilate.

Thus the office of the right auricle is, by its dilatation, to prevent that interruption to the movement of the blood which takes place between the auricle and ventricle at the movement of the ventricular systole from being communicated to the veins; and by its mode of contraction as above related, to permit the rapid filling of the ventricle, independently of increased flow of blood through the veins.

If we now proceed, taking these views concerning the action of the auricle as our basis, to an explanation of the phenomena observable in the cervical veins, and which are not dependant upon the respiratory movements, it will appear, that when the tricuspid valves are healthy, the backward movement of the blood from the ventricle towards the auricle which occurs at each ventricular systole will be prevented by the dilatation of the auricle from reaching as far as the veins; but that this movement will be propagated into the cervical veins, and will even produce in them a rapid dilatation synchronous with the ventricular systole—the venous pulse—whenever the right ventricle drives backwards an increased quantity of blood, through defect of the tricuspid valves; or whenever the dilatation of the right auricle is prevented by extensive pericardial effusions. Moreover, an abnormally strong contraction of the right auricle will cause pulsation in the cervical veins—a pulsation generally synchronous with the ventricular diastole, the normal contraction of the auricle taking place during the ventricular diastole.

A vigorous contraction of the right auricle may, *a priori*, be anticipated, whenever it is much over-distended by blood; and we may also observe, that the venous pulse corresponding with the ventricular diastole occurs only in connection with dilatation of the right auricle produced by impediment to the circulation of the blood, and are forced to attribute it to the abnormally strong contraction of the auricle, as there is no other imaginable cause present. The double and triple venous pulsations, which occur during one ventricular systole and diastole, are caused either by two or more abnormal contractions of the right auricle, or there exists, in addition to abnormal strength of its contractions, defect of the tricuspid valves; or the backward impulse of the blood produced by such abnormal contractions of the right auricle, or by defect of the tricuspid valves is repeated once or several times merely as a consequence of disturbance of its equilibrium, or through a double contraction of the auricle. In the last case, there may be observed during the period of a ventricular systole and diastole, one strong, and one or two feeble venous pulsations.

If we suppose, lastly, that the right auricle has lost its contractile power, then will it not be either distended during the ventricular systole, or contracted during its diastole; the blood flowing on in the veins of the neck during the systole of the ventricle will not be received into the right auricle, and will therefore cause distension of those veins; on the other hand, the filling of the

right ventricle during its diastole will not be brought about, as under normal circumstances, in part at the cost of the blood in the auricle, but altogether at the expense of the blood in the veins; so that the blood columns which had gradually accumulated in the veins of the neck during the ventricular systole, will suddenly sink away at the diastole. In this manner the different phenomena mentioned above as observable in the cervical veins become explicable.

II.—THE INFLUENCE OF THE CONTRACTILITY OF THE LUNGS, AND OF THE MOVEMENTS OF RESPIRATION UPON THE CIRCULATION OF THE BLOOD.

It has been shown above, that the columns of blood in the veins of the neck remain unchanged during ordinary inspiration and expiration; and hence it might be judged, that the natural respiratory movements exercise no influence upon the blood in the veins. It is impossible, however, to deny, that unless, by some especial contrivance preventing, blood as well as air must be drawn into the thorax during inspiration, and expelled during expiration, so that the question here presents itself, what is that contrivance by which the influence of the respiratory movements on the circulation of the blood in the veins is prevented.

The lungs normally possess a contractile power, which causes the arching upwards of the diaphragm, and the depression of the intercostal spaces, and which persists, even during expiration. Experiments on animals show us, that when the thorax is opened the lung frequently contracts into less than half the space it occupied at the end of expiration, when the thorax was uninjured.

The persistence of the contractility of the human lungs, even up to the completion of the expiratory act, is proved by the fact, that in healthy persons the percussion sound is not more tympanitic during expiration than during inspiration; whilst, on the other hand, the entire loss of contractility of the lungs in disease is shown by the presence of the tympanitic percussion sound during both inspiration and expiration, and the diminution merely of their contractility by the presence of the tympanitic sound only during expiration.¹ The healthy lung must, consequently, exercise, both during inspiration and expiration, an influence upon the walls of the thorax, and upon all its contents. This influence must certainly be greater during inspiration than during expiration, but the difference can hardly be thought sufficient to produce a distinct rising and falling of the cervical veins during the respiratory movements; for, even in the intercostal spaces no marked interruption to their distended condition is observable during quiet expiration. The slight expansion of the vena cava, which must take place within the thorax during inspiration, will be compensated by the simultaneous contraction of the abdominal cavity, and the increased pressure thereby caused on the inferior vena cava; and the contraction of the vena cava within the thorax, accompanying expiration, by the enlargement of the abdominal cavity, and the consequent diminution of pressure upon the inferior vena cava.

But there is no such thing as pressure on the vena cava during tranquil expiration, so that the blood cannot thereby be driven backwards in the vena cava, nor the valves of the cervical veins be called into action. Consequently the contractility of the lungs, which is of about equal force during inspiration as during expiration, occasions a continual hastening of the current of blood in the veins, when the respiration is normal, and in conjunction with the slight widening and narrowing which the upper and lower venæ cavæ undergo, prevents the appearance of any undulatory movements of the blood in the cervical veins.

As regards the influence of the contraction of the lungs on the other parts of the circulating organs, it must act both upon the auricles and ventricles, and favour their dilatation. It is overcome by the contraction of the auricles, and

¹ The important fact here referred to will, I fear, be quite incomprehensible to those who are unacquainted with Skoda's labours.—W. O. M.

still more easily by that of the ventricles, but aids the dilatation of the auricles and of the ventricles.

The contractility of the lungs, indeed, opposes the contraction of the pulmonary artery, but its force is so slight in comparison with that which produces the systole and diastole of the pulmonary artery, that its absence can cause immediately no appreciable change in the condition of that artery. Not so, however, is it with the capillaries of the lungs, for these vessels are placed in the most favourable condition for the passage of the blood through them, in consequence of the tension exercised upon them by the contraction of the lungs.

The expansion of the pulmonary veins, caused by the contractility of the lungs, exercises no influence, either in impeding or in hastening the circulation of the blood. The contractility of the lungs is very insignificant when compared to that of the aorta; and even though it did produce an expansion of the thoracic aorta, such expansion would neither assist nor impede the circulation. Hence it appears, that the contractility of the lungs aids the circulation of blood in the systemic venous system, assists the filling of the right auricle and ventricle, and the passage of the blood through the capillaries of the lungs; that it consumes a portion of the contractile power of the auricles and ventricles, and renders almost null the influence of the normal respiratory movements upon the circulation.

The actions which have been here ascribed to the contractility of the lungs are most manifestly shown during the act of impeded inspiration. If the cervical veins previous to such forced inspiration are filled with blood, they will be emptied during its act, and even compressed by atmospheric pressure. Such an inspiration, moreover, consumes a great part of the contractile power of the auricles and ventricles, and it is possible, that the diminution of the pulse, so often observed when the inspiration is impeded, is occasionally caused by such consumption of the contractile power of the left ventricle. Thus we see, that an impeded inspiratory movement hinders the circulation.

The increased velocity of the current of blood flowing from the cervical veins and inferior vena cava into the superior vena cava and right auricle does not increase the rapidity of the current in the other veins; for these, on account of the yielding nature of their walls, whenever suddenly emptied of their blood, are compressed by the weight of the atmosphere, and the propagation of the increased velocity to the other portions of the blood thereby prevented. On the other hand, the loss of a part of the contractile force of the left ventricle necessarily causes a diminution of velocity in the systemic circulation. In like manner will the pulmonary circulation be retarded. The before mentioned inspiratory movements cause widening of the arteries, veins, and capillaries of the lungs, whereby the current is retarded. In addition to which there is also a corresponding diminution of the contractile power of the right ventricle.

An impeded expiration causes a like pressure upon the tensely rendered contents of the thorax and abdomen. The effect of this pressure must be different upon different parts of the circulating organs. The blood of the right auricle and the vena cava will be forced back into the venous trunks which open into these veins, until its reflux motion is arrested by their valves.

If the expiratory movement be of no great duration, its influence does not reach much beyond these valves; but when it is prolonged, the blood accumulates in all the veins which enter into the thorax and the abdomen, and this accumulation at length extends to the whole of the veins and the capillaries of the systemic circulation; increased pressure in the arteries is thereby required for its removal, and this takes place when, through the heart's movements a portion of the contents of the vena cava are removed, and through their narrowing the influence of expiration upon them, lessened.

Expiration opposes the expansion of the auricles and ventricles, which therefore is effected solely by the *vis a tergo*: but on the other hand it assists their contractions, and is thus a frequent cause of the increase of the pulse which is felt at the moment of expiration, when the breathing is impeded; a

sudden expiratory movement, indeed, under such circumstances, as, for instance, coughing, occasionally creates a strong pulsation in the arteries during the heart's diastole, which pulsation is evidently caused by the sudden compression of the distended left ventricle and aorta.

Expiration tends to produce a narrowing of the arteries, veins and capillaries of the lungs, and in this way increases the friction between the blood and the walls of the vessels. It impedes the pulmonary circulation, for the increase of the systolic power of the right ventricle produced by expiration is far from compensating for the increased friction between the blood and the vessels, which is also produced by it.

In like manner expiration impedes the systemic circulation. Thus it appears, that during a forcible inspiratory effort, the circulation of the blood is interfered with by the consumption of a portion of the heart's force, and that during a forcible expiration it is likewise impeded by increase of the friction between the blood and its vessels in one great division of the circulating system. The alternation between inspiration and expiration under such circumstances eases the circulation, inasmuch as at the conversion of one act into the other the impeding force of the one ceases before that of the other begins.

There is no impediment to the entrance of air into lungs which are highly emphysematous, but whose bronchial tubes are neither narrowed nor occluded; the expiration is performed by the muscles of expiration on account of the lung having lost its contractile power, and even here, when there is no febrile action and the patient in repose, the expiratory effort is not great. But on account of the want of contractile force, the air is not driven equally out of all parts of the lungs, but more particularly from its superficial portions and its borders, and so conversely is the air drawn chiefly into those parts during inspiration. In the central divisions of the lungs there is but little renewal of the air. Respiratory movements of this kind cannot facilitate the passage of the blood through the lungs; but on the contrary, the loss of contractility of the lungs causes the filling of the heart's cavities to be performed entirely by the *vis a tergo*. In such cases the circulation is not facilitated by the alternation of the inspiratory and expiratory movements; the cervical veins, particularly when the patient lies down, remain distended with blood, not only during expiration, but also during inspiration, and there is also a high degree of cyanosis present, even when the breathing does not appear much affected.

SURGERY.

DISCUSSIONS ON SYPHILIS IN THE IMPERIAL ACADEMY OF MEDICINE, PARIS.

Within the last 18 months the subject of syphilis has been frequently brought under the notice of the Academy of Medicine in Paris, and the opinions of some of its most distinguished members elicited in the discussions which have taken place. It may be interesting to lay before our readers a short abstract of the views entertained abroad on some of the controverted points of this intricate subject. Before doing so, it is necessary to premise a few observations regarding the doctrine of *syphilisation* recently brought forward.

About the year 1844, M. Auzias, Turenne, endeavoured by inoculation to produce syphilis in the lower animals, and particularly in monkeys. In this attempt, he was, he says, successful, and he states that he observed that fresh chancres produced in the same animal became milder, until at length the subject of experiment became incapable of contracting chancres, having become, to use his own expression, *syphilised*. It then occurred to him that the same thing might happen in the human subject; and after making numerous researches, he found a certain number of prostitutes, who, having suffered frequently from syphilis,

had become refractory to it, and were at the same time in sound health. On these observations, he founded the theory and practice of syphilisation; the theory being that when a sufficient quantity of syphilitic virus has been introduced by successive inoculation, the system becomes incapable of being farther affected by it; while the treatment consists in practising numerous inoculations with pus obtained from chancres, with the view either of curing existing syphilis or of rendering a healthy individual safe from an attack of the disease.

Another advocate of the same doctrine appeared in the person of Dr Sperino of Turin. This gentleman had thought he observed among his patients in the syphilitic hospital, that the greater the number, extent, and duration of the primary sores, the less likelihood was there of the supervention of secondary affections. Hearing of the results obtained by M. Auzias, he saw an analogy between them and his own observations, and, accordingly, he performed a number of experiments on his female syphilitic patients. He states that he found, when inoculations were repeated a sufficient number of times they came to produce no effect, although on persons not syphilised inoculation with the same purulent matter produced primary ulcers. Accordingly Dr Sperino agrees with M. Auzias, Turenne, in looking upon syphilisation both as a cure and a preventative of syphilis.

This question was brought before the Academy of Medicine in Paris, on the 18th November 1851, when M. Ricord exhibited to that learned body a young German doctor who had submitted to numerous experiments in order to determine the truth of the doctrine of syphilisation—a number of the inoculations having been practised by M. Auzias himself. At this time he presented all the symptoms of constitutional syphilis. A question having arisen as to whether the pus used in one inoculation had been derived from a primary or a secondary sore, a committee was appointed to inquire minutely into the circumstances of the case, and report to the Academy on the subject. The committee consisted of M. M. Velpeau, Ricord, Lagneau, Roux, and Bégin. Their report was not submitted to the Academy till the 20th July 1852. It described the state of the experimenter at the time he appeared before them, in general terms decided against the doctrine of syphilisation, either as a preventative or curative agent, and left unsettled the inoculability of secondary affections, stating that the observation in question was of little importance either way. Ricord at the same time mentioned that the German who had only appeared once before the committee, had been seen frequently by him, and by his pupils in the hospital, that he had submitted to upwards of two hundred inoculations, but that in spite of this he was suffering both from secondary and tertiary syphilis. The discussion was not entered into till the next meeting, and it was decided to divide it into two parts, the first embracing the question of syphilisation, the second the inoculability of secondary affections.

The discussion was opened by *M. Malgaigne*, who expressed himself undecided as to the truth of the facts on which syphilisation was founded; but the conclusion he drew was that some of the results obtained were too important to be lightly rejected, and that they imperiously require to be examined. He had been himself a partisan of the views of Ricord, but he considered that two points of his doctrine had been overthrown. In the first place Ricord maintained that syphilis belonged to man alone, whereas it was now proved, beyond the possibility of doubt, that it could be communicated to animals: and in the second place, as an acquired immunity in certain individuals was, in his (M. M.'s) opinion, incontestible, it followed that chancres were not necessarily inoculable, and hence the value of inoculation for purposes of diagnosis, and in questions of medical jurisprudence fell to the ground. If it were proved that the doctrine of syphilisation were true, *M. Malgaigne* should consider it wrong to employ the practice as a preventive in the healthy (except in the case of prostitutes), while in the case of these already diseased, he should look upon it as a benefit.

M. Velpeau stated that the only point in the case brought before the Academy which had interested him, was as to the inoculability of secondary syphilis. The

committee, he thought, should not have entered into the subject of syphilisation, as he considered the doctrine unworthy of being examined, and because the more such questions were discussed, the more chance they had of making proselytes. He should accordingly reserve himself for the second point of the discussion.

M. Depaul agreed with the opinions expressed by *M. Malgaigne*; he thought the committee should have trusted more to facts than to reasoning. Why, for instance, had they not inquired into the case of *M. Laval*, who having been syphilised by *M. Auzias*, had been inoculated by *M. Ricord*, but without the production of chancres. *M. Dupaul* considered that numerous experiments had satisfactorily proved that syphilis was communicable to animals, and that not only in its primary form, but as a constitutional disease. With regard to the practical application of the doctrine, while he should be willing to inquire into the merits of syphilisation as a curative agent, he agreed with *M. Malgaigne* in absolutely rejecting all attempts to employ it as a prophylactic.

M. Ricord considered that the absurd theory of syphilisation could not be maintained,—there were absolutely no facts in its favour. It had been stated that he had failed to inoculate *M. Laval*, but this was incorrect, for one of his inoculations had succeeded, and *M. Laval* had himself stated that if any of the experiments had failed, it was owing to the weakness of the pus, not to any immunity he possessed. *M. Malgaigne* considered that two points of his (*M. R.*'s) doctrines had been overthrown. But *M. Ricord* still denied that syphilis could be communicated to animals. It may sometimes be with difficulty *transplanted* upon them, but is then limited to its most local manifestation; there is never a series of constitutional accidents as in man, and a chancre alone does not constitute syphilis. The second point was that some individuals could not be inoculated with syphilitic virus; this, however, had not been established, and, besides, he (*M. R.*) only drew conclusions where the inoculation was positive; on a future occasion he would state what he concluded from *negative* results. The supporters of syphilisation maintained that the longer the primary affection lasted, and the more extensive it was, the more readily was the individual syphilised. But *M. Ricord* had exhibited at his clinique a chancre which remained inoculable after seven years, and *M. Laval* had been inoculated with the pus of a chancre of eighteen months' standing. If a large chancre syphilised more than a small one, how was it that the latter heals in general so readily, while the former may continue for months or years; this would show that what syphilises the most has the least tendency to get well. Syphilisation was never applied to phagædenic chancres, and it was well, yet these were the very cases where its application was the most called for. It was stated that syphilisation was not painful, but the only proof brought forward, was the saying of a philosopher, that pain is but a word. It was maintained that syphilisation was not dangerous, but *M. Ricord* mentioned some cases where it had been so, and one where death ensued from induced erysipelas. It was also asserted that the treatment was more rapid and more commodious than the ordinary method; and he could not understand this, when during six weeks they continued to produce fresh chancres till you had the arms, legs, and body covered with them. *M. Ricord*'s conclusion was, that syphilisation, as a prophylactic, should be prohibited by law, and as a curative, it should be rejected by all judicious practitioners.

M.M. Larrey, Lagneau, and Begin expressed their opinion as to the absurdity of the doctrine of syphilisation, and as to the dangers and inconveniences of the practice.

The following resolution, proposed by the committee, was then put to the vote:—"The Academy, by its vote, declares that it approves of the principles expressed in the report of its committee, in so far as regards the practice of syphilisation, both as a prophylactic and as a curative agent."

This resolution was carried all but unanimously, only two votes being given against it.

In our next number we shall give an abstract of the discussions on the inoculability of secondary syphilis.

PROPOSAL FOR THE EFFECTUAL CURE OF PROLAPSUS OF THE PELVIC VISCERA
AND LACERATED PERINEUM. BY MR HILTON.

[Among the diseases of females, none is the cause of more permanent suffering and wretchedness than prolapsus of the pelvic viscera,—either the uterus, vagina, rectum, or bladder. And unfortunately medicine has hitherto had very little success in its efforts and plans for the relief of these affections. Trusses, pessaries, operations, medicinal substances, alike fail in many cases, in others they are inapplicable; in all they are only half cures when available. We hail, therefore, with much hope the ingenious surgical treatment, applicable, as it is, to one and all of these cases, and which Mr Hilton of Guy's Hospital, has lately proposed. Already the plan has been tried, and with encouraging results, in some severe cases under the charge of Dr Lever. These cases it is not necessary to quote here. Mr Hilton operated in them, and in a letter to Dr Lever, he explains the theory of the operation, and the mode of performing it, as follows:—]

“I certainly think the cases of lacerated perineum are worth publishing, and I have great pleasure in sending to you a statement of the reasons which induced me to adopt the operation performed in each of the cases. As far as I know, such an operation had not been done before that period, 1848, with the purpose of relieving the distress and annoyance to which these patients were exposed; but in this opinion regarding the originality of the operation I may be wrong, if so, your better information will set me right.

“When you requested my assistance to determine what had best be done in a surgical direction, remembering that the levatores ani have one firm and fixed attachment to bone near the arch of the pubes, and another at the coccyx, and that the external sphincter ani might be regarded anatomically nearly in the same light in relation to its effects upon the injury to the perineum, and bearing in mind that all muscles contract towards their more fixed point, no matter how that fixity of position may have been acquired, it occurred to me, if I could by a simple and uncomplicated operation, disengage the coccygeal attachments of the levatores ani, I might allow them to retract the anal aperture and adjacent structures in a direction towards the pubes, as it were, to bury the perineal injury deeply in the pelvis, thus enabling the lower fibres of those muscles (which blend with the muscular parietes of the vagina, rectum, and perineum) to assume the office of a sphincter to the lacerated opening, by approximating the edges of it, and drawing it upwards towards the pubic arch. In reference to the external sphincter ani, I concluded that, by taking away or separating the coccygeal fixed point of that muscle, I should necessarily change the direction of its contractile power from the coccyx towards the vagina, and thence to the pubes; this I hoped would help to occlude the lacerated opening between the vagina and rectum. Whether I had reasoned rightly or not, the results were as satisfactory, and indeed more so, than I had anticipated. It seemed to myself, that two ulterior purposes might be held in view by such an operation; the first was to ascertain how much of complete relief could be afforded by an operation which promised to be altogether free from both the danger and the severity of the ordinary operation for such cases, and secondly, should no important immediate benefit be derived, it would certainly tend to the advantage of the patient, by putting the parts into a better state (by relaxing them, and so taking off tension) for the easy and perfect accomplishment of the usual but more formidable operation of paring the edges of the lacerated wound, and maintaining them in contact for a time by sutures.

“The method of proceeding was as follows:—A narrow sharp-pointed knife was introduced through the skin on one side of the point or free extremity of the coccyx, about half or three-quarters of an inch from its end; it was then passed into the pelvis, between the concave surface of the coccyx and the rectum, special care being taken not to puncture the intestine; the cutting edge of the knife was now made to sweep over the sides and ends of the coccyx, so as to separate from it the coccygeal attachments of both the sphincter and levatores ani; the

knife was then withdrawn through the same small opening by which it had been introduced; scarcely any blood escaped at the wound, but a compress of lint supported by adhesive plaster was applied over it to keep the parts quiet, and to intercept the flow of blood.

"That the operation had accomplished its intention of detaching the muscles from the coccyx, was obvious enough, by examining with the finger upon the skin, the median line between the end of the coccyx and the posterior margin of the anus, the resistance which the muscles naturally give to pressure in that position had disappeared, and the anal aperture became retracted or drawn up into the pelvis.

"During the time I had the opportunity of seeing the patients after the operations, I have no hesitation in saying they were much benefited by what had been done for them, so much so, that no further treatment was deemed necessary. How far the operation may have succeeded ultimately and persistently, I do not know.—*Guy's Hospital Reports*.

MIDWIFERY.

NATALIS GUILLOT. SECRETION OF MILK BY THE MAMMÆ OF NEW-BORN CHILDREN.

The examination of a large number of newly-born children has led the author to determine :—1st. That the mammæ of children of both sexes secrete milk a short time after birth. 2d. This function ordinarily succeeds the separation of the umbilical cord, beginning from the seventh to the twelfth day, and lasting for several days. It is quite normal, and takes place only in healthy children. 3d. During the secretion the mammary glands are sensibly swollen. 4th. The fluid may be squeezed out from the mammæ by pressure, either in a jet or in drops. It is white, neutral, or alcalin; it acidifies by contact with the air; it separates into two portions, one serous, the other creamy, and possesses the same chemical composition, and the same appearance under the microscope, as woman's milk. The author adds a table of the exact day of age at which he observed the lacteal secretion in thirty-nine male and thirty-four female children.—*Acad. des Sciences*, Oct. 17.

BOULARD. ON ANTEFLEXION AS A NORMAL CONDITION OF THE UTERUS BEFORE PREGNANCY.

From observations, embracing 107 dissections, the author concludes that anteflexion is, with rare exceptions, the normal condition of the uterus in the foetus, the young girl, and woman previous to pregnancy. The body of the uterus is bent forwards on the neck, so as to give the organ the shape of a retort, with the broad end directed upwards and forwards. The axis of the body of the uterus is nearly horizontal, while that of the neck corresponds with the axis of the superior aperture of the pelvis; the fundus is in relation with the posterior wall of the bladder, the anterior surface is directed downwards and the posterior surface looks upwards: the anterior surface is bent forwards upon itself, the angle of flexion being at the junction of the body and the neck. This flexion cannot be considered a cadaveric change, because, if artificially straitened, the uterus returns to the bent position. In ten examinations in the living subject, the author has found the same condition. By the occurrence of pregnancy the anteflexion is removed, and if observed after this period, it depends on some pathological condition, as abnormal adhesions, etc.—*Gaz. Med.*, Sept. 29, 1853.

MAZIER. RUPTURE OF THE UTERUS: GASTROTOMY PERFORMED FORTY-TWO HOURS AFTER: RECOVERY.

This case of Cæsarian section, which occurred at Laigle (Orne), is a wonderful instance of recovery under the most adverse circumstances, and fully exem-

plifies the greater success of dangerous operations in the country than in towns. Madame Soret, æt. 32, of robust constitution, the mother of two children, after a normal pregnancy fell in labour on the 30th December, about 3 p.m. The child presented by the feet. The pains, at first slow, became strong and frequent towards nine o'clock. About eleven, a pain of great violence came on, forcing the patient to scream aloud with suffering, and blood was at the same time discharged in a copious stream. The labour ceased immediately, and on examination the feet of the child could no longer be felt. The blood continued to flow in abundance for an hour, but without any return of the pains. The medical men in attendance declined to act till the pains should return, and the patient remained that whole night, and the following day and night, in a very precarious condition, suffering severely from acute pain in the whole abdomen, but especially in the epigastrium. On the 1st January M. Mazier was called in, and, arriving in the afternoon, was not joined by the other medical men till late in the evening. He found the patient pale and emaciated, a marked coldness over the body, the belly tense and unable to support the least pressure. The child could be felt through the abdominal parietes, apparently removed from the pelvic region, and occupying the superior part of the abdomen. On examination, per vaginam, an extensive rupture of the uterus was detected on a level with the superior aperture of the pelvis, the tear remaining open for a quarter of its length on the left side, and elsewhere obstructed by clots of blood, etc. Gastrotomy being decided on, the operation was performed by an incision of above sixteen centimetres in length, dividing nearly the whole extent of the linea alba from the umbilicus to the symphysis pubis; and the child and placenta were extracted from the peritoneal cavity. The child was dead, and had evacuated, for the intestines of the mother were stained by the meconium; the dirty serum and small blood clots were removed, and the incision united by the twisted suture. The operation was succeeded by very little fever; the pain and tension of the abdomen soon gave way; there was some suppuration by the vagina; the wound was cicatrised on the 8th. With the exception of an eruption on the skin, and pain in the rectum, attended by constipation, and removed by appropriate evacuations, no remarkable symptoms occurred, and the patient steadily improved. At length the neck of the uterus could be felt pushed back and adherent to the rectal region of the true pelvis. The cicatrix of the rupture and of the incision was complete, and the patient had resumed her ordinary occupations forty days after the operation. A hernia of the linea alba could be observed under the skin for one half the extent of the incision, but this was the only trace left of this perilous operation.—*Jour. de Med. et Chir-pratiques*, quoted in *Revue Med. Chirurgicale de Paris*.

DUBOIS' EPIDEMIC OF PUERPERAL FEVER, AND GANGRENE OF THE EXTERNAL GENITALS ORGANS IN THE HÔPITAL DES CLINIQUES OF PARIS.

During October and November, puerperal fevers and cases of gangrene of the external genital organs became simultaneously epidemic in the wards of the Clinique des Accouchements. The two affections, however different in their nature and symptoms, seemed to have some connection in their cause, and were probably only separate manifestations of epidemic influence. Latterly, almost all the patients were seized; those who did not take puerperal fever were attacked by the local gangrene. The particulars of the cases are thus described in a brief sketch. Within two or three days after delivery, most of the women are seized with some of the following symptoms:—First, a rigor, more or less violent, more intense, and of longer duration, than the rigor of the milk fever, and occurring either in the first twenty-four hours or on the third or fourth day. The rigor is succeeded by intense fever and acute pain in the hypogastric region, or on one side of the abdomen; the lochial discharge is diminished and becomes sanio-purulent in character. These symptoms are accompanied by bilious vomiting, headache, etc.; in short all the characteristic signs of metro-peritonitis are speedily de-

veloped. This fever, however, although it has attacked a very large proportion of the patients, has not presented the very fatal character usual in such epidemics. Only a few deaths have occurred; and in the great majority of cases the disease terminated favourably after a longer or shorter course. In the cases of gangrene, generally after the day succeeding the delivery, sometimes only a few hours after it, an ecchymosis is usually observed on the inner surface of one of the labia majora, or one side of the fourchette. Soon afterwards one or more small fissures make their appearance, and in some hours the epithelium is destroyed, and replaced by an ulcerated surface of greater or less extent, involving one or both labia, and disposed most generally in the form of a horse-shoe, with the convexity directed downwards. This ulceration becomes covered with a diphtheritic exudation, and in some cases spreads rapidly, destroying in a few days a great extent of the mucous membrane, of the vulva, and the surrounding integuments. Of two examples which are given, in the one the whole perineum was destroyed from the fourchette to about two inches behind the anus, and presented on the separation of the eschar a large granulating wound, in the centre of which the circular fibres of the anus could be distinguished; in the other case the whole mucous membrane of the vagina was exfoliated. In some instances the gangrene was accompanied by fever, but in the great majority of cases there was little or no general reaction.

The general preventive measures employed consisted in prohibiting any of the articles or dressings which had served in the affected cases, from being used for the new patients. Moreover, following out the recommendations of the physicians of Vienna, M. Dubois has forbid the students engaged in making the autopsy from taking part in the deliveries. To arrest the local progress of the gangrene, cauterisation, with the acid nitrate of mercury, and subsequently the application of the actual cautery, at a white heat, were freely used."—*Gaz. des Hôp.*, Nov. 17th and 19th.

[That such epidemics should occur at the Hôpital des Cliniques, is perhaps hardly to be wondered at, when its unhealthy situation is considered, being placed in the vicinity of the dissecting-rooms of the Ecole Pratique, and built, it is said, upon or close to foul drains.

PROFESSOR FAYE'S CASE OF CÆSARIAN SECTION, NECESSITATED BY UTERINE TUMOURS.

The patient was a primiparous woman, aged forty, whose labour commenced on the 3d June 1850. After continuing some time the pains altogether ceased; when venesection and enemata were had recourse to, but without benefit. On a vaginal examination the breech of the child was felt immediately above the symphysis pubis, and to the right of it was distinguished a large rounded body. A large immobile tumour, of cartilaginous consistence, was felt occupying the whole hollow of the sacrum, and the left iliac fossa. A similar tumour, pretty high in the pelvis, could also be diagnosed to the right of the sacrum. The uterus was so high that the cervix could not be reached by the finger. Thus matters continued for two days; then weak pains recommenced, but with no effect, as it was found that the tumours had somewhat descended, diminishing the outlet of the pelvis to one inch in diameter. On an exploring trocar, with hooked extremity (*wiederhaken*), being thrust into them from the rectum, no fluid was found to exude through the canula, nor was any of their substance brought away. Cæsarian section was resolved on, and performed in the usual manner under the influence of chloroform.

The child was found in the second breech-presentation (sacro-posterior), and was easily extracted alive. Some hemorrhage occurred after the removal of the placenta, but this was effectually arrested by compression of the abdominal aorta. Vomiting occurred after the operation; followed next day by abdominal pain, return of hemorrhage, collapse, and death. On examination, *post mortem*, two large fibrous tumours were found attached by pedicles to the posterior wall of the uterus.

This is the fourth case of Cæsarian section which has been performed in Norway within the last ten years. Two of these occurred under very disadvantageous circumstances, viz., after forceps and craniotomy had been tried. In three cases the children were extracted alive, and in two they ultimately survived. In every case the mothers died—at periods varying from a few hours to five days after the operation.—*Norsk Magazin Bd. 6.*

HYSTEROTOMY AFTER RUPTURE OF THE UTERUS, BY J. F. HALDER. RECOVERY.

The patient was a woman, with an oblique and contracted pelvis, who had twice borne dead children. In her third confinement the head of the child became impacted at the brim. The pains, which were very strong, suddenly ceased; the patient became pale and uneasy, and the pulse sunk very low. On examination a rupture was found to have taken place at the posterior part of the uterus. Dr Halder thereupon performed the Cæsarian section, and extracted, with ease, a dead child, having a greatly enlarged head. In five weeks after the operation the patient had completely recovered.—*Nederland Weekbl. August 1853.*

DR SEYFERT ON ALBUMINURIA IN THE PREGNANT AND PARTURIENT FEMALE.

This author, after a lengthened series of investigations, has arrived at the following results:—

1st. Albuminuria is not an essential accompaniment of normal healthy pregnancy.

2d. The theory, ascribing albuminuria to the pressure of the enlarged uterus on the renal vessels, is inadmissible.

3d. When anasarca, from Bright's disease, occurs during pregnancy, the patients seldom are attacked by eclampsia.

4th. The albuminuria in cases of eclampsia is occasioned by the interruption of the functions of the respiration and circulation by the attack.

5th. In such cases, the albuminuria terminates with the attack.

6th. Albuminuria is not present in *all* cases of eclampsia.

7th. Albumen is found in large quantities in the urine of epileptics, *immediately after an attack*, but not invariably after every seizure, or in every case of the disease.

8th. Provided there be no Bright's disease, this albuminuria among epileptics ceases soon after the convulsions, and only returns after the next attack.

If, therefore, says the author, eclampsia depends on an alteration in the constitution of the blood, are the copious venesections, so universally recommended, judicious? The blood is impoverished during pregnancy; and we know that poisonous matters, commixed with the blood, act much more powerfully when there is an anæmic state of the system.

Dr Seyfert says he has seen the most beneficial effects from the use of chloroform in eclampsia.—*Wien. Medic. Wochenschr. 12. 1853.*

CASE OF FOETUS WITHIN THE PLACENTA, BY S. SARGENT, ESQ.

Mrs M., of Lawrence, U. S., became pregnant with her third child in February, 1853. After the second month she was attacked with frequent floodings, which would last for a day or two, and then subside. During the last two months which she carried the child, the bleeding became more profuse, so as to waste her strength, and at times became alarming, so much so that Mr Sargent was called in several times to arrest the hemorrhage.

On the 20th of August he was called to see her, and found her with labour pains. On examination, per vaginam, he found the os tincæ somewhat dilated, and the placenta attached over the mouth of the womb, and presenting. As the dilatation was not great nor the flooding profuse, nothing was done for three or four hours. The labour slowly progressed, and upon making a second examination, four hours after the first, he found the same presentation, with the parts a little more dilated. No part of the child could be felt, but only the placenta protruding through the os tincæ.

Mr S. now attempted for some time to remove the placenta or some portion of it to one side, in order to give the foetus room to pass down. After nearly exhausting his patience without gaining ground, during a strong pain he made a thrust, and his finger went through the placenta. Water flowed out in large quantity. The head of the foetus could now be felt. The pains continued strong, the parts were *well* dilated, yet the descent of the child was slow, and only with the placenta. At length this was expelled. Neither cord nor membranes could be felt. Upon opening the placenta, it was found to *contain the foetus*, weighing two pounds or more; the cord, fourteen inches in length, of the medium size, and a portion of the liquor amnii which had not escaped through the opening previously made with the finger. The placenta was a complete sac, the cord starting off from its smooth inner surface, like the trunk of a tree from its roots. The child was a male, and breathed a few times.

It seems that the placenta had entirely surrounded the membranes, attaching its inner surface to them, while the exterior was attached to the whole surface of the womb. It looks as though the foetus had been formed, together with the funis and membranes, without a placenta, and that was altogether an after consideration, and supplied by the whole inner surface of the womb, and surrounding the membranes and foetus.

The placenta and cord, with its attachment, are preserved.

[This case is very imperfectly recorded and the explanation of it is absurd.]

CASE OF PLACENTA PRÆVIA. BY M. BONNASSIS.

On the 6th of November 1847, at ten o'clock in the morning, I was sent for to the Faubourg St Antoine to see Madam Es, who had been suffering from uterine hemorrhage since the previous evening. A midwife was in attendance. The patient had had five children, all born at the full time and healthy, and in addition, an abortion at the fourth month of pregnancy. She was about 35 years of age, and had always been healthy. She believed herself to be more than eight months gone. During this pregnancy she had had slight hemorrhage at the fourth month, which returned during the sixth, but on both occasions it ceased spontaneously. On examination, I found that the neck of the uterus was slightly dilated, and I felt a soft body, which I recognised as the placenta. The blood flowed without intermission, and there was not the slightest expulsive pain. I ordered my patient to keep her bed, to apply cold fomentations, and to take cold acid drinks. The hemorrhage continued during the day, becoming gradually more considerable. At ten o'clock in the evening the patient was extremely feeble, the dilatation of the neck had made no progress, and there was no appearance of uterine contraction. On auscultation the beatings of the infant's heart were audible. I called in another practitioner, who arrived immediately, and we decided that there was no time to be lost. Accordingly I introduced the fingers one after the other, and easily obtained complete dilatation of the neck, which offered no resistance. I believe that in similar cases dilatation is easily produced, at least I once noticed the same thing under similar circumstances. The hand having passed the neck, I pressed it through the placenta at its central part; on the rupture of the membranes amniotic fluid mingled with blood escaped. I seized the feet, brought them to the vulva, and finished the labour. The infant, although very feeble, and not appearing to be at the full time, was living; it was a girl, who is now five years of age, and in good health. The placenta, when extracted, presented in the centre an opening by which the child had passed. The cord was in the normal condition. The patient herself made a good and tolerably rapid recovery.—*Communicated to the Société Médico-Chirurgicale, Paris.*

[It will be remarked that, in this case, the practitioner passed his hand through the placenta, a proceeding which is of very questionable propriety. The disruption of the placenta is unfavourable to its easy and complete removal, and the laceration of foetal vessels, which necessarily follows this practice,

must add greatly to the danger of the child. The proper plan is to detach enough of the placenta to let the hand enter the uterus, and then rupture the ovum through the membranes.]

BECQUEREL AND VERNONIS ON THE INFLUENCE OF MENSTRUATION ON THE MILK.

The return of menstruation during lactation is considered by most obstetricians to be very injurious to the health of the child, and an indication that nursing should be discontinued, or a substitute procured. MM. Becquerel and Vernois have examined the milk of nurses during the presence and absence of the catamenia; and the results of their investigations militate against the commonly received opinions, showing that the composition of the milk is very little changed during menstruation, and that, so long as the child continues to thrive, there is no necessity for changing the nurse. The following are their analyses :—

	I. When catamenia absent.	II. During menstruation.
Specific gravity,	1032·24	1031·58
Water,	889·51	881·44
Solids,	110·49	118·56
Sugar,	43·88	40·49
Casein and extractive matters,	38·69	47·49
Butter,	26·54	29·15
Salts in residue,	1·38	1·45

The above changes do not materially affect the nutritive qualities of the milk. The authors recommend that the child be put as seldom as possible to the breast during the catamenial period, and that a little sugar and water be given to compensate for the diminished saccharine matters of the milk.

These remarks apply only to menstruation as it occurs at an early period of gestation, and without causing much pain and constitutional disturbance. The secretion of milk becomes generally arrested when menstruation occurs too profusely and of long duration; and almost with certainty when the catamenia return towards the end of lactation. This latter circumstance indeed is an indication for the weaning of the child. Painful menstruation occurring during lactation, may occasionally cause the milk to disagree with the child, inducing colic, vomiting, flatulence, or diarrhoea; but these effects are very transitory, and the milk soon recovers its normal healthy condition.

[It must be recollected that chemical changes in the milk are not the only possible alterations. Although it is ascertained that the milk is really richer in nutritious material during menstruation occurring in the natural period of lactation, yet it is well known that occasionally, though rarely, the milk at the same time acquires some obnoxious influence which is exerted upon the child. The cause of this we cannot detect by chemical analysis; but the result in the child proves its existence. On the occurrence of menstruation in a nurse, a paroxysm of rage or of grief will often affect the child injuriously (the latter sometimes mortally), without any corresponding discoverable chemical change in the secretion.]

MATERIA MEDICA AND DIETETICS.

DESMARTIS. ON BATHS AND LOTIONS OF GUANO IN SKIN DISEASES.

First proposed by Recamier, this treatment has been found very successful in cutaneous affections. Pemphigus has completely disappeared after two or three baths; tinea capitis has been cured by the lotions in one or two months; cases of psoriasis and eczema, chronic, and considered incurable, have yielded

completely after these applications, prolonged, however, for a considerable length of time. Opacities of the cornea, leucoma, and albugo, have been removed by collyria containing guano. Scrofulous and cancerous ulcerations have been benefited, and the suppurations checked. Constitutional treatment, with iodine, mercury, arsenic, purgatives, etc., was employed at the same time. The baths contain 500 grammes of guano; the lotions, which should be diluted as the inflammatory state of the part may render necessary, may vary from 50 or 60 up to 120 grammes to the litre of water, and they should be filtered after the ebullition. An ointment may also be used in the proportion of 2-10 grammes to 30 grammes of axunge.—*Revue Thérap. de Méd.*

ON THE EFFECTS OF TEA AND COFFEE ON THE ANIMAL ECONOMY.

Dr W. F. BÖCKER, with a plodding perseverance worthy higher ends, has been investigating the effects of this beverage on the animal fluids and solids. For this purpose he experimented on himself; living in his usual way, upon food of a uniform character, and drinking alternately, for periods of seven days, water alone, and tea alone. The total amount of the ingesta and egesta for each seven days was carefully noted, and the general results compared and tabulated thus:—

A. Ingesta.	I.		II.	
	Drinking water only.		Drinking tea only.	
Total weight (in grammes),	.	3610.50	.	3617.00
Water,	.	2938.84	.	2957.87
Solids,	.	671.52	.	659.13
B. Egesta.				
Loss of weight of body,	.	539	.	203
Fæces,	.	178.30	.	96
Perspiration,	.	1349.90	.	1335.7
Urine,	.	2621.143	.	2550.000

From this it will be seen that while drinking tea alone the amount of the excretions was lessened; the daily loss of weight was not so great as when water was drunk, and that the fæces, urine, and perspiration, were all diminished.—*Arch. für Wissensch. Heilk.* 1, 2, 1853.

Dr M. J. LEHMANN relates some experiments on the effects of coffee on the economy, which lead him to the following conclusions:—

1. That coffee exerts upon the economy two actions which it is difficult to reconcile. It augments the activity of the vascular and nervous systems, whilst it relaxes the metamorphosis of tissue.

2. The state of healthy excitation which the use of coffee produces, is due to the simultaneous action of the empyreumatic oil, and of the caffeine.

3. The retardation of the decomposition of the tissues must be ascribed principally to the empyreumatic oil, the caffeine not contributing to produce that effect except when it acts in rather strong doses.—*Journal de Pharmacie*, Dec. 1853.

DR GLOVER ON THE ACTION OF COD-LIVER OIL.

Dr Glover says he still differs from Dr Hughes Bennett with regard to the *modus operandi* of cod-liver oil. That the fact of oil placed in contact with albumen under the microscope forms a globule, with a central nucleus and outward pellicle, affords any sufficient explanation of the value of cod-liver oil in scrofula and phthisis, he objects to, as being too mechanical. He asks, is it reasonable to suppose that the mechanical fact of a little albumen and oil, forming a globule, with a non-vital nucleus, can explain the power of cod-liver oil in enriching the blood? He considers the change to be chemical rather than unchemical, for the following reasons:—

“First,—That drunkards hardly ever die of phthisis. Even if the proportion of deaths from phthisis in drunkards were only as great as in other people of more temperate habits, the circumstance would still be curious, since an irregular

life, *cæteris paribus*, predisposes to phthisis; but if it be the case, as I have stated, that drunkards rarely die of phthisis, the circumstance is still more remarkable. Drunkards of course die of numerous diseases, but, I believe, rarely of phthisis. Second,—Oils of all kinds, and fatty substances, are beneficial in phthisis. Third,—Pitmen, according to my observation and inquiries, very rarely die of phthisis."

He says, if these statements be true, what do they point to? and adds:—

"Oils, alcohol, and the carburetted hydrogen of mines, constantly inhaled by the coal-miners, all agree essentially in affording to the blood large quantities of combustible substances; and I may mention along with this, that I have known, during my experience, many instances of young surgeons of small fortunes being compelled to go a voyage in a Greenland ship, and thus saved for some time at least from hereditary phthisis. Now during these voyages there is a great consumption of nutritious substances, and perhaps not a little of alcoholic fluid. I infer that the efficiency of the oils and, generally speaking, the hydro-carburets in tuberculosis, is due to some chemical influence which they exercise. This is supposing my premises admitted."

With regard to the iodine and bromine contained in the oil, he says that in 100 parts there are only 0.04 parts of iodine; and as iodine exists in all fish, what possible virtue can it have, especially as it no doubt exists combined with alkaline or earthy bodies? A fabulous reputation seems attached to bromine. He has taken the bromide of sodium with his dinner, instead of common salt!—*Lancet*.

[We fear that Dr Glover, although criticising the recent work of Dr Bennett, has not carefully read over that portion of the first chapter which refers to the *modus operandi* of fats. The question is not whether the mechanical union of oil and albumen forms nucleated cells, which no one, so far as we are aware, has ever asserted. Dr Ascherson indeed thought that the globules formed by triturating oil and albumen were changed directly into blood corpuscles; but then these are not nucleated cells, but free nuclei. Such, however, is not the view of Dr Bennett, who simply believes these substances by uniting together form an emulsion, the minute molecules of which enter the chyle to form the molecular basis of that fluid. This, as is well known, further elaborated by the mesenteric and blood glands, serves as the blastema out of which blood is formed. As to the chemical theory, it is in no way incompatible with the structural one. That drunkards or pitmen seldom die of phthisis is a generalization which certainly requires more facts in its support before it can be received as an argument in favour of any theory whatever.]

SIR JAMES MURRAY ON DEODORIZING COD-LIVER OIL.

This invention, for which a patent has been obtained, consists in subjecting cod-liver oil, placed in high pressure cylinders made of silver or other suitable material, to the influence of carbonic acid gas in combination with continuous agitation. For this purpose an apparatus of a similar kind to that employed in the manufacture of soda-water is made use of, which should be sufficiently strong to withstand a pressure of 200lbs. on the square inch. By means of agitators or beaters attached to the revolving axle of the cylinder, the carbonic acid, injected by a force-pump, is brought into contact with every particle of the oil, which thus becomes deodorized, the unpleasant odour or flavour being discharged by suitable contrivances into the atmosphere. Instead of oil alone, any admixture of it with alkaline, mucilaginous, or other solutions may be treated in a similar manner to that above mentioned.—*Pharmaceutical Journal*.

[We have lately received from Mr Archibald of Newfoundland a gallon of cod-liver oil manufactured by a new process, secured to him by patent. The liver we are told is only subject to the influence of heat for a minute, and it is supposed the oil has not the same tendency to become rancid as that manufactured by different processes. It certainly possesses very little taste, and seems exceedingly pure. Of its cost we know nothing.]

DR CORSON ON IODIDE OF POTASSIUM AS AN ANTIDOTE TO THE INJURIOUS EFFECTS OF MERCURY.

The good effects of iodide of potassium in those cases which suffer from the injurious consequences of a mercurial course have been long recognised, but M. Melsens, of Paris, was the first to show, by an extensive series of chemical and clinical experiments, the mode of action of the remedy. His observations were published not long since in the "*Annales de Chimie et de Physique*," and have been translated by Dr Budd, of Bristol, in the "*British and Foreign Medico-Chirurgical Review*" for January 1853.

M. Melsens lays it down as an admitted principle, that mercury as well as lead combines with the animal tissues, and remains fixed in the system for years. This is proved by the well-known fact that persons, who have once freely taken mercury, years afterwards find gold coins discoloured by the mercurial in their perspiration; also that mercury has been sometimes detected in the body after death. Secondly, that in the body, as well as out of it, iodide of potassium acts as a powerful solvent of the compounds of mercury and lead, disengages them readily from the animal tissues, and drains them off through the kidneys. M. Melsens first proved that the iodide of potassium passes off principally in the urine, by taking large quantities himself, and then analyzing the different secretions of the body; he found that the urine was loaded with it, while the *fæces* scarcely contained a trace. It passes off with great rapidity; thus a person took seventy-seven grains, and in a few minutes the urine was charged with it. He then found that the compounds of the iodide of potassium with mercury and lead pass off by the kidneys in the same way. An extraordinary cure of mercurial paralysis in a looking-glass maker is given, in which the iodide of potassium in very large doses was taken for several months, and repeatedly the iodide of mercury was detected in the urine.

The great efficacy of iodide of potassium as an antidote to the slow poison of mercury and lead, M. Melsens proved by experiments on dogs, which were fed with the carbonate and sulphate of lead till paralysed, emaciated, and nearly dead, and then in a short time restored to health and flesh by the administration of iodide of potassium. He also cured three cases of severe lead paralysis among house-painters and workers in lead, and greatly relieved a fourth by the same remedy. In five cases of mercurial paralysis among gilders and workers in quicksilver, the iodide of potassium accomplished great relief or perfect cure in a few weeks.

As to the doses in mercurial or lead poisoning, M. Melsens recommends that we should begin with fifteen grains of the iodide in solution three times a day, and increase it as the patient will bear it. Such large doses, according to Dr Budd, require to be given on an empty stomach, and largely diluted. In milder cases, where mercurial paralysis is not induced, and the system is not highly charged with the noxious mineral, Dr Corson's cases seem to prove that smaller doses, if continued sufficiently long, are highly efficacious. The quantity that may be safely borne is often immense. Thus M. Melsens took from half a drachm to a drachm and a half daily for two months, two thousand grains in all, without any inconvenience beyond temporary coryza, and the eruption of a few pimples; and with a decided increase of appetite. One of his worst cases of mercurial paralysis took 2314 grains in three months, and one of Dr Corson's cases had five grains three times a day for eleven months with the greatest benefit.

In some cases the poison seemed to be liberated so rapidly by the remedy that it was badly borne. Sometimes profuse salivation was the consequence; and Dr Corson believes that the iodide of potassium never salivated, except by liberating mercury. In one case, a gilder, aged 65, suffering from mercurial tremors and paralysis, took eight grains three times a day; but such distressing ptyalism was produced, that he refused to continue the remedy. Dr Corson is in the habit of neutralising, as he imagines, the too severe effects of mercury

in syphilitic and scrofulous throat affections, by combining it with the iodide of potassium; and in strumous syphilitic cases he gives blue pill at night, and the iodide by day; he also suggests the exhibition of iodide of potassium as a protection to painters and workers in lead in seasons of special exposure.

It was in the hope of directing more attention to M. Melsen's observations that Dr Corson published, in confirmation of them, five cases, selected from many others. Four of these had legitimate evidence of scrofulous, or tuberculous taint, and the fifth suffered from Bright's disease; in all, therefore, from the constitutional state, mercury was contra-indicated; yet he says mercury was the only remedy capable of meeting the exigency of the cases. Extensive dropsy, with hepatic congestion, threatening obstructive inflammation of the throat, immense pleuritic effusion, suffocative laryngitis, and severe puerperal peritonitis, were the formidable maladies he had to combat; and he believes that the singular counteracting agency of the iodide of potassium enabled him to give mercury to salivation with perfect safety, notwithstanding the constitutional contra-indications. Mercury was given till ptyalism was freely produced; iodide of potassium was then administered, and no wandering pains, cachectic appearance, or other signs of injury, from the constitutional effects of mercury, ever appeared, although some of the cases were watched for years.—*New York Journal of Medicine.*

M. JOBERT ON THE ACTION OF ANÆSTHETICS.

According to this author, anæsthetics, as chloroform and sulphuric ether, affect first the surfaces with which they come in contact, and thereafter, by means of the circulation, the nervous system. The first symptoms of their action on the nervous system become manifested in the cerebrum; next in the cerebellum, and the anterior and posterior columns of the spinal cord; and finally, in the pons varolii. Hence the organ of perception first comes under their influence; then that of combined motion; next that of motion and sensibility; and finally the vital centre of the nervous system, (?) the pons varolii. The physiological action on the nervous system by which these effects are produced is, as yet, as obscure as that of opium or belladonna.

THE NUTRITIVE POWER OF ALIMENTS.

The nutritive power of different aliments is directly in proportion to the quantity they contain of the elements of the blood, in a state capable of assimilation. The latter clause is inserted by Dr Molleschott especially to exclude the valuation of food solely in the ratio of the quantity of nitrogen it contains. This proposal would class perfectly insoluble fibrin, coagulated vegetable albumen, etc., among nutritive substances, besides supposing that the body is increased by them, caffenin, and other similar principles, of which there is no evidence; except, indeed, what is afforded by the resemblance of their chemical formula to that of creatinine, and some of the uninvestigated animal alkaloids. Nutritiveness, then, depends in no slight degree on digestibility, and so far our investigations on this head will receive help from a consideration of the mechanical conditions of the several component parts of the alimentary body. While looking to the more obviously required constituents of food, it must not be forgotten that blood, and that which is to be formed of blood, contains also inorganic mineral elements. Soda, and potash, and chlorides, are required, and the food must possess these, or it cannot be said to be nourishing. Now, the researches of Liebig (quoted in Lehmann's "Phys. Chemie," part iii. p. 91) show a very different proportion of sodium and potassium in the extracts of the flesh of different animals. A pursuit of similar researches into the comparative amount of phosphate of lime, mentioned in the same place as being carried on by Schlossberger and Von Bibra, may lead to valuable results. The deficiency of this salt in the flesh of young animals was observed by Liebig and Berzelius, and may perhaps help to account for the inferiority of its nourishing power, which has long been empirically suspected.

The proper proportions in which the elementary principles of food should be

combined cannot be better estimated than by observing what they are, in that typical aliment which the Creator has prepared for us before reason makes us cooks, and when the "whole duty of man" consists in growing big and strong. The proportions in milk are, according to Dr Lehmann, 10 parts of plastic material, 10 parts of fat, 20 parts of sugar, and 0.6 of salts. But then we must remember that this golden age has an end, that other functions and other duties arise; the outer world makes its demands, mental and bodily deeds have to be done, and therefore (have we not a right to say *therefore*?) a different expenditure and a different income must be calculated on. This is obvious enough, but as to what the differences are we are completely in the dark. We cannot even guess why different young animals should be supplied, as they are, with different fare—why the calf should be allowed so little sugar and so much fat and casein (unless, indeed, we count butter and cheese dairies as part of the final causes of cows)—why the "poor little foal of an oppressed race" should be treated to as much sugar as his rider, and much more fat. Why should kids and lambs enjoy the sweets which puppies are denied?—*Brit. and For. Med.-Chir. Review.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIII., 1853-54.

January 4, 1854.—JOHN TAYLOR, M.D., Vice-President, in the Chair.

ON PUERPERAL ARTERIAL INFLAMMATION AND OBSTRUCTIONS. BY PROFESSOR SIMPSON, PRESIDENT OF THE SOCIETY.

Professor Simpson said that the subject of puerperal arterial obstruction and inflammation had hitherto been little studied; and though it was not of so frequent occurrence as venous obstruction and inflammation, yet he believed it to be more common than was generally supposed, and to be pathologically and practically important in many of its consequences.

No notices of the disease were, he believed, to be found in books on midwifery. The first case which had fallen under his observation, ten years ago, he had recorded in the transactions of the Edinburgh Obstetric Society, printed in the *Monthly Journal* for 1847. He believed that in this communication he had first suggested the opinion that the arterial obstruction might be due to the separation of a cardiac vegetation. The pulse suddenly ceased in the right arm, at a point where the obstruction was found after death. The patient died of phlegmasia dolens of the opposite arm.

A second case he had seen last summer with Dr Moir, presenting similar symptoms, and in which a similar diagnosis was made. The patient was a lady, aged 28, who was prematurely delivered at the seventh month. For three weeks she made a good recovery, when she became slightly feverish, with a general miliary rash on the skin, and occasional diarrhoea, accompanied by abdominal pain. The lochia were hemorrhagic for a few days. At this time the pulse was 120, and intermittent. Pains of a neuralgic type were complained of in the right limb; they subsequently moved to the left leg, and became permanent and severe. Seven weeks after delivery, sudden pain was complained of in the groin, over the vessels, which was relieved by leeching. A loud systolic bruit was now heard on the left side of the heart. The pulse became suddenly arrested as high as the elbow; but no pain was complained of at the seat of occlusion. In a short time, with the exception of the left arm, the pulse in the extremities ceased, though it returned a few days before death. Gangrene set in on the left foot ten weeks

after delivery. On a *post mortem* examination, the left heart was found filled with dark coagula. A large soft valvular excrescence was situated at the aortic aperture. It was composed of three portions, that on the right valve being the largest; and from its soft texture seemed to be of recent formation. The auriculo-ventriculor aperture exhibited a few traces of vegetations. At its bifurcation the aorta was obstructed by a firm conical coagulum, not attached to the arterial walls, which extended for a couple of inches into the iliac vessels, and which contained, in its interior, portions of structure physically and histologically the same as the cardiac excrescence. In both extremities the vessels were blocked up by similar coagula. There was an obstruction in the artery of the right arm, the vessel being much thickened and containing pus.

In a case kindly furnished him by Dr Macfarlane of Glasgow, the obstruction occurred ten days after delivery of a fifth child. Acute pain and numbness were complained of in the right arm, that continued till death, which took place three weeks after. There was no pulsation below the elbow; some could be felt in the right thigh. Gangrene commenced four or five days after. On dissection, the aortic valves were found encrusted with vegetations. The aorta itself was atheromatous, and firm fibrinous clots, with a nucleus of substance like a cardiac excrescence, existed in the middle of the brachial and in the iliac arteries.

Dr Lever, of Guy's Hospital, had communicated to him a case of gangrene of the left arm and leg, following acute rheumatism during pregnancy. Both in the arm and limb the pain was excessive. Vegetations existed on the valves of the heart, and were found in the arteries of both extremities. The veins contained fibrinous deposits also.

Dr Kirkes's most interesting collection of cases, brought before the London Medico-Chirurgical Society, principally referred to vegetations running up the carotid, and obstructing the vessels of the head, chiefly the middle cerebral, and producing ramollissement.

In a case, the particulars of which he had received from Dr Burrows, of St Bartholomews, hemiplegia suddenly occurred when the patient was recovering from symptoms due to over-lactation. A loud rasping systolic murmur was audible. The patient ultimately died of ramollissement of the brain. A *post mortem* examination revealed the existence of vegetations on the mitral and aortic valves. The left corpus striatum was a mere diffuent pulp. The middle cerebral artery was obstructed by a vegetation the size of a grain of wheat. The arteries of the limbs were not examined. Dr Burrows had at the very first made a correct diagnosis of the cause of the hemiplegia, etc., in this case.

In the cases to which he wished specially to draw attention, and in which the disease occurred subsequently to delivery, the following causes might be assigned for its supervention. 1. The separation of organised vegetations from the aortic valves. 2. The escape of recently formed unorganised masses of coagulum from the heart, and then discharge into the circulation. 3. The occurrence of a true arteritis. 4. Certain diseased conditions of the blood, or certain morbid matters carried along in its current. 5. In one of the cases he communicated there was supposed to be laceration of the inner coat.

1. In the five preceding cases, the cause was apparently the same, viz., the first which had been previously mentioned, and for the following reasons:—First, because in all the aortic valves were affected with vegetations; secondly, because one or more loose bodies, presenting a perfect similarity to these vegetations, were found in the obstructed arteries; and thirdly, the suddenness of the supervention of the arterial obstruction further showed the nature of the obstructing cause. Other circumstances rendered it probable that this cause was the true one. In three of the cases endocarditis was present either before or during pregnancy, as in Dr Lever's case, while in Dr Moir's the lymph on the valves was of still more recent origin, the result of puerperal endocarditis. Again, it was well known that similar morbid structures did separate from their place of growth. The so-called foreign bodies in joints from synovial membranes, polypi from mucous surfaces, and the cases described by the late Dr John Reid, of the

separation of polypoid bodies from the external surface of the peritoneum and pleura, might be instanced. But in the heart these vegetations were much more liable to be displaced, 1st, from the looseness of their attachment; 2nd, from the constant motion of the parts; and 3d, from the contact of strong fluid currents to which they were subjected. When detached they were hurried into the circulation, and arrested at last in vessels of their own calibre. If the mass were large, it would be arrested at the bifurcation of the aorta; if smaller, it might pass into any of the aortic branches, as the left carotid, though the reason given by Ruhle, that this vessel lay more in the current of the blood, was not so satisfactory. When the vegetation was arrested, new coagula rapidly formed around it, and inflammation of the internal tunics might then take place secondarily; in one (the 2nd) case, ultimately involving the femoral vein. Lastly, the mass might become disintegrated and broken down.

2. The second cause which he had mentioned had reference to morbid masses coming from the heart itself; for example, the globular polypi found in its cavity might, if small, be sent along the circulation. And though not actually separated, they may yet cause obstruction. The rough surface of the heart and valves in endocarditis might also suddenly arrest and entangle the fibrine of the blood, and polypi thus formed and becoming detached, produced similar effects. The experiments in which foreign bodies, as needles, etc., were introduced into the ventricle, illustrated this. Cruveilhier mentioned the occurrence of gangrene of an extremity after such an accident, a needle passed accidentally into the left ventricle forming a nucleus around which fibrinous coagula formed, and from which they separated and were projected into the vessels. And a case of rheumatic endocarditis, which supported the view in question, was read from Legroux. One case was mentioned by Dr Macfarlane, of puerperal arterial obstruction, where no vegetations, only coagula (in which no hard nucleus could be detected), were found to be the obstructing cause. The patient had had rheumatic fever, and died ultimately of general dropsy.

3. Of the third cause, viz., local inflammation of an artery, he gave an instance in a case which happened to Dr Duncan, when surgeon to the Royal Infirmary. Acute gangrene of both extremities occurred four weeks after delivery, and the patient speedily sank. No disease was found in the heart; but an adherent fibrinous effusion blocked up the aorta a little above its bifurcation, and passed down into the iliacs. It was a true arteritis. The aorta was thickened, and coagulable lymph coated the upper portion of the coagulum. A similar case of puerperal gangrene was read from Dr Cowan.

4. The fourth cause of arterial obstruction was certain diseased states of the blood, or morbid matters circulating in it. In puerperal phlebitis, we had particles of pus and fibrin circulating in the blood; at first passing from the uterine veins to the right side of the heart, and becoming deposited in the lungs, liver, etc. Now pus, or fibrin, was known to serve as nuclei for coagulating the blood; and in the pulmonary and other arteries, sometimes thus led to the formation of obstructing masses.

Two cases of puerperal obstruction of the pulmonary arteries from this source were given from Cruveilhier; and the possibility of various morbid appearances in the liver, spleen, etc., being explicable by the same cause, was dwelt upon.

5. And lastly, we might have possibly puerperal arterial obstruction from rupture of the internal coat of the artery, as had been so well described by Dr Turner. He referred to an interesting case, supposed to be of this kind, described by Dr Oke, of Southampton, so early as 1831. The patient was twenty-four years of age, and had aborted. Hæmorrhage, followed by uterine pain, ensued. Severe headache, with dimness of vision, now set in; and the left arm became cold and pulseless, and gangrene supervened. It was limited, however, to the integuments of the thumb and part of hand. There was no embarrassment of the respiration, and recovery followed. On inquiry, he learned that pulsation had returned,—the patient was still alive, and had no cardiac symptoms. Other cases of puerperal arterial obstruction, ending in gangrene, were described.

The symptoms of puerperal arterial disease varied as to the artery which was obstructed, or rather as to the function of the part which that artery supplied. As for example, in the case of occlusion of the middle cerebral artery, described by Dr Burrows, where the occurrence of hemiplegia in so young a subject, and the existence of valvular disease, led to the true diagnosis. Similar cases might be found in the work of Abercrombie, etc. The cases related by Testa, Corvisart, and Stokes, where sudden dissolution of an eye was associated with acute and chronic disease of the heart, he thought, might be explained by the obstruction of the ophthalmic artery by a separated vegetation. A case of sudden puerperal destruction of one eye—the patient being comparatively well and without phlebitis—was referred to this cause.

As to the seats of the obstruction, in Ruhle's cases it was the internal carotid, while in Kirkes's cases the middle cerebral artery was occluded. No instances were on record of vessels such as the coeliac axis or renal arteries being affected; but no doubt this arose from the want of due *post mortem* investigation. Cruveilhier and Bérard both speak of cases of gangrene of the lungs, in which they found the corresponding portions of artery obstructed. The vessels of the extremities, however, were those most frequently implicated. The symptoms referable to the limb were principally, 1st, stoppage of the pulse, though this was not a constant system; 2nd, there was pain in the affected limb, sometimes most severe; 3d, paralysis. Should the paralysis be imperfect, there was numbness and a pricking sensation in the affected limb. The pain, too, was generally great in the cases where the obstruction was not complete, which should be a sufficient warning to us for careful examination in puerperal patients. 4th, Gangrene sometimes supervened in the limb.

The cause of the pain was enveloped in great obscurity. In Dr Moir's case, he had been inclined to refer it to neuritis; but on dissection the nerves were found to be healthy. Mr Tufnell ascribed it to pressure on the nerves of the extremity, and Cruveilhier to inflammation of the internal coat; but it was too rapid in its occurrence to be the result of arteritis. In one of the recent cases of injection of perchloride of iron for the cure of aneurism, agonising pain instantly followed the injection, and gangrene ensued. A certain amount of arterial obstruction produced it; but how, it was difficult to explain.

He thanked the society for their attention to his hurried notes on a subject which he considered of great importance, and which he regretted that his engagements had prevented him presenting to the society in fuller detail. He hoped that the remarks which he had made might stimulate some one to pursue the subject more fully; and if so, he would feel that the object of his communication had been amply attained.

Dr Gairdner would like to know from *Dr Simpson*, whether the obstructing bodies were fixed or not; and also, what were the specific points of difference between the cases which they had heard that evening, and those related by *Dr Turner*?

Dr Bennett said that there were two points in the interesting communication of *Dr Simpson*, to which he wished to refer. One of these had just been alluded to by *Dr Gairdner*. Though the observations in the paper were of great importance, yet the connection between the cardiac vegetations and the obstruction of the artery must in his opinion be always difficult to prove. Clots and vegetations were frequently met with in the heart, while gangrene of the extremities was of rare occurrence, and had hitherto been attributed to an arteritis. He had frequently examined the obstructions in vessels so produced, and could detect no difference between such and those now presented to the society. The disease to which *Dr Simpson* had referred, such as cerebral ramollissement, though considered by Abercrombie and others to be analogous to gangrene, were in his opinion more analogous to fatty degeneration, dependent on an exudation from the capillaries—in short, it was a form of chronic inflammation of the brain. He thought also that the analogy failed in cases of the secondary affection of the eye.

The other point to which he would allude was dependent on the admixture of pus with the blood—the fourth cause described by Dr Simpson. He thought the opinions of Mr Henry Lee and others required further proof. At least, from cases of leucocythemia, which had fallen under his notice, he had seen reason to doubt their correctness. Along with Mr Barlow of this city he had injected pus into the jugular vein of an ass. No obliteration of the vessel was produced. The experiment was repeated; the vein was dissected for six inches, and two ounces of pus were injected in the direction of the heart. At first two distinct streams of pus and blood were observed, but no coagulation could be detected. On applying increased pressure to the handle of the syringe, the vein was seen to be entirely filled with pus; on diminution of the force the two streams were again visible, but still there was no coagulation. Two ounces more were then injected, and the animal was released, exhibiting no signs of uneasiness. These experiments had completely satisfied him, and he regarded the generalization sought to be established by Mr Lee as erroneous, and that pus does not necessarily coagulate the blood in the living vessels. He regarded the existence of pus within the clots as very questionable. The observations of Mr Gulliver appeared to have been overlooked by Dr Simpson; and Dr Stokes in his late work when treating of “Cysts in the Heart” made the same omission. One striking case had fallen under his (Dr B.’s) notice where the entire right ventricle of the heart was filled by a fibrinous mass, which contained in its interior two ounces of an opaque unctuous fluid closely resembling pus; but which, under the microscope, was found to be destitute of pus corpuscles, and to be composed of granules and the debris of the disintegrated clot. Now, in many of the clots which had just been passed round, he believed that the so-called pus resulted from the disintegration of the interior of the clot.

Dr Mackenzie said that no doubt existed as to the occurrence of obstruction as the effect of local arteritis, and he thought that Dr Simpson had adduced satisfactory proof that evening as to the same effect being produced by detached aortic vegetations, not to speak of the trustworthy observations of Kirkes, Virchow, and Tufnell, to which allusion had been made. The arguments of Dr Bennett were in his opinion completely met by the two facts,—*First*, that in the cases mentioned by Dr Simpson previous symptoms of arteritis did not exist. There was no pain complained of along the artery before the obstruction occurred, but then the pain was sudden. *Secondly*, that the vegetations were actually recognized; and the differences between them and ordinary coagula were too well marked to allow of any mistake.

Mr Tufnell’s case he had had an opportunity of examining. There had been great pain, obstruction of the vessels below the knee, and a pulsating tumour made its appearance in the ham. On a post-mortem examination the popliteal artery was found to be obliterated, the canal of the vessel being contracted and plugged with fibrine. The appearances presented were those of obstruction from a simply mechanical cause independently of local arteritis, and the history of the case, the absence of arteritis, and the coincidence of the presence of loose and easily detached warty excrescences on the mitral valve, confirmed the view adopted by Mr Tufnell, that the obstruction was referable to the detachment of one of these excrescences, and its arrest at the bifurcation of the popliteal artery.

The deposit which occurred round such a vegetation or other foreign body arrested in an artery had been spoken of by Dr Simpson as lymph, the product of inflammation excited by the presence of the foreign body. The occurrence of inflammation, however, did not seem necessary to account for the deposition, which seemed rather to consist of the fibrine of the blood mechanically deposited around the vegetation.

The sudden accession of severe pain, which Dr Simpson had mentioned as apparently coincident with the arrest of these vegetations in one of the arteries, was a curious circumstance. Dr S. had referred to the same occurrence in the treatment of external aneurism by compression. In these cases the sudden attack of pain had occurred simultaneously with the complete solidification of the

contents of the aneurismal tumour and consequent complete obstruction of the artery. It was difficult to account for the sudden accession of pain in these circumstances, for it did not accompany the sudden obstruction of an artery, as by ligature. This circumstance, however, confirmed the views taken by Dr Simpson as to the nature of the cases under consideration.

A strong argument in support of the opinion that the changes in the brain, observed in some of these cases, resulted from obstruction of the vessel and not from simple arteritis, was in his opinion, obtained from the fact that ramollissement had been observed in some cases as a sequel of ligature of the carotid. Dr M. referred to two cases of aneurism at the root of the neck, which had been treated by Mr Fergusson by breaking down the solid contents of the tumour by manual pressure on the tumour; in both of which strongly marked cerebral symptoms had followed the sudden obstruction of the carotid artery. In one of the cases hemiplegia had resulted within an hour or less of the sudden obstruction of the vessel.

Dr Moir added, with regard to the case related by Dr Simpson, that he had felt the pulse between 9 and 10 P.M., and next morning it was absent. No pain had been complained of previously along the vessel, which was incompatible with the existence of arteritis. One remarkable circumstance he might mention, that in one limb and arm no pain had been felt, though the arteries in both were obstructed.

Dr Christison mentioned the particulars of a case of spontaneous occlusion of an artery, which had lately come under his notice, and which, though not puerperal, was interesting, inasmuch as the seat of the pain was carefully noted, a point about which there was much confusion in the recorded cases of the disease.

A lady, about 40, while on a visit at St Andrews, had what appeared an ordinary cold, and afterwards a very circumscribed pneumonia in the interior region of the left side. On the 5th day, when she came to Edinburgh, the symptoms, which were obscure, seemed to indicate old pleuritic thickening and recent very limited pneumonia; the only discoverable indications of an affection of the heart, were, that its first sound was double and its impulse rather increased, and that she complained of indescribable uneasiness in the heart, but without palpitation. She had been very long liable to dyspnoea in climbing, but had otherwise enjoyed good health. In two days more, being better, yet, far from well, it became necessary for her to travel to Berwickshire, where she got worse, but with symptoms rather of general fever than of a pulmonary disease. On the 12th day she returned to Edinburgh, labouring under apparently a gastric fever, for which she was treated with brisk purgatives by a medical friend, and with evident advantage, so that she was able to sit up for several hours. On the evening of the 17th day, after having cheerfully enjoyed the society of her family, she went to bed in good spirits, and to all appearance quite convalescent; when, in a few minutes, she suddenly complained of acute pain, numbness, and loss of power, in the right hand, exciting a fear of approaching paralysis. Dr Christison, who was immediately again sent for, found that no pulse could be detected at the wrist, or any where higher up the arm, even in the axilla; but there was distinct pulsation, both to the finger and stethoscope, over the subclavian artery, very near the outer end of the clavicle. The state of the arm was peculiar. There was acute pain in the hand, but not higher up, and without any tenderness, even in the axilla, close to the seat of occlusion. The whole arm felt heavy and numb; yet sensation was quite entire. The power of motion in the arm seemed to the patient to be gone, and the movements of the fingers were very feeble; but the movements of the wrist were stronger, those of the elbow stronger still—and those of the shoulder entire. The arm was everywhere very cold; but warmth was restored by external means without much difficulty.

At first there was some feverish excitement. For a day or two at this time the double first sound of the heart was displaced by a short insignificant bellows murmur. The former slight pneumonic symptoms did not return; but she had occasionally cough. Under perfect rest, warmth, low diet, and opiates and laxa-

tives, the fever ceased, the pain of the hand became endurable, and the temperature of the arm could be more easily maintained, the pulse however remaining entirely absent from the axilla downwards. On the 23d day she was able to travel 80 miles by railway to her residence in Ayrshire. Three days afterwards there was a temporary increase of pain and numbness; but her general health continued good, and hopes were entertained that recovery might take place in the usual way. In the sixth week, however, matters began to look unfavourably. The right femoral region became painful and tender; an irregular fever again established itself; and although both of these affections gradually subsided, she never got rid of a peculiar haggard expression, which constantly excited a fear of lurking mischief. On the 63d day from the beginning of her illness, and the 46th from the first symptoms of occlusion of the axillary artery, she was again so well and cheerful as to be in the drawing-room for some hours with her family; the pain in the arm was almost gone, and its temperature was well preserved. Yet the heart's action had been for some days forcible; and the beating was sometimes difficult and interrupted by dry cough. That day she was attacked rather suddenly in the morning with urgent dyspnoea, frequent cough, and copious frothy expectoration. These symptoms subsided towards evening, returned next day, subsided again, but in the evening returned with increased force. The expectoration became quickly more and more difficult, insensibility supervened, and she died during the night, 49 days after the axillary artery became occluded. There was unfortunately no opportunity of ascertaining by dissection the nature of the occlusion, or the cause of death; as to both of which various opinions might be formed. The case is of interest, as showing that the seat of pain and other uneasiness may be, not in the seat of occlusion, but in the most distant part of the limb.

Dr Matthews Duncan said that his difficulty in regard to the cases described, was to see any pathological connection between the arterial disease and the puerperal state of the patients. It was easy to trace, in most cases of disease of the veins, the pathological connection of phlegmasia dolens with the peculiar condition of the open uterine sinuses on the inner surface of the uterus; but, in the present cases, he could see none, unless it might be that the excitement of the circulation after delivery facilitated the separation of the vegetations.

Dr Simpson said that in the first set of cases which he had read to the society the proof, that separated cardiac vegetations were the cause of obstruction, seemed to him in the highest degree probable from the facts, that the obstruction was sudden and unattended by the symptoms of arteritis; if seen early, the obstructing body was loose and not attached organically to the walls of the vessel, and it presented characters similar to the valvular excrescences, as was well shown in a case published by *Dr Burrows*, where both the cardiac vegetations and the body obstructing a cerebral artery presented marks of calcareous degeneration. The principal distinction between his cases and those of *Dr Turner* was, that while in the former there was no laceration or puckering of the internal coat, it was invariably present in *Dr Turner's* set of instances. He readily admitted that gangrene did not always supervene on obstruction of a vessel; and he had no doubt that in many cases, which had escaped notice, the effects might probably be limited to atrophy and other lesions of organization and function. As to obstructing causes, such as he had described, not having been frequently met with in the dead body he thought it might be easily explained by the fact that they were never looked for.

The case published by *Dr Burrows* was a complete answer to the objections of *Dr Bennett*, as to the production of ramollissement of the brain; for the valves presented calcareous vegetations, no inflammation of the obstructed artery could be detected, but impacted in the vessel there was a calcereous mass similar in all respects to the vegetations on the valves, and the portion of brain supplied by this impervious vessel was in a state of ramollissement. Again in *Mr H. Lee's* experiments, the result was not an invariable one, but was only met with in certain diseases. The conclusion to which *Dr Bennett* had come was ex-

plained by the circumstance, that the subjects of his experiments were in a state of health. He was quite of Mr Gulliver's opinion as to the possibility of the fibrin in the interior of the clots breaking down and presenting a semi-purulent appearance ; indeed, he was inclined to ascribe the phlegmasia dolens which had occurred in one of the cases to this very process ; but in the case he had related, the arterial tunics were thickened, and evidently the seat of inflammatory action, which in his opinion explained the presence of the pus. The influence of the puerperal state on the liability to arterial obstruction and inflammation was simply a fact, whether we could explain it or not. We could not account any more for the liability to the occurrence of puerperal mania. He had adduced ten or twelve cases of arterial obstruction in the puerperal state ; and perhaps there were not a greater number yet of the same disease in non-puerperal subjects. Something in the way of causation in the puerperal state might be due to the hurry of the circulation during labour, and to the occasional presence of puerperal endocarditis, and to that liability to morbid states of the blood in the puerperal state, which was also, he believed with Bouchet and Mackenzie, the origin of phlegmasia dolens.

Dr Bennett said it would be a matter of great practical importance to prove that an arteritis was invariably accompanied by gradual pain, and the impaction of a clot with sudden pain. *Dr Mackenzie*, however, had not shown this. Assertion was neither argument nor demonstration.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

SESSION 1853-54.

MEETING III.—*December 17, 1853.*—Professor BENNETT, President, in the Chair.

1. The Committee on the Structure of Muscular Fibre was reconstituted, and Mr Lister was added to the number.

STRUCTURE OF THE TORBANEHILL MINERAL.

2. The following report of the committee appointed by the Physiological Society to investigate the structure of the Torbanehill mineral was then read :—

The committee having satisfied itself of the general structure of undoubted coal, and recognised that in its longitudinal section it presented a fibrous structure, and that in transverse sections it exhibited an appearance of rings, presumed to be sections of the elongated cells of a plant, also directed its attention to the ashes of coal, in some specimens of which siliceous fibres and black opaque plates could be recognised, with circular openings.

The committee then satisfied itself that these appearances were not observable in the Torbanehill mineral, and directed most of its attention to a solution of the question, whether the appearances it presented were or were not indicative of an organic structure. The sections of the mineral, made in any direction, whether longitudinal, transverse, or oblique, present under a magnifying power of 250 diameters linear, two distinct substances—1st, A yellowish transparent material, of an irregular rounded form, with indented margins, varying in size from the $\frac{1}{2000}$ to the $\frac{1}{200}$ of an inch in diameter. These rounded masses are imbedded in a dark brown or blackish substance. Towards the centre of the mineral bed, these sections on being ground very thin, become lighter and almost transparent. The whole being composed of the yellow matter, the masses apparently melting together. On the other hand, towards the upper layer of the mineral bed, the two substances were mingled with masses of a brown bistre colour, containing numerous scalariform vegetable ducts.

On magnifying the yellow masses formerly described with a power of 50 diameters linear, they were seen to present a finely radiated structure, from the centre towards the circumference, with occasional cracks, irregularly dividing

the mass into various compartments in the form of a rosette. It was distinctly determined that no appearance of a cell wall existed, or any kind of distinct boundary line, which could be considered as such, and this, although in many sections, these yellow masses were distinctly isolated—neither did they contain contents of any kind whatever.

From all these facts the committee are of opinion,—

1st, That no appearance of vegetable texture occurs in the Torbanehill mineral, except at the upper part of the bed, where it comes in contact with decided layers of coal, or other coal strata.

2d, That the yellow masses which by some have been considered to be cells, are homogeneous masses of a bituminoid or resinoid material, which have a tendency to split or crack in a radiated manner.

3d, That the Torbanehill mineral is widely different from all the kinds of coal examined by the committee.

J. HUGHES BENNETT, Reporter.

Mr Kirk agreed with Dr Bennett in regarding the lemon-coloured bodies as being unorganised, and the radiatory markings as due to a crystalline arrangement, which was supported by the fact that when cut in a certain direction they had a very appreciable action on polarised light. He had been induced to come to a different conclusion, however, in regard to the nature of the mineral from observation on the structure of the matrix in which the transparent bodies were imbedded. In the first place, we often find masses of scalariform tissue, the structure of which is as complete as it is in the recent plant. In other portions there are to be seen large masses of disintegrated tissue in which the various rods composing such vessels remain quite entire, although their relations to each other have been destroyed. In most parts of the mineral, however, the disintegration of tissue had gone much farther, so that nothing could be discovered but small fragments of the original rods. When such portions were ground sufficiently thin they were of a bistre colour. (Sections showing these facts were exhibited to the Society).

In several parts there existed no matrix whatever, in such sections there was a continuous resinous mass in which were imbedded here and there small fragments of vegetable tissue, and sometimes spore cases.

The following were the changes observed to take place on the application of heat to the sections. First, the transparent yellow bodies became quite black and opaque, as well as the matrix which was previously of a bistre colour; thus indicating the presence of carbon in both structures. On elevating the temperature, there remained nothing but a charred matrix in which there were empty spaces, once occupied by the yellow masses, which had volatilised without leaving any solid residue, and seemingly without previously melting.

On increasing the heat to near redness in the open air, the carbonaceous matter of the matrix was driven off, when there was left a small amount of ash which was of very small bulk compared with the original, and consisted entirely of granular matter.

We may conclude from the preceding facts that at the time of the formation of the deposit there had been some sort of lacustrine flora composed principally of plants allied to the ferns, and that the herbaceous and woody parts of such plants had been reduced to a mud, in which the yellow bodies had been imbedded, either then, or that they had been formed afterwards by the continued action of an elevated temperature.

Dr Bennett could not satisfy himself from the preparations shown by *Mr Kirk*, that the dark matrix was composed essentially of disintegrated scalariform tissue. No doubt in the section then placed under the microscope, the scalariform tissue was broken up, but this was evidently the result of the mechanical rubbing down of the soft portion of the mineral, from which the particular specimen was prepared. Numerous other sections, now in his possession, nowhere exhibited the matrix to be composed of such organic friable structure, but, on the contrary, of an amorphous, mineral material, opaque when collected

in mass, not of a red bistre colour, but dark brown, passing into black, and so firm that it could be ground fine with the greatest ease. The mud, composed of broken-down herbaceous and woody parts of ferns, supposed to constitute the matrix by Mr Kirk, had certainly never yet been demonstrated.

Mr Barlow, like Mr Kirk, had succeeded in driving off the transparent yellow substance by heat, so as to leave, as it were, the skeleton of the earthy material entire.

CHEMICAL ANALYSIS OF DISEASED WOOD.

3. *M. de Chaumont* read a communication on the analysis of the specimen of diseased wood exhibited by Mr Kirk at last meeting.

This wood seems to have undergone the process of eremacausis, probably slightly modified. The composition of healthy wood is woody fibre, sap (fluid when fresh, dry when old), a certain amount of resinous matter, and ash. The sap consists of starch, gum, sugar, etc. There is also a small quantity of a proteine compound, vegeto-albumen, or fibrine. The sap, albumen, and resin, dry up when the wood is old. Woody fibre is said to be $C_{12}H_{10}O_8$ in the willow, and $C_{36}H_{22}O_{22}$ in the oak; while Mulder considers the general formula to be $C_{40}H_{28}O_{26}$. The last is probably the most correct. Woody fibre is made up of two principles,—cellulose $C_{12}H_{10}O_{10}$, and lignine (the sclerogen of Payen), said to be $C_{36}H_{24}O_{20}$. If the latter formula were correct, it is evident that $C_{36}H_{22}O_{22}$, for woody fibre generally, must be wrong, since no possible combination of cellulose ($= C_{12}H_{10}O_{10}$) and lignine ($= C_{36}H_{24}O_{20}$) could ever give rise to such a formula. But I always look with great suspicion on any organic formula where the number of carbon equivalents is odd, and I therefore think it most improbable that the true composition of lignin is $C_{36}H_{24}O_{20}$. Now, *two* equivalents of this with *four* of cellulose give nearly exactly *three* equivalents of woody fibre, according to Mulder's formula $C_{40}H_{28}O_{26}$. This gives for composition in a hundred parts:—

$$\begin{array}{r} C = 50.42 \\ H = 5.89 \\ O = 43.69 \\ \hline 100.00 \end{array}$$

whilst if the per-centage be calculated from the sum of two equivalents of lignin ($= C_{36}H_{24}O_{20}$) and four of cellulose ($= C_{12}H_{10}O_{10}$), we obtain, $C_{120}H_{88}O_{80}$;

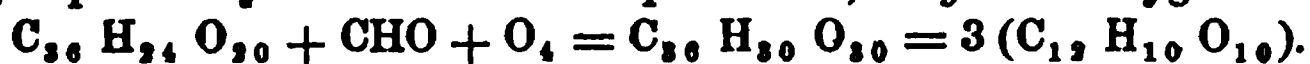
$$\begin{array}{r} C = 49.723 \\ H = 6.077 \\ O = 44.200 \\ \hline 100.000 \end{array}$$

This is very nearly the result of Petersen and Schödler's analysis of the wood of the ash (*Fraxinus excelsior*).

$$\begin{array}{r} C = 49.356 \\ H = 6.075 \\ O = 44.569 \\ \hline 100.000 \end{array}$$

The slight variation may be accounted for by the fact of the oxygen of the *ashes* being included in the calculation. As woody fibre consists of cellulose, which forms the walls of the cells and vessels, and lignin, or sclerogen, which forms the darker incrustation, which supports and gives hardness to the structure, it is clear that any formula representing its composition as a whole, must

be one not less than the sum of the formulæ of its two constituents taken in their relative proportions; and, therefore, although that of Mulder appears to be nearer the truth than the others which have been proposed, I yet prefer the formulæ $C_{120} H_{88} O_{80}$, obtained by the sum of four equals of cellulose and two of lignin, or $C_{50} H_{44} O_{40}$, from two of cellulose and one of lignin. The specimen of altered wood under consideration is exceedingly light, and burns slowly, but without flame, like common touch-wood, leaving a very small quantity of ash, entirely soluble in hydrochloric acid, and consisting apparently of salts of potash and lime. The wood yielded little or nothing to water. Alcohol and ether removed scarcely anything; dilute nitric acid removed a small quantity of matter in solution, from which it was precipitated by potash. The precipitate redissolved in sulphuric acid without blackening, showing that it was not lignine. Dilute potash became of a light brown colour, and dissolved some matter, which was precipitated by acids. This might be either a vegeto-proteine compound; ulmine, the peculiar principle of mould; or resin. The first is unlikely, as the nitrogenous would probably be the first substances to undergo decay. The second *might be*, from the presence of mould forming a soil for the growth of the fungus observed by Mr Kirk, but as the precipitate was pale yellow, whilst that of ulmine is dark, it is not probable that it was that principle. Lastly, resin is dissolved by potash, and precipitated from the solution by acids, in yellow flocculi. This answers well to the present case. Besides which, the portions of wood treated with alcohol and ether yielded very little to the dilute acid, a further argument in favour of its being resin. Strong sulphuric acid blackened the wood, and disintegrated it, dissolving out the cellulose. This blackening gave place to a pale brown colour on the addition of nitric acid. Strong nitric acid added to the wood first reddened it, giving off copious fumes of hyponitric acid, and subsequently left the wood quite white, having dissolved out all the lignine. The latter principle was evidently in greatly smaller quantity than in ordinary wood. The wood, after being thus treated with nitric acid, showed under the microscope the ordinary structure, except that the sclerogen, which was present before in small quantity, was now entirely absent. It was now entirely soluble in strong sulphuric acid, without blackening, giving a clear, colourless solution. In the process of decay, the constituents of the sap have thus disappeared, as well as the nitrogenous matters, which would probably be the first to suffer. The resinous matters have also, to a certain extent, disappeared. The lignin, or sclerogen, which gives hardness and inflammability to wood, has, to a great extent, disappeared, losing carbonic acid and water, and being probably partially converted into humus for the growth of the fungus, while another portion of it, by attracting water and oxygen, may have become converted into cellulose. This change may be easily explained by the addition of *six* eqs. of water, and *four* of oxygen to lignin,



After the wood was treated with dilute nitric acid, as above-mentioned, the concentrated acid removed much less pure lignin. Possibly the substance dissolved by the dilute acid, which was precipitated by potash, and soluble in other acids, may have been an altered form of lignin, acting to a certain extent the part of a base, combining with acids, from which it was again precipitated by alkalis.

The general result has been, then, that some of the minor constituents of wood have disappeared; that the lignin is present in greatly diminished quantity, and probably to some extent in an altered form, while the cellulose seems to have suffered little or nothing.

WAXY DEGENERATION.

4. *Dr Bennett* exhibited a specimen of waxy spleen, with distinct translucent Malpighian bodies closely crowded together, and forming the chief part of the substance of the organ. He also showed a waxy kidney from the same subject.

Dr Sanders demonstrated, under the microscope, a section of the liver from the same case, in which fatty particles were seen accumulated at the circumference of the lobules, while their central part was pale, and the nuclei indistinct.

Dr Bennett, when he first became acquainted, several years ago, with this waxy degeneration of various tissues, had supposed that it was an ultimate form of fatty alteration. He was now satisfied, from numerous observations, that it was a primary alteration of the cells, and though frequently associated with fatty degeneration, was not essentially connected with it.

Dr W. T. Gairdner remarked that, while acting as Pathologist in the Royal Infirmary, he had frequent opportunities of observing the condition of the spleen in the specimen exhibited, and of remarking its connection, in some cases, with a similar condition of other organs, especially the liver and kidney. The firmness of the organ, its *waxy* character, and the distinctness and transparency of the Malpighian bodies, were apparent; and, though never very accurately described, had been noticed by various writers. Dr Sanders, however, had pointed out to him (Dr G.) about a year ago a peculiar alteration of the contents of the sacculi, of which no adequate notice, if indeed any notice, had been taken, and which he hoped Dr S. would take this opportunity of describing and demonstrating to the Society.

In the liver, the *waxy degeneration* was of great importance. The organ was usually pale or fawn-coloured—sometimes, however, congested, and sometimes mottled; the distinctions of the acini were obscured, the cut surface was smooth, dry, and homogeneous. The organ was extremely slow to decompose; its specific gravity was notably increased, being sometimes in portions above 1080. Microscopically, there was a change in the entire structure, but chiefly in the glandular epithelia, which were compressed, irregular in form, and had atrophied nuclei. They were also altered in a manner which could scarcely be described, but were easily recognised after being once seen, presenting a peculiar *horny* refraction, and giving the idea of a substance much denser and more resistant than the normal elements of which these cells are composed. Sometimes the cells cohered in masses, sometimes they were very small, at others as large as the ordinary cells, making allowance for compression. This condition of the liver was found to be the source of a large proportion of the cases of hypertrophy of that organ; it was common in scrofulous, syphilitic, and other chronic exhausting organic diseases. In phthisis, it was the most common of all the types of diseased liver,—far more common than the fatty liver, but often, indeed, occurring along with that condition,—while in other cases no fatty matter was to be found. Dr G. considered it certain that these two forms of disease had often been confounded by authors. He himself had seen this mistake very frequently made, although to an instructed observer the difference was usually well marked. Rokitansky, although distinguishing the two diseases, regards them as mere varieties, and looks upon the “waxy” material as a distinct deposit allied to fat, which it certainly is not. Louis, in his work on phthisis, manifestly confounds together the waxy and the fatty liver under the latter designation, and the confusion thus originated has passed into numerous other works. Dr G. believed that most of the extreme instances of hypertrophy of the liver were due to the waxy degeneration, simple or combined, with fatty infiltration, for the fatty infiltration alone might exist in a very exaggerated degree without much hypertrophy. Further, in the purely fatty degeneration the specific gravity was not raised, but reduced; he had seen it as low as 1005.

In certain forms of Bright's disease of the kidney, a precisely similar alteration took place in the histological elements of that organ; but here the vessels, and particularly the arteries and the Malpighian tufts, were the first portions distinctly affected. In the spleen, on the other hand, the most marked change was in the contents of the sacculi. It was not very clear to what these modifications of the textures should be ascribed; but he (Dr G.) was inclined to regard it as a peculiar modification of the protein compounds, in virtue of

which they approached the character, and possibly the chemical constitution of keratin, and presented a much greater resistance than in the normal state to acid and alkaline solvents. The nature of this change, however, was an inquiry of great promise for the chemist. Dr G. was inclined to believe that ultimately the gelatinous tissues underwent a similar metamorphosis, for he had often observed that in exaggerated instances of waxy degeneration organs lost much less both of weight and bulk by boiling than under other circumstances.

It was of some pathological interest, and might aid the investigations of the chemist, to observe that this change in the albuminous tissues was not always confined to the original structures of the body. Very pure instances of this degeneration are often presented in old deposits in serous membranes, which in these cases lose their fibrous character and become dense, non-vascular, semi-transparent, and horny (*verhornung* of the Germans). Tubercle not unfrequently assumed a similar appearance. Finally, Dr G. remarked that in several cases of cancer of the liver which he had examined, and in which the liver presented the *waxy* character to a marked degree, the cancerous nodules had undergone a precisely similar change; and on microscopic examination the cancer cells could not be distinguished from those of the liver, except perhaps by a slightly lighter colour.

Dr Sanders remarked that the waxy alteration of the spleen, though generally overlooked and unknown, was a comparatively frequent and most important lesion. In a more or less advanced stage it was found in a large proportion (about 10 per cent.) of the bodies examined at the Royal Infirmary, and it was usually associated with the waxy liver and kidney in certain forms of Bright's disease and tubercular phthisis. It indicated, therefore, a constitutional cachexia of a grave and fatal character. In the most marked examples of the lesion, the spleen is enlarged and has a swollen appearance, its weight and density are greatly increased, it feels hard like wax to the touch, and it cuts with a smooth dry section. On exposure to the air, the cut surface becomes of a bright red colour, and the Malpighian bodies are seen large and distinct, forming rounded colourless *transparent* granulations, about the size of a pin's head, slightly prominent, and so hard that they may be picked out by the point of a knife. The pulp is greatly diminished in quantity, and in some places is entirely wanting, so that the clear granulations are crowded together, or join in small continuous masses of a lobulated form. A small arterial twig can be observed by the naked eye or the microscope passing through these Malpighian bodies at their *central* part, as was seen in the preparations which were formerly shown to the Society. At the central part, also, the transparent alteration frequently does not take place, and is always less advanced there than at the circumference. In sections under the microscope, which fortunately, owing to the firm consistence of the organ, can be easily made, the characters are even more striking than to the naked eye. Under a power of 40 diam. the Malpighian bodies appear as large clear spaces of a circular or oval form, surrounded by the dark, red-coloured pulp. Under high magnifying powers (250 diam.) the nature of the alteration is distinctly observed. The normal corpuscles of the Malpighian bodies are found converted into and replaced by masses of a colourless, dense, highly translucent, homogeneous material. At first these masses appear to exhibit no structure of any kind, but on careful examination the outlines of irregular, mis-shapen cell forms may be distinguished. From their toughness, the masses are with difficulty broken up into the particles which compose them. These particles vary much in size, from that of a normal spleen corpuscle to four or five times that size; they are generally larger than the splenic corpuscles; they have no nucleoli or other structure, and present various rounded and angular shapes. Besides its remarkable physical properties, this translucent substance is very little acted on by the usual reagents: it does not lose its transparency when treated with acids, or alkalies, or alcohol;

chromic acid and iodine render it yellow and brown, but do not alter its homogeneous aspect; indeed, its characters are so marked and so permanent, that there is no difficulty in identifying it wherever it is met with. It is the formation of this translucent matter which constitutes the waxy degeneration.

It should be noticed that several varieties of the waxy spleen are found. The transparent Malpighian bodies are sometimes large, sometimes very small; and in the latter case are best seen after exposure to the air for a little, or after being put in alcohol or solution of chloride of zinc, which bring out the waxy appearance very distinctly. The transparent bodies show sometimes a white colour, like milk or suet; often they have a red tint, and have sometimes the colour of port wine, to the naked eye; sometimes they are stained purple by extravasated blood; but the waxy material under the microscope is the same. The spleens are almost always firmer than natural; a few examples, however, occur where the spleen is *softened*, and as in these the Malpighian bodies are generally small, perhaps it may be an earlier stage of the lesion. Another form of waxy spleen, which Dr Gairdner had observed, must be distinguished from all the preceding. It presents the waxy hardness, the smooth section, and other characters of the lesion, but the *Malpighian bodies* are not visible. It is unnecessary to do more than notice this, as the degeneration in which the Malpighian bodies are principally affected, is alone at present under consideration.

What is the origin and the pathological relations of the transparent waxy material? It seems highly probable that it is produced by a peculiar transformation of the normal corpuscles into some imperfect protein compound; for the normal elements are seen apparently in stages of transition, becoming pale and altered in shape, and aggregating together in homogeneous masses. It is possible, however, that some structureless plasma of this waxy nature may infiltrate the textures, giving rise to the alteration of the cells and their cohesion together, and affecting other textures, such as the bloodvessels, whose walls often appear thickened and translucent. However this may be, the great hypertrophy and condensation, which are very remarkable features of the waxy lesion, indicate an increased as well as an altered nutrition of the organs.

The most interesting point of the pathological relations, is the fact that the waxy conditions of the liver and kidney depend upon the same change as that which takes place in the spleen. On this point Dr Gairdner and Dr Sanders had made repeated examinations. In the advanced stage, many of the acini of the liver, the Malpighian bodies, and other parts of the kidney, have undergone alteration into this transparent substance, which exhibits under the microscope the same physical, and, so far as tested, the same chemical characters as in the spleen. We must therefore class these conditions of the liver, kidney, and spleen together, as being similar not only in outward aspect but in the real nature of the affection. It is also apparent that the waxy lesion does not depend on fatty degeneration, for the fatty condition of the spleen is not met with; nor upon mere increased growth and compression of the cells, because the bloodvessels are also affected; but it depends on a peculiar alteration in the nutrition of the textures, and as it generally occurs simultaneously in several organs, it must arise from deep-seated constitutional causes, and may very probably be connected with an altered condition of the blood.

In regard to its literature, the morbid state of the spleen had been first well described by Dr Hodgkin, and had been noticed by Bright and Carswell. It is one form of the speck-miltz (baconny spleen), probably the "fishrogen miltz" of the Germans. The microscopical appearances of the translucent matter have been observed by Dr Handfield Jones, in a case described in the reports of the Pathological Society of London for 1852. But the subject has never attracted notice, nor has the true nature or frequent occurrence of this lesion been known to pathologists. Imperfect as our knowledge still is, there is sufficient to show the necessity of renewed and careful investigation, from which most important results may be expected.

MEETING IV.—December 31, 1853.—Professor BENNETT, President, in the Chair.

ENCHONDROMA.

1. *Dr Bennett* showed a specimen of enchondromatous tumour, which had occurred in a gland of the neck, and was not connected with bone, as such tumours usually are. It presented the microscopical characters of cartilaginous growths, and was in some parts firm and dense, in other parts it was softened and disintegrating.

EXPERIMENTS ON THE ACTION OF THE HEART.

2. *Dr Bennett* gave an account of three experiments on dogs, which had been made at the Veterinary College, in the presence of Drs Cartwright, Gairdner, Sanders, and Barlow, in order to test Dr Cartwright's "diastolic theory of the heart's action," brought under the notice of the Society at its second meeting.

Experiment 1. A tolerably large dog having been rendered insensible by chloroform, had the ribs rapidly removed on its left side, the pericardium cut through, and the heart fully exposed. The organ was of a cocoa-nut shape, and its apex was rounded. The action was irregular, and no tilting forward of the apex could be observed. It was difficult, in consequence, to follow with exactitude the order of contraction in the auricles and ventricles, and no definite result could be arrived at, although the long first sound apparently preceded the pause, and seemed to be synchronous with the dilatation.

Experiment 2. In a small dog, rendered also insensible by chloroform, the heart was exposed in the same manner as in Experiment 1. It was of a conical shape, the apex pointed, and it was seen to be tilted forward at each contraction of the ventricles. The actions were regular and could be easily observed, the long pause being followed by the ventricular contraction, and this immediately succeeded by the dilatation, which was very rapid. On placing the stethoscope on the heart, he (Dr B., as well as Dr Gairdner) heard the longer first sound to be synchronous with the ventricular contraction and tilting forward of the apex.

Experiment 3. A larger dog than the last was similarly operated on. The heart was more rounded, and the apex did not so perceptibly tilt forward. At first its actions were faint and irregular, apparently the result of shock, but on artificial respiration being had recourse to, they became strong, frequent, and regular. The same order was then observable in the succession of these actions, as were seen in the last experiment. After a time the contractions became slower, and then it was distinctly determined by himself, with the aid of the stethoscope, that two sounds existed,—the first and longer synchronous with the contraction, immediately succeeded by the short sharp sound, and this, in its turn, by the long pause which corresponded to the heart's period of repose.

From a review of these experiments, Dr B. considered that they confirmed the prevailing theory and were opposed to Dr Cartwright's opinions.

Dr Cartwright contended that the experiments above mentioned had been imperfect, owing to the use of chloroform; the contractions observed were merely spasmodic, and did not give a true notion of the regular rhythmic action of the heart; he maintained that more favourable experiments would show the correctness of his views, and he begged that further inquiry should be made before a conclusion was arrived at.

It was agreed that the experiments should be continued; the report to be given in at next meeting.

MEETING V.—January 14, 1854.—Professor BENNETT, President in the Chair.

1. *Dr Bennett* reported that another experiment (*Experiment 4*) on a dog, in reference to the action of the heart, had been performed since last meeting. The facts observed appeared to him to be identically the same with those

in Experiment 3, and served equally to confirm the usually received doctrines in regard to the heart's action.

Dr Cartwright was still of opinion that the normal action of the heart had not been well observed in these experiments, because in them the usual rhythm was so altered that the pause occupied the half of the duration of a beat, the systole and diastole occupying the other half; and the systole was slightly longer than the diastole: whereas in the normal beat, according to *Dr Cartwright*, the pause occupied only one-fifth, the systole and diastole, each two-fifths of the time. *Dr Cartwright* further observed, "though I considered the experiment a total failure, yet after the other gentlemen had satisfied themselves, I then listened, and during the time there occurred several so-called regular and strong beats, and I heard two sounds similar in character, certainly not the usual cardiac sounds, but produced by an impulse or shock communicated to the stethoscope during the diastole as well as during the systole; the sound was immediately under the instrument, and diffused. Once I heard a slight click like the second sound, but that appeared to be with the systole. I can now fancy whence arose the erroneous ideas, that the cardiac sounds were produced by muscular contraction or by a blow, ascribed to *Majendie*."

2. *Dr Cartwright* read the following paper in regard to the diastolic theory, particularly in reference to the opinions of *Hope*, *Williams*, and others:—

DIASTOLIC THEORY OF THE HEART'S ACTION.

I have, in the first place, to correct an error in my former paper, with reference to the fifth observation of *Hope* and *Williams*. Both in the 3d and 4th edition, *Dr Williams* uses merely the expression "tension or tightening," which apparently by a double translation into French, and again into English, becomes transformed into "tension and elongation," *vide* page 202, *Barth and Roger*, by *Newbigging*, 1842.

I regret much that I have not succeeded in showing the real action of the heart, in consequence of the dogs having been over-chloroformed; but it is a great satisfaction to myself, that I have witnessed in the third and fourth experiments, the action of the heart exactly as described by *Hope* and *Williams*.

It appears to me that *Dr Hope*, pages 10-13, 3d edition, 1839, commenced his experiments on larger animals, deeply biassed by preconceived convictions, and that his account of the duration of the motions is not exactly justified by his observations:—Thus

Page 20.—"The ventricular systole occupies half the time, or thereabouts, of a whole beat." At page 57, he quotes *Mr Bryan* as saying, the duration of the systole is a "third only;" but *Mr B.* says, in the note, "less than one-third."

"The ventricular diastole occupies one-fourth, or at most one-third." At page 19, "an instantaneous motion;" page 56, "an instantaneous expansive movement;" expressions quite incompatible with one-fourth or one-third of a whole beat.

"The interval of repose occupies one-fourth or rather less;" that is, the interval of repose occupies less time than an instantaneous motion.

All this looks very like squaring what was seen, to fit with previous auscultatory knowledge; but *Dr Williams*, page 292, 4th edition, gives a different duration, though not so nicely coinciding with auscultation: "systole, two-fifths; diastole, one-fifth; the period of natural rest and silence succeeds, occupying the remaining two-fifths." The expressions, interval of repose, period of natural rest and silence, denote that it must have occupied a considerable portion of the whole beat.

But let us test the durations by dividing a whole beat into fifteen parts:—

Systole,	4½ parts,	or less than one-third,	ascribed to Bryan.
Diastole,	3 —	or	one-fifth, " " Williams.
Repose,	7½ —	or	one-half.

Now, these durations coincide remarkably with what I saw in the third and fourth experiments; the interval of repose occupying fully half a beat, and the systole of somewhat longer duration than the diastole, which was an instantaneous motion.

Hope,	.	Systole, $2\frac{1}{2}$ fifths.	Diastole, $1\frac{1}{2}$.	Repose, 1.
Williams,	.	" 2	" 1	" 2.
As seen by myself,	.	" $1\frac{1}{2}$	" 1	" $2\frac{1}{2}$

Hope's statement agrees very charmingly with auscultation; Williams' very badly; and mine, not at all. But the order of succession of the motions, does not agree with what is observed in living animals.

Page 61. Objections by Hope to Majendie's alternate theory:—"The great defect of this view is, that it leaves no interval of repose. It is easy to see how M. Majendie has been misled, namely, by operating on *living* animals."

The Diastolic Theory, or the Action of the Heart, as it occurs in a Healthy Man:—

Diastole; the ventricle dilates easily, gradually, and with great force; the apex shooting out, becoming tense, and somewhat globular; the appendix of the auricle contracting just at the termination of the diastole.

Systole; the contraction of the ventricle commences at the apex,—the diastole passing, as it were, into the systole.

Pause; a tonic contraction.

Each motion is accompanied with a strong impulse or shock; the diastolic is directed towards the apex, the systolic towards the base.

Of the Sounds:—

The first sound is caused by the diastole sucking in the blood with great power through the auriculo-ventricular valve, through which in the living body the blood passes as through a vein in a stream, certainly not larger than the fore-finger.

The second sound is caused by the sudden and forcible passage of the blood through the semilunar valves.

Order of Succession and Duration:—

1. The diastole, terminating in the impulse, occupies rather more than 2-fifths, or 7 parts divided into 15ths.
2. The systole, rather less than 2 " " 5 " "
3. The pause, 1 " " 3 " "

The diastolic theory satisfactorily explains the pulse-move, its speed through the larger and smaller arteries, and the variations between the pulse and impulse; and it also proves, that the conclusions drawn from Volkman's hæmodromometer are erroneous.

Having now seen experiments similar to those of Hope and Williams, and now knowing on what fallacious grounds the systolic theory rests, I am, if possible, still more firmly convinced of the truth of the diastolic theory.

3. *Dr Cartwright* called the attention of the society to a case of stricture of the mitral valve, reported by Dr Markham in the last number of the "*Monthly Journal*;" it had a peculiar interest at the present moment, as an auscultatory test of the diastolic or systolic theory of the heart's impulse. At page 27, 2d case, a girl, æt. 17, Dr M. says, "there was a distinct *frémissement cataire*; a loud and prolonged murmur over the precordial region, loudest over the left side of the heart, and most concentrated at its apex, becoming weaker as the stethoscope was removed further from that spot, etc.; no bruit was heard along the aorta, and its sounds were audible. The rhythm of the heart's movements was entirely perverted; the movement, associated with the murmur, occupied

nearly the whole of the time of the systole, the diastole, and the interval ; the impulse,—that is, the first sound,—immediately followed the cessation of the murmur, or rather seemed to be its conclusion, and suddenly to wind it up ; it was brief and instantaneous, and, after a very short pause, followed by the recommencing murmur."

Dr Markham was led, from the character of the murmur, to conclude that it was diastolic ; the patient was examined by several other physicians, and on the pulse being reduced by digitalis to 40, there then remained on their minds no doubt that it was diastolic, its peculiar character conveying the idea of blood passing forcedly through a narrow aperture from the auricle into the ventricle.

Dr M. then tries to persuade himself, that this murmur was not the altered first sound, because it was not synchronous with the impulse, "which seemed to be its conclusion, and suddenly to wind it up ;" but the impulse is not literally synchronous with the first sound, but is its termination, *vide* Hope, 3d edition, page 40. And Dr Williams, in his separate publication, says, "I am disposed to admit that, in violent action of the organ, its more sudden and abrupt strokes against the chest *do* cause a sound, which constitutes the loud termination of the first sound ;" page 41, "but I may remark that I have for many years noticed the first sound to be *double* in some patients. The cause is, that the blow of the heart against the edge of the rib is a little later than the first sound." "The heart in gliding forwards and upwards during its systole, strikes with its apex against the *inferior margin* of the fifth rib, and thus creates an accidental sound, attended with cliquetis when the blow is smart." [But Skoda says, the heart glides down during the systole an inch even in an infant, and such is now the prevailing opinion in the profession ; that is, however, only one of the numerous inconsistencies among systolists, and they regard it probably as a mere trifle.]

Dr Cartwright then observed, that it was evident that the murmur was the altered first sound of the heart, and, therefore, Dr M. and his coadjutors were placed in an awkward predicament ; they must either "believe their own ears, and give up the systolic theory," or "disbelieve the evidences of their own senses, and believe in its conclusions drawn from wretched experiments on dead asses." For his part, he considered that Dr Markham, by a brilliant piece of stethoscopy, had brought forward valuable evidence in favour of the diastolic theory, which easily and satisfactorily explained the whole murmur, commencing with the second sound, and terminating with the impulse.

4. *Dr Sanders* read a paper presented by Dr Thomas Anderson, Resident Medical Officer of the Royal Infirmary, "On the effects of the internal administration of Belladonna in poisoning by opium." Drs Gairdner and Sanders were appointed a committee to report on the communication.

HYDATIDS.

5. *Mr Kirk* showed specimens of white, rounded, or oval bodies, varying from the size of a pea to that of a bean, and presenting the characters of some species of acephalocyst, probably the astoma. They were contained in the jelly-like (colloid) fluid evacuated from the abdomen in a case of ovarian disease. The same bodies were found in connection with the ovarian tumour on *post mortem* examination ; and they appeared to be attached to its peritoneal surface. The case had occurred under Dr Simpson's care in the Royal Infirmary.

Demonstrations of these bodies were made under the microscope.

A committee composed of Drs Bennett, Gairdner, Mr Kirk, and Mr Barlow, were appointed to examine into this subject, and report to a future meeting.

STRUCTURE OF TREE FERNS.

6. *Mr Kirk* exhibited specimens of the resinous bodies which are found in such great abundance in the cellular tissue of many tree ferns.

CONTAGIOUSNESS OF CHOLERA AT DUMFRIES.

To the Editor of the Monthly Journal of Medical Science.

SIR,—In an article in the *Medical Times and Gazette* for 24th December 1853, entitled "Medical Doings in the North," Dr W. T. Gairdner of Edinburgh is reported to have made, at the debate in the Medico-Chirurgical Society concerning the contagiousness of cholera, the following statements:—

"Even in some towns where a report had been given that no importation could be traced, more close investigation had discovered some channel of communication. Such was the case at Dumfries, as elicited by correspondence in the *Medical Times*."

The only correspondence on the subject of cholera which appeared in that Journal was that between Dr Barker and myself, the physicians to the infirmary; but in these letters the question of importation was never discussed by us. To quote the words of Dr Barker—"The isolation of the patients being the principal question at issue between Dr Grieve and I."

But further, in the January number of the *Monthly Journal*, Dr Gairdner is reported to have said—"In Dumfries, during the epidemic of 1832, all the medical men were opposed to the doctrine of contagion; and in one of the reports of 1848 it was stated, that medical men and nurses were not more subject to cholera than others; and that cholera cases were distributed with impunity among the other cases in the hospital. Now, the true state of matters was that great care was taken in the isolation of cholera patients in the general hospital, and that in 1832 no cholera cases were admitted. In 1848 cholera cases were admitted, and one nurse out of five died. Several of the nurses were affected with diarrhoea, which was quite general among the patients; one medical man out of seven died; and of twelve special medical men two were attacked, one of whom died."—P. 76.

Now, as Dr Gairdner's observations are all more or less fallacious, I think it my duty to correct the errors into which he has fallen, inasmuch as an accurate knowledge of the phenomena of cholera as it occurred in a town so severely visited by the epidemic, on both occasions, as Dumfries, may aid the profession in forming an opinion upon the much-disputed *questio vexata* of its contagious or non-contagious character. I shall, therefore, with your leave, notice *seriatim*, and very briefly, the observations made by Dr Gairdner on the occasion referred to.

1st.—As to the Importation of the Disease.

In my "Practical Observations on Epidemic Cholera" in the *Medical Times*, May 26th, 1849, No. 504, page 584, the following passage occurs:—"In 1832, the first cases occurred on the 15th September, and in 1848, on the 15th November, and it has been proved beyond a doubt that neither of these individuals had ever been in contact with infected persons, or been in any district where cholera prevailed; in fact, on both occasions, the nearest infected town was Edinburgh, with which none of the individuals first attacked, it has been satisfactorily ascertained, had any communication direct or indirect. The next well authenticated cases did not occur, as would naturally be supposed if contagious influences were in operation, in the same house or tenement, or in the same district or locality, or amongst the attendants and relatives, but in different parts of the town, with which there had been no personal intercourse or communication whatsoever."

The above statements my subsequent inquiries tend only to strengthen and confirm.

2d.—As to the Isolation of the Infirmary Patients.

No precautions were had recourse to beyond those always in force in respect to typhus and other pyrexial diseases, as is proved by the fact, that *all* the attendants not only waited upon cholera patients, but, at the same time, acted as nurses in the other departments of the house. The cholera patients were, like those having typhus, isolated only to a certain extent by being placed in

separate wards, and the restrictive measures were neither so complete nor so rigid as to afford any great security against cholera spreading among the other patients (supposing cholera to be contagious, which I believe it is not); indeed, our accommodation at that time was too limited, and our appliances too scanty, to allow of effective isolation in such an exigency as epidemic cholera.

3d.—*As to the Inmates and Officials of the Infirmary.*

During the cholera there were constantly seven attendants in the Infirmary whose services were general throughout the whole establishment; and during the course of the epidemic, 14 nurses in all were employed from first to last. Of these 14 nurses, *all of whom were more or less in attendance upon cholera patients, only one died, i. e.*, in the proportion of 1 in 14, not 1 in 5, as stated by Dr Gairdner. This fatal case occurred in a woman of dissipated habits—an important fact, when we take into account that 82 cases of cholera were treated in the Infirmary from 21st November 1848 to 12th January 1849. As regards diarrhoea among the inmates, I find on examining the Infirmary books of that year, that not a single case of diarrhoea occurred during the *cholera epidemic* either among *patients* or *nurses*, only *six* cases being recorded for the year, one of which happened *before*, the other *after* the visitation.

4th.—*As to the Opinions of the Medical Faculty of Dumfries.*

Dr Gairdner says that in 1832 “*all the medical men were opposed to the doctrine of contagion.*” Now, this statement is quite erroneous, for several of the medical men, as Drs Maxwell and Symons, both of them experienced physicians in the place, were contagionists; as also Dr Thorburn, who, at a meeting of the Faculty held on 18th October 1832, said ¹ “that as several of his most experienced and talented medical friends, who entertained similar, or at least nearly similar opinions to his own, were absent, he would content himself with offering a few cases and observations in illustration of his view of the subject.” Dr Gairdner also states that in 1832 no cases were admitted into the Infirmary. This is so far true, inasmuch as the governors of the institution passed a resolution not to admit cholera patients. But I may here mention that a patient who came from the country, after being three days in the Infirmary, was seized with cholera. This case was under my own care, as physician of the house; and it is worthy of record that this case proved *a solitary one*, although he was *not* isolated from the other patients.

In bringing these observations to a close, I have only to remark, that my only object has been to elicit the truth. I have had at my command every important and trustworthy source of information; and have availed myself of the opportunities thus afforded me, with a conscientious desire to ascertain and establish the real facts as they occurred.

What Dr Gairdner's sources of information are, I know not, nor have I, perhaps, any right to inquire; but, from whatever quarter he may have derived his intelligence, I can fearlessly challenge him to impugn or controvert the facts above narrated.

It is from the fact that any statements made by a physician so eminently distinguished for careful discrimination and minute investigation as Dr Gairdner, are entitled to respect, that I have considered it a duty I owe to the profession to correct the fallacies he has unwittingly propagated.—I am, Sir, yours, etc.,

J. GRIEVE, M.D.

To the Editor of the Monthly Journal.

SIR,—The remarks of Dr Grieve, which you have put into my hands, are founded partly upon what I *did* say at the meeting of the Medico-Chirurgical Society, and partly upon what I am *reported* to have said, but did not actually say. The difference between my statements and the report quoted by Dr Grieve is considerable; but I shall confine my rejoinder to such points as are calculated to forward his object and mine, the elucidation of the truth.

1st. As to the *importation* of the disease into Dumfries, I never had, and

¹ See Johnson's Medico-Chir. Journal, Vol. xviii., 1833.

never pretended to have, any information whatever. I can, therefore, have no objection to Dr Grieve's statement on this point.

2d. As to the *isolation* of the *Infirmaries* patients, my statement was, that the same precautions were taken against contagion in cholera as in typhus and other contagious diseases, the patients being placed in separate wards from the general cases. This is admitted by Dr Grieve, although he says that "no precautions were had recourse to *beyond those always in force*," etc.; that is, beyond those which have been found amply sufficient for the purpose in diseases of acknowledged contagious power.

3d. As to the *inmates* and *officials* of the *Infirmaries*.—My statements on this point were derived from the correspondence of Dr Grieve, Dr Barker, and Mr James Smyth in the "Medical Times," vol. xix. pp. 630, 647, 668; and vol. xx. p. 167; and from Dr Barker's report in the "Monthly Journal," vol. ix. p. 940. Dr Barker, who had the actual charge of all the cholera cases in the Infirmary, except six, says expressly that the nurses employed about the cholera patients were five in number, and that of these five one took the disease. Dr Grieve says that all the fourteen nurses of the institution were, *more or less*, in attendance on cholera cases. Supposing this to be as he states, I presume that Dr Barker's five were *more*, and the remaining nine were *less* so employed. And the account then stands thus: Of five who were *much* employed, one took cholera; of nine who were *little* employed, none were affected. Similar results were obtained in Edinburgh and Glasgow, and were there considered to bear in favour of contagion.

My authority for the occurrence of diarrhoea in the hospital was Mr Smyth, then the house surgeon. His statement may not be quite exact, being apparently founded merely on recollection; but Dr Grieve's answer is equally deficient in the elements of a precise statement, for no person will, I think, consider it improbable that in a period of pressure and work, diarrhoea might have occurred among the patients without being formally recorded in "the Infirmary books."

4th. As to the state of medical opinion in Dumfries, I am quite ready to defer to Dr Grieve, who, of course, knows the whole facts. In the Society I merely stated that there *was* in Dumfries a strong opinion against the contagious property of cholera; that Dr Grieve had been the advocate of that opinion; and that in his report to the "Monthly Journal" he had conveyed impressions which required qualification, as appeared from subsequent correspondence. I am sure that Dr Grieve will, on reading this explanation, acquit me of having spoken either lightly or unadvisedly. My object was simply to show the necessity of the evidence being thoroughly sifted in cases where, as in Dumfries, it was asserted that nothing rendering contagious communication probable had occurred. Such a position undoubtedly appears to me to be scarcely tenable, when it is shown that two out of twenty-one medical men, and one out of a very small staff of nurses, actually died of the epidemic, not to mention other cases of cholera and choleraic diarrhoea among those engaged in attendance upon the sick.—I am, etc.,

W. T. GAIRDNER.

MEDICAL REFORM AND THE NEW BILL.

What is medical reform? This question may be answered in various ways. To the philosopher it might mean the rendering medicine an exact science. The philanthropist might say, it is an attempt to put down the great evil of quackery, both outside and inside the profession. The officials of our colleges appear to think that it is the acquirement for their respective institutions of such privileges, as somehow or other will lead to their own profit. The busy practitioner, we are afraid, pays little attention to this matter, and what his idea of medical reform may be we do not know. But there are certain anomalies among the legal requirements of the recognised schools of this country, and some absurdities which the antiquated privileges of certain colleges render

evident, which, after years of agitation, we had imagined, were universally recognised, so that at length all parties had been brought to agree that a repeal of these was the *sine qua non* of medical reform. That in this great country a man who is a physician in Edinburgh, is not entitled to practise as such in London, or that a distinguished professor in Dublin cannot hope to teach clinically in London, without undergoing an elementary examination before local boards, must surely be considered absurd enough. This depends on the circumstance that the London College of Physicians holds sway over that city, and the narrow circle of seven miles round it, and no more, and that all hospital appointments, and all public positions, must in the first instance receive the stamp of its authority. Another absurdity is that every medical board prescribes its own curriculum, or the course which the student *must* follow to obtain its diploma. At the present moment we know a student in this University, who intends entering the army, and who, in consequence, is obliged to attend four six months' course of elementary anatomy, and this he is now carrying out in his fourth year, by deserting the clinical wards where he should study practical medicine, and avoiding the *post-mortem* examinations at which he should make himself familiar with morbid anatomy. Hence the wisdom of the army board is not satisfied unless evidence be given that those who treat soldiers, shall have heard a course of elementary anatomy four times over, although most other boards are satisfied with two such courses, and none other require more than three. The confusion that results in medical education is of course intense, as the student must regulate his classes in different schools, according to these rules, and frequently be made to sacrifice really important branches of knowledge, for useless repetitions of well-known subjects.

But the evils, vexations, and losses entailed on the student, the practitioner, and on the profession, in consequence of the prevailing system, require no illustration, the difficulty has hitherto been,—1st, How to arrive at such common remedial principles as will be universally received; and 2d, How to carry them out without so far entrenching upon vested rights, as would render their execution hazardous or unjust. Without then entering upon the broad questions of the improvement of medical science; of quackery; of corporate privileges; of university reform, and of parliamentary representation, all of which are important topics, it will suffice to say that the principles which must ever be kept constantly in view, as the basis of all medical reform, are—

1st. *Equality in Medical Education*.—Let the same curriculum for the medical student be rendered necessary in every college and school throughout the country, and as far as practicable let the same test of qualification be required for the respective grades of physician, surgeon, and general practitioner.

2d. *Equality in Medical Privilege*.—Let a qualified doctor, a surgeon, or a general practitioner in one part of her Majesty's dominions, be equally qualified over the whole breadth of the land. Let a member or a fellow of one royal college, should he change his place of practice, receive his corresponding grade in a similar college having jurisdiction over England, Ireland, or Scotland, without undergoing degrading and useless examinations,—but on the payment of such fair fees as may be agreed upon.

3d. *A Registration of every Legal Practitioner*.—The object of this is to separate the qualified from the non-qualified practitioner; and confine all public appointments and medical opinion in courts of law, to the former; and enable magistrates, in case of manslaughter or mal-practice, to deal summarily with the latter.

Let us limit our demands to these three practicable things, and a great boon will have been obtained; less than this will certainly be of no advantage to the profession, a proposition we had imagined to have been universally agreed to when the last new bill, brought out by the Provincial Association, convinced us of our mistake. In this document it is proposed, in the 22d clause, that—

“Every person who shall be registered, and shall possess a certificate in force, according to the provisions of this Act, shall be entitled to practise medicine

throughout that part of the United Kingdom for which his certificate was issued; and every person who shall be registered in one part of the United Kingdom may transfer his name to the register of any other part of the United Kingdom in which he may be about to practise, on production to the registrar of the last-named part of the United Kingdom of his certificate of approval, and certificate for the current year, and *in England and Ireland, shall also produce the diploma of the Royal College of Physicians, or the diploma of the Royal College of Surgeons, in England or Ireland, as the case may be*; and the registrar shall thereupon grant to such person transferring his name a certificate, which shall remain in force till the first day of February then next ensuing."

According to our reading of this clause, any Scotch practitioner, however eminent, before he can practise in England or Ireland, must produce the diploma of the Royal College of Physicians or of Surgeons in those parts of the kingdom; in other words, he must present himself for examination, and pay for another piece of parchment. This clause, therefore, is a direct violation of one of those principles absolutely essential in every bill for amending the state of the profession in this country, and tends to perpetuate the most odious part of the present anomalous system.

Other objections might be urged to this bill, but clause xxii. must be completely modified before it is worth our while to criticise any other of its details.

SANITARY AND TEMPERANCE MOVEMENT.

Every body knows that sanitary improvement and temperance have attracted much attention of late, and all must admit that they have taken a good hold on the public mind. We were never more impressed with the truth of this than at the recent annual dinner of the Royal College of Physicians, where instead of such songs as "The Glasses Sparkle on the Board," or "Willie brewed a peck o' maut," we heard the following ditty, which was received with much applause. The vocalist had adopted the air and measure of Moore's well known Irish melody, "Love's Young Dream," but managed to infuse into the music such a degree of lugubriousness, that at each repetition of the *refrain*, we experienced a sort of hydropathic chill, which, however, it is but justice to the song to say, was followed by a healthy reaction. As we are decidedly interested in the progress of Sanitary and Temperance Reform, we give the words, which we were enabled to note down with tolerable correctness:—

AIR.—"Love's Young Dream."

Oh! the days are gone when claret bright
 Inspired my strain,
 When I sang on every festive night
 About champagne.
 Prime "Thirty-four"
 In floods may pour,
 And glasses gaily clatter,
 But there's nothing half so safe to drink,
 As plain cold water.

Though the bard may make a greater noise
 Over his wine,
 When with other Bacchanalian boys
 He chances to dine;
 Yet if he wake
 With a headache,
 And wonder what's the matter,
 He learns there's nought so safe to drink,
 As plain cold water.

There's Dr Hassall,¹ he proclaims
 That water's full
 Of curious brutes, with curious names,
 In every pool.
 Now you will see,
 That this must be
 A most important matter,
 For it's clear there's meat, as well as drink,
 In plain cold water.

Professor Clark of Aberdeen,²
 Says chalk is there,
 And Monsieur Chatin, iodine³
 Finds every where.
 If this be true,
 It's clear to you,
 It's just so much the better,
 For there's meat and drink and physic too,
 In plain cold water.

So if your health you would keep good,
 With wine have done,
 And like that wise man,⁴ with your food,
 Drink water alone.
 About he drives,
 And well he thrives,
 And every day grows fatter,
 Which shows that folks can live quite well
 On plain cold water.

VARIETIES.

ROYAL INFIRMARY OF EDINBURGH.—The General Annual Meeting of the contributors was held on January 3d, in the Council Chambers. Of the number of patients admitted, it appeared that there remained in the hospital at 1st October 1852, 408; admitted from 1st October 1852 to 1st October 1853, 4262; total under treatment during the year, 4670; of these there were dismissed cured, 2599; relieved, 796; with advice, or at their desire, 173; as irregular or improper, 98; having received no benefit, 103; deaths, 512; patients remaining in the hospital at 1st October 1853, 389. Of the cases treated to a termination during the year, 574 were cases of fever; 2094 were ordinary medical cases other than fever; and 1613 were surgical cases. The average daily number of patients in the house throughout the year has been 434; the greatest number at any one time, 476; the lowest, 391; and the average time during which each patient remained under treatment, 37 days. In the immediately preceding year, the average daily number of patients was 409; the total number of cases treated, 4736; the greatest number at any one time, 453; the lowest, 371; and the average period which each patient remained under treatment, 34 days. The managers announced, as usual, a continued excess of expenditure over income; but the new Surgical Hospital expenses have formed for years a heavy drain upon them, which is fortunately now stopped.

¹ On the water supplied to the metropolis by Dr Hill Hassall.

² Patent process for removing the carbonate of lime from water for domestic use, by Thomas Clark, M.D., Professor of Chemistry, Marischal College, Aberdeen.

³ *Journal de Pharmacie*, tomes xvii., xviii.

⁴ Indicating a distinguished Teetotaller.

TESTIMONIAL TO DR TURNER OF KEITH.—On the 30th of last December plate to the value of one hundred guineas was presented to Dr Turner of Keith, as a mark of the esteem in which he is held by his friends in that and the adjoining parishes. We sincerely congratulate our respected contributor Dr Turner on this honourable testimony to his professional skill and character.

THE CHOLERA may be now considered to have nearly disappeared from this country and from France. In Paris there have been 451 deaths and 471 recoveries.

PROFESSOR TIEDEMANN.—On the 10th of March next this distinguished anatomist will have been a Professor for half a century. It is proposed to celebrate the event by distributing to his admirers, friends, and numerous pupils, bronze and silver medals, at a moderate cost (10s. and 20s.), bearing his likeness. These may be obtained by subscription, addressed to "Die Senckenberg naturforschende Gesellschaft, zu Frankfurt, a. m."

PROFESSOR CHELIUS.—The distinguished Professor of Surgery in the University of Heidelberg, has recently visited Paris, and been entertained at a splendid banquet given to him by the Surgical Society of Paris, presided over by Professor Denonvilliers. Those who, like ourselves, have studied in Heidelberg, and remember not only his skilful surgery, but the urbanity and kindness with which he always receives the English, will cordially congratulate him on the honours he has lately received in the French capital.

PUFFING, COUNTER-PUFFING, AND BICKERINGS OF THE LONDON HERDOMIDIARIES.—"The circulation of 'the Lancet' more than doubles that of any other Medical Journal published in Europe."—*Lancet*, Dec. 3d.

"The proprietors of the 'Medical Times and Gazette' believe that its circulation exceeds that of any other British Journal among members of the Profession. They can *prove*, that statements made of larger circulation by another Journal are not correct; they have no desire to increase their list of Subscribers by adding thereto cigar-makers, grocers, and pickle dealers."—*Medical Times and Gazette*, Dec. 10th.

"We can assure our readers that our circulation during the past year has been greater than either of our contemporaries, and we have a confident hope that, during the coming year 1854, it will far exceed the circulation of the 'Lancet' and 'Medical Times and Gazette' combined."—*Medical Circular*, Dec. 13th.

"Our reporters complain that their accounts of the discussions in the Epidemiological and Medical Societies are copied *verbatim*, without acknowledgment, in the 'Association Medical Journal,' and question whether such a proceeding is *honest*."—*Med. Times and Gazette*, Jan. 7th.

"We can afford to have our honesty impeached by our 'gratuitous adviser,' and his mythical staff—it being well known that the 'Association Journal's' reports are generally earlier, and always more accurate than those of the 'Medical Times.'"—*Association Journal*, Jan. 13th.

DISMISSAL OF MR GAY FROM THE LONDON ROYAL FREE HOSPITAL.—Mr Gay, who has acted as surgeon to the Royal Free Hospital of London, with great credit for seventeen years, has been dismissed by the Committee of Governors, because he furnished particulars of himself, and sat for his portrait, to be published in the Medical Circular. At the meeting Dr Peacock, in a manly speech, that does him great honour, observed, that "whatever the character of the periodical, or that of the memoir, a medical man should be proved guilty of some great moral delinquency, or want of medical skill, or culpable neglect, before he was dismissed. He could not help thinking, as a warm and sincere friend of the hospital, that if the committee confirmed their act, it would be a serious injury to the institution." Similar sentiments have been embodied in a series of resolutions, held at a public meeting of the profession, attended by upwards of a thousand gentlemen, and presided over by Dr Copland, in the Hanover Square Rooms, on the 18th ultimo. The affair has caused great excitement among the profession in London; and so discreditable has it

been considered by Drs Ballard and Cormack, and Mr Atkinson, that they have resigned their appointments in connection with the hospital and school.

IRISH COMPLACENCY.—The peculiar complacency with which our Dublin friends regard everything, simply because it is Irish, seems to be fostered very early among the medical students there. Thus the president of the Dublin Students' Society, in his introductory address, assures them that it "is the only one of any note in either England or Ireland." It is true, he says, in a foot-note to his printed address, "since the above was written, I have been informed that in Edinburgh a Students' Medical Society has existed for many years; and that through their own exertions the students have collected a valuable library, and support a medical reading room for their own use. A creditable example, well worthy of imitation."—(*Dublin Med. Press*). This patronizing notice refers to the Royal Medical Society of Edinburgh, which has been flourishing for the last 120 years, and numbers amongst its members most of the distinguished medical men in Europe!

PUBLICATIONS RECEIVED.

- Surgical Anatomy.** By Joseph Maclise, F.R.C.S. Second Edition. Fasciculus III. Folio.
- The Science and Art of Surgery, being a Treatise on Surgical Injuries, Diseases, and Operations.** By John Erichsen, Professor of Surgery in University College, etc. London, 1853. 8vo. Pp. 951.
- The London and Provincial Medical Directory, 1854.** London. Small 8vo. Pp. 833.
- The Diseases of the Rectum.** By Richard Quain, F.R.S., etc. London, 1854. Small 8vo. Pp. 178.
- Medico-Legal Observations upon Infantile Leucorrhœa, arising out of the alleged cases of felonious assaults on young children, recently tried in Dublin.** By W. R. Wilde, F.R.C.S., Surgeon to St Mark's Hospital, etc. London, 1854. 12mo. Pp. 40.
- Unsoundness of Mind considered in relation to the question of Responsibility for Criminal Acts.** By Samuel Knaggs, Member of the Royal College of Surgeons, etc. London, 1854. 8vo. Pp. 96.
- Remarks on the Examination of Recruits, intended for the use of young medical officers on entering the army.** By H. H. Massy, A.B., M.B. 4th Light Dragoons. London, 1854. 8vo. Pp. 131.
- The Odontalgist: or, How to Preserve the Teeth, Cure Toothache, and regulate Dentition from Infancy to Age.** By J. Paterson Clarke, M.A., etc. London, 1854. Small 8vo. Pp. 184.
- Guy's Hospital Reports. Second Series.** Edited by E. L. Birkett, M.D., and Alfred Poland. Vol. VIII. Part II. London, 1853. 8vo.
- Report for the Medical Relief Committee of the Parochial Board of the City of Edinburgh, on the state of the Idiotic and Imbecile in the Eastern Division of the Poor House.** By John Smith, M.D., F.R.C.P.E. Edinburgh, 1853. 8vo. Pp. 10.
- Twelfth Annual Report of the Edinburgh Night Asylum for the Houseless, Old Fishmarket Close.** Edinburgh, 1853. 8vo. Pp. 20.
- Introductory Address to the Students of the Extra-Academical School of Medicine, Edinburgh. Session 1853-4.** By R. J. Mackenzie, M.D., F.R.C.S.E., etc. Edinburgh, 1853. 8vo. Pp. 17.
- On the British Tritons.** By John Higginbotham, F.R.S. London, 1853. 8vo. Pp. 16.
- Practical Observations on the History, Nature, and Treatment of Cholera Asphyxia.** By John Coghlan, M.D., Wexford; M.R.C.S.E., etc. Dublin, 1853. 8vo. Pp. 25.
- Traité élémentaire des Maladies de la Peau, par Maurice Chausit, Docteur en Médecine, etc., d'après l'enseignement théorique et les leçons Cliniques de M. le Docteur A. Cazenave.** Paris, 1853. 8vo. Pp. 448.
- The Retrospect of Medicine: being a half-yearly Journal, etc.** Edited by W. Braithwaite, Lecturer on Obstetric Medicine, Leeds. London, 1854. Small 8vo. Pp. 476.
- Homœopathy: its Globules (Bubbles) Analyzed. Second Edition.** By W. J. Cox, M.R.C.S., M.B., etc. London, 1853. Small 8vo. Pp. 84.
- Some Remarks on the Medical Evidence given at the trial of William Bourke Kirwan for the murder of his wife.** By John Field, M.D. With a Commentary thereon. By Frederic C. Skey, F.R.S. London, 1853. 8vo. Pp. 15.
- The Common Sense of Cholera.** By a Practical Practitioner. London, 1854. 12mo. Pp. 68.
- The Scottish Review. A Quarterly Journal of Social Progress, and General Literature.** January 1854.
- The Abstainer's Journal.** Edited by the Rev. William Reid. No. XIII. January, 1854.
- The Scottish Temperance League Register and Abstainer's Almanac.** 1854.

The review of Mr Paget's volume on Tumours will be given next month, together with that of the recent work of M. Velpeau.

The papers of Dr Reeves and Dr T. K. Chambers will be inserted next month.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Presence of Fat in the Excretions.* By C. E. REEVES, M.D., London.

THE presence of fat in the excretions is a subject of considerable interest both to the physiologist and to the pathologist. The opinion advanced by Liebig, that sugar and starch, in the process of digestion, was converted into fat, denied by Dumas and Boussingault, but confirmed by Pelouze and Gellis;—by the discovery that butyric acid could be formed by the fermentation of sugar, and strengthened by the experiments of Grundlach and Edwards on bees;—the conversion of starch into sugar in diabetes, and the influence exercised over the discharge of fat by a vegetable diet;—all tend to prove the truth of the theory of the great German chemist.

Fat forms one of the constituents of the serum, urine, bile, and fæces. Winslow notices in a cursory manner the existence of oil in the blood early in the last century, but Hewson first obtained it some years later by drying the serum of the blood and then pressing it between folds of paper, which became greasy. In 1821 Dr Traill published a paper in the *Edinburgh Medical and Surgical Journal* on its presence in the serum of blood drawn from a person labouring under internal inflammation—it was milky, and when dried gave a greasy stain to paper, and left greasy marks in the vessel in which it had been evaporated. In the same year he examined a sample which yielded 2·44 per cent. of oil; and two years later he examined another sample which yielded 4·5.

In 1829, Dr Christison, in a paper in the same Journal, observed it to be present in the serum of the blood of persons suffering from renal dropsy, and he obtained it by means of æther. In 1830, Dr Babington read a paper before the Medical Chirurgical Society on the same subject. Dr Adams (*Transactions of Medical and Physical Society of Calcutta*) mentions the case of a man who went to bed drunk, and was found dead in the morning. The vessels of the brain were greatly distended with blood, and oil was seen floating in it. In one of the volumes of Schmidt's *Jahrbücher*, an instance is reported where the blood contained as much as 8 per cent. of oil. Rayer (*Maladies des Reins*, tome i.) speaks of the case of a man, who, after drinking largely of punch one night, set fire to some charcoal, and laid himself down on the bed. He was brought to La Charité the next day, where he expired in the course of a few hours. The blood was observed to contain globules of oil, and the urine presented the same appearance. Serules at Strasbourg had observed the same thing. Gibson (*Diseases of Horses*) remarks that

when fat horses are over-driven, the blood, urine, and fœces will contain oil. The blood, and other secretions of fat Europeans, on going to a hot climate, will be found to contain oil, from the fat becoming re-absorbed (*Art. Graisse, Dict. des Sc. Médicales*).

Sauvages remarks (*Nosologia Methodica*) *Diarrhœa adiposa*—*Ex synocha ab equitatione violenta funditur adeps, et partim in venas resorbetur uti copiosus cum sanguine mixtus ipsi viscositatem magnam concilians*—partim in intestinorum cavum defluit, et fœces quasi butyro fuso involit; differt à colliquativa quod sit sine hectica.

Fat in the Urine.—The labours of Christison, Rayer, and Lehmann, have fully established the existence of fat, in an abnormal quantity, in Bright's disease, and the same state occurs in diabetes, but not in such an amount as to render its existence evident without the aid of the microscope.

Dr Prout several times met with cases where fatty matter was passed; and M. Beauvais (cited by Rayer) has also met with instances. Bourchardat (cited by Simon) found as much as 2.5 per cent. in the urine of a woman.

Dr Golding Bird (Urinary Deposits) met with the case of a female, æt. 35, fat and flabby, who had passed it in her urine for several years, particularly during her pregnancies. It was very capricious, sometimes lasting for weeks and then disappearing. Since she had commenced to get fat it had been more frequent. Dr Bence Jones has published two cases in the *Medico-Chir. Transact.* for 1850. The first case, that of a man, is a master-piece of careful observation.

In the case which Dr Elliotson has cited from Tulpus, fat was discharged from the bladder and from the intestines; and the same occurred in a case which fell under his own notice. In his *Practice of Physic*, he mentions the case of a lady with whom he was acquainted, but whom he did not attend, who passed it in her urine.

Schmidt (cited by Rayer) mentions the case of a man who suffered from pain in the loins, with night emissions; the urine was fatty for years.

Fourcroy notices the existence of fat in the urine in jaundice, and Naumann (*Handbuch der Klinik art. Gelbsucht*) mentions a case. Canstatt (*Handbuch der Med. Klinik*) makes the same remark.

Oil, when taken by the mouth, has been passed with the urine. Fernel mentions an instance; and Bechetoni reports the case of a young lady, suffering from nervous fever, accompanied by retention of urine. When drawn off, it contained some oil of almonds, which had been taken some days before.

Mettenheimer (Vogel, Nasse, and Beneke, *Archiv. des Vereins für Gemeinschaft, Arbeit. Für Fortdenung der Wissentschaft. Heilkund.*—1 Band, 3 Heft, 1853), mentions two cases of fatty urine—one that of a man æt. 64, who died from cancer of the lungs. The oil appeared three weeks after he had taken cod-liver oil. The second case, that of a man æt. 55, to whom an emulsion had been

given, when convalescent from an attack of slight inflammation of the kidneys.

Fat in the Fæces.—Berzelius first drew attention to its existence in the fæces. Percy found 11·95 per cent. in the fæces of a person living on ordinary diet. Dr Davy found the meconium of still and new-born children to contain margarine and olein, with a small amount of cholesterine. The fæces of a child, six weeks old, nourished by its mother's milk, Simon found to contain, in 100·00 parts 52·00 fat, 16·00 bile pigment with fat, 18·00 coagulated caseine with mucus, 14·00 moisture and loss. In diabetes the fæces contain as much as 34 per cent. of fat; and a diabetic person, who took daily 8 oz. of dry gluten bread, 11 oz. 5 dr. dry meat, 2 oz. dry eggs, and 2 oz. cod-liver oil, discharged 2 oz. of fat in twenty-four hours.¹ In two cases, one occurring to Dr Bright, and the other to Dr Elliotson, diabetes co-existed with the fatty discharge. In the last gentleman's case, the fat was observed twelve months before the diabetes appeared. In Mr Marston's case diabetes also existed, but it disappeared before death.

Fatty matter may appear from an excess being taken into the system, as in the case where castor or olive oil has been taken—hence, where the last-named article enters largely into the diet, as is the case in the south of France, Spain, and Italy, fatty concretions are of frequent occurrence. The large quantities of cod-liver oil taken in phthisis, would almost lead us to suppose that oily matter would be frequently met with in the fæces, but the great quantity of heat-producing material demanded prevents this; and although I have examined the evacuations of a considerable number of patients suffering from this disease, yet I have never perceived it. After taking the oil for a short time, the motions acquire a more greasy aspect, and this was particularly evident in two cases, where pneumonia was lighted up, and where, from some misunderstanding the patients went on taking the oil, each dose being followed by an increase of the inflammatory symptoms. Morgagni (*De Sed et Caus. Morbi Epist.* 21, *Art.* 26 and 44) mentions two instances where the exhibition of almond oil in pneumonia was attended by great aggravation of the symptoms.

The power of the intestines in taking up fat has been investigated by Boussingault and Lentz. Both found that the amount absorbed was the same, whether the quantity introduced was small or large.

Lentz (*De Adipis Concoct et Absorbitione*) further found that the prevention of the entrance of the bile or pancreatic juice, either singly or both, into the intestines, did not influence the quantity absorbed; thus refuting the opinion of Berard, founded on the cases reported by Dr Bright, that the presence of fat in the fæces was due to the prevention of the entrance of the pancreatic juice into the intestines. Dr Bright was at first disposed to consider disease of

¹ Simon's Animal Chemistry, translated by Dr Day.

the pancreas as a cause of its existence, but meeting subsequently with several cases of disease of this gland where no fat was observed, he admits that he drew his deductions from too few observations. I have met with several cases of chronic inflammation, obstruction of the ducts, and cancer of this organ, where no fat was observed, and its absence was always compensated for by an increased secretion from the other salivary glands.

Fat, as a disease, must, I think, be looked upon as resulting from some change either in the bile itself or from the liver secreting it from the portal vein, the blood of which must contain an unusual quantity of fat, from its being reabsorbed,—and from the system not being in a state to re-receive it, instead of being taken up in the intestines by the columnar epithelium, already no doubt loaded with oil, it appears in the fœces. In one of the cases which fell under Dr Elliotson's notice the intestines were as if bathed in oil.

As a proof of its being probably due to the first cause, I may cite the following instances:—Chevalier found the bile of a man, who died from scirrhus pancreas, to contain yellow semi-crystalline fat. Bizio found the same in some thick dark bile. Mérat also found it in the bile of a man in whose fœces it had existed during life. During my student-ship the body of a female, highly jaundiced, was brought into the *École Pratique*; the gall bladder and the ducts were distended with thick dark bile, and in it fatty masses were floating. The duodenum was inflamed, and the opening of the duct was obstructed. Portal met with masses in the livers of both a male and female who had passed fat during life. Dr Ogle (*Transact. of Pathological Soc. of London*) found in the body of a patient who had died from disease of the kidneys, and an obscure form of pneumonia, masses in the liver equal in diameter to 6d., of a light yellow colour, and in considerable number. The liver itself was in the first stage of cirrhosis. Under the microscope these concretions were found to consist of oily molecules, with a white amorphous blastema, and nucleolar corpuscles. The kidneys contained similar deposits. Mr Lloyd found the common duct to contain a brownish-yellow fluid, like that passed during life. Further, every case of fatty discharge has presented some hepatic disturbance, generally jaundice, and when the opening of the duct into the duodenum became closed, the fat has ceased to appear.

The fat may be passed either in masses, as a buttery fluid, becoming more or less hard by exposure, or as an oil. The two last occur together, but the buttery fluid is the most frequent; but they are always distinct from the first, which bears a close resemblance to tallow, and is met with in masses, varying in size from a pin's head to an almond. Its discharge, unlike that of the buttery form, is never constant, and is never fatal, the death in the case of Madame Bassompierre took place from disease of the brain, who suffered from it each time the liver and hepatic ducts became congested, its discharge being preceded by jaundice, induced either by irregularities in diet or mental emotion, and was only relieved by copious evacuations, in which the masses were seen floating.

CASE I.—A lady, æt. 55, very stout, of sedentary habits, and a free liver, bowels irregular. She is the subject of occasional hernial protrusion near the umbilicus, but the exact spot, from the thickness of the fat on the abdominal wall, cannot be defined, but it gives her but little pain, unless left unreduced for some time. She is also the subject of prurigo pudenda, and for years she has suffered from colic, from the passing of gall stones. These attacks are invariably preceded by constipation, tension, and an uneasy sensation in the right side, and her eyes and skin assume a slight yellow tinge. The attack yields in three or four days to the use of brisk purgatives. No gall stones have ever been discovered. The motions, on being examined, presented a number of yellowish bodies floating in it, varying from a pea to a small grape. She derived marked benefit from a vegetable diet and more exercise.

CASE II.—For years, says Howship,¹ an elderly lady had suffered from bilious attacks, attended by severe pain in the hepatic region, with thirst, high-coloured urine, and jaundiced tinge of the skin. The attacks would be violent for some days, and then decline, a large quantity of fatty matter, in the form of small masses, sometimes from aperient medicines, being passed. In 1809 the attacks returned; gall stones being suspected, an emetic was given, without benefit. Dr Simpson, of New Maldon, advised the exhibition of a large dose of olive oil, as he had seen benefit result. It was had recourse to, and induced copious evacuations, in which fatty masses were seen varying in size, the largest being equal to a grape, with relief.

Dr Babington met with a similar case (Home in Phil. Transactions, 1813) in a lady who had been for several years subject to an affection which was considered to be gall stones. She had recourse to olive oil in doses of two or three ounces, followed by the discharge of fatty masses, varying in size from a pea to a grape.

A female, says Cappezzuoli (Gazetta Toscana della Scien. Med., 1848), who was supposed to be suffering under disease of the liver, passed fatty masses of the size of almonds, of a greenish hue.

A friend of mine, says Dr De la Motte, often passed fatty concretions like biliary calculi; they were attended by a sensation of weight in the right hypochondrium.

Hufeland mentions having seen them passed by a person who suffered much from constipation.

A young lady,² highly hysterical, took some purgative medicine to remove an enormous distension of the abdomen; fatty masses like blanched almonds were passed with the motion; they continued a week and then ceased.

¹ Observations in Surgery and Morbid Anatomy. This case is cited by Mr Sampson, *vide* his Homœopathy, as an instance of the truth of the law of similars curing similars. Where the similarity exists, I am at a loss to know when the same results would have followed a dose of salts or any other purgative. But words are useless. Sancho Panza says:—Xabonar cabeza de asno, perdimiento de xabon.

² Dr Turner, cited by Dr Elliotson.

Hildanus¹ mentions the case of a female, 50 years of age, who suffered some years from severe pain in the stomach, which extended over the whole abdomen. After discharging a large quantity of fat, in all amounting to 3 lbs., she recovered. It was shown to him; it was white and pure, in detached pieces, and had been passed unmixed with fœces.

Mareschal (Mem. de la Acad. de Chir. tome iii. 55) mentions the case of a woman, who for fifteen years had been the subject of bilious colic, and for ten to severe constipation. He felt a body high up in the rectum, which was extracted, and found to consist of a large concretion, the outer layers of which were formed of a substance like fat, and which, when thrown into the fire, burnt.

A man, says Arnot (Edin. Med. Essays, vol. vi.), injured his back in attempting to lift a heavy weight, and immediately lost all power over his legs. At the end of three months he had so far recovered power over them as to be able to get about. He then, for the first time, observed in his motions a white substance of the size of a walnut, and for several days after he continued to pass pieces of the size of kidney beans or peas.

Mérat cites the case of a girl from the "Journal de Médecine" of the Abbe Laroque, 1683, who passed fatty concretions. I have examined this work. The account is sent by Dr Sigismond of Berne to Mr Hook, but the worthy Swiss, the chief magistrate and others, had evidently been imposed upon.

Stalpart Van der Wiel (Obs. Rarior Med. Anat. Chir., cent i. obs. 61) mentions having been called with his brother to a female, 50 years of age, who had been suffering some days from a flux, with which she had passed a great number of green fatty masses of the size of nutmegs, and which, when thrown into the fire, burnt vividly.

A servant girl (Scott in Duncan's *Med. Comment.*, vol. iv.) from 24 to 25 years old, after working hard at reaping in very hot weather, was taken with pain in the abdomen, loathing of food, obstructed menstrual discharge, and colic. Various means were tried; at last, after the use of rhubarb and calomel purges, fatty masses were passed by stool in considerable quantity—in pieces of the size of nuts, beans, peas, etc., and bore a strong resemblance to tallow.

A case is cited in Schmidt's *Jahrbücher* for 1836, which occurred to Dr Schneider, and which bears word for word a like resemblance to the above.

Fat passed in the form of an oily or buttery fluid.—In the *Miscel. Med. Phys. Germanica*, decen. 2, an. 2, obs. 152, is reported the case of an infant who passed a butter-like substance.

Dr Babington (cited by Home) met with the case of a child who, six months after birth, became subject to jaundice. She was seen when eighteen months old, with tumid abdomen and great weakness in the back. At three years of age the mother observed yellow fat-

¹ Dr Turner, cited by Dr Elliotson.

like oil run from her as she walked across the room. The discharge existed when she was four and a half years of age, and occurred every ten or fourteen days, in quantities of from one to three ounces. Her health was good, her flesh firm, and urine natural.

Tulpius (cited by Dr Elliotson) mentions an instance where it was passed daily in large quantities for fourteen months. The person was a female, often indisposed from tertian ague or obstructed spleen. It was unattended by either pain or emaciation (Dietrich, obs. quodam Rarior) where waxy mucous substance was passed. Paulini mentions a similar instance.¹

Tulpius² mentions the case of an old woman of 70 who passed fat from the bowels and bladder. She had neither fever nor emaciation until towards the close of life, when they manifested themselves, and death found her a dried up and juiceless corpse.

Dr Elliotson mentions a similar instance which occurred to Mr Pearson. A female, æt. 79, long the subject of gall stones, had had dull pain in the region of the liver for some months, and her motions had been free from fœcal odour; urine pale, but natural in quantity. She recovered from this attack, and a month after fatty oily matter appeared, both in the motions and in the urine. She sank. No post-mortem was made.

Moellenbrocus³ relates the case of a man who discharged fat for two years. He grew thin and weak, and died.

Moebus⁴ mentions the case of a woman who discharged fat daily from the bowels. It was attended by wasting away of her body.

The existence of fat in a fluid state is not always fatal, as the following cases show :—

CASE I.—A medical man, æt. 40, of moderate stature, brown hair and eyes, and of irritable temper, subject to much mental anxiety. Skin rather harsh, and frequently the subject of psoriasis. He suffers much from attacks of bilious headache, which have troubled him for years, coming on at uncertain intervals, often excited by the slightest irregularity in diet, and greatly influenced by the state of his mind. He constantly suffers more or less from indigestion, and with distension and tenderness along the duodenum. Bowels constantly constipated, never acting without purgatives, and the motions obtained by them generally scanty, and often light coloured. This is particularly the case if more than half a grain of calomel be taken for a dose. Urine generally scanty, loaded with urates. When labouring under an attack of headache, the tenderness and distension in the right side of the epigastrium is greatly increased, and relief is only obtained by the evacuation of thick dark bile, which is often seen in streaks in the motion. The liver is rather small, and even during the attacks of headache it is not sensibly increased in size, although it becomes rather tender to the touch; the gall-bladder can be felt distinctly distended, although not very pro-

¹ Cited by Mason Good.

² Cited by Dr Elliotson in *Medico-Chir. Transactions*, 1833.

³ *Ibid.*

⁴ *Ibid.*

minent, and pressure on it during the headache often causes bile to be poured into the intestines. During two and a half years which I had an opportunity of watching his case, fat appeared twice only in the form of a dark, yellowish, buttery fluid—the quantity passed each time amounted to a dessert spoonful, and at the close of the attacks of headache, and when they had been of longer duration than usual. His habits were very regular, never taking more than two glasses of wine a day; but he took no active exercise.

CASE II.—A man¹ of extreme sensibility, although of strong constitution, became, after great mental anxiety, unable to take either wine, coffee, or any kind of stimulants, without becoming greatly agitated and suffering from loss of sleep. He was seen by me in June 1773. His aspect was yellow, particularly the conjunctivæ; liver prominent, particularly in the epigastric region, and he had pain there, with bitter taste in the mouth. The abdomen nearly always swollen, which often commenced suddenly, and then as suddenly subsided. He had at times evacuations of an oily fluid, and when this took place he felt himself better, both morally and physically. By using relaxing drinks, baths, vegetable diet, with but very little meat, and horse exercise, he got gradually well.

A case is quoted by Lussanna in the *Giornale Veneto di Sc. Med.* ii. 766, and a case is given which occurred to himself. In both cases the individuals recovered.—(Note appended by the editor of the *Monthly Medical Journal* to Mr Marston's case.)

Vomiting of Fat.—A preacher, æt. 50 (Kirby Phil. Trans. 1673), from severe study and exertion, was seized with vomiting and purging. He got well by rest and medicine, returned to his duties, but soon had a relapse, and vomited up masses of a tallow-like substance as large as the end of a man's thumb.

Morgagni mentions the case of a lady, who vomited up a soft whitish mass, which became hard on cooling.

A man, æt. 70, says Dr Pasquali (*Annali Univ. di Med.* 1826), who had suffered from an attack of jaundice, became, during the last two years, subject to attacks of vomiting. After an excess he vomited largely. When the paroxysm seemed to all appearance over, he began to bring up pure blood and a thick kind of oily matter. In twenty-four hours he brought up a large quantity of this mixture, and his skin hung in large folds. It then stopped, and he soon recovered his former health.

Simon Schultz, in Bonneti's *Medicina Septen. Collatitia de Infimo Ventric.*, sect. v. cap. 26, mentions the case of a person suffering from severe dyspnœa, etc., who was taken in the night with severe burning in the stomach, accompanied with nausea, which was followed by vomiting, with immediate relief, of a bitter soapy fluid, of a buttery odour, the mouth and fauces being clogged with greasy matter. On examination of the vomited fluid, it was found to be like liquid butter, which, on exposure to the air, congealed into

¹ Portal *Maladies du Foie*, 426.

a mass like butter. It amounted in quantity to 4 oz., and was surrounded by a small quantity of offensive serosity. Marc. Donatus (Hist. Med. Mirabil.) speaks of a similar instance, where a buttery-like oil was ejected by vomiting.

Dr Nasse (cited in Schmidt's Jahrbücher, 1844) examined a greenish bitter fluid, with yellowish-white masses in it, which had been vomited up by a female. The vomiting alternated, often on the same day, with pyrosis; both were accompanied with severe cramps in the abdomen. The lumps lay together, held by slimy mucous. In ether they were nearly all dissolved, leaving a little fibrous substance, which appeared by examination to be some protein compound.

Summary.—In reviewing the fatal cases in the appended table, sixteen in number, ten occurred to males and six to females. In regard to age, in the first, four cases occurred between 30 and 40 years; four between 40 and 50; and two between 50 and 55 years. In the last, one was 31; one 48; one 50; and the other 57 years of age. In one case the female's age was not mentioned, and in another æt. 60. The fat was met with only occasionally, and in the form of masses. In one of the cases which occurred to one of the males, the fat was also discharged in masses, and although he died from general dropsy, consequent on disease of the heart, yet the discharge of fat seems to have hastened the fatal termination.

Post-mortem appearances.—The liver was enlarged in eight out of the 16 cases; in six of this number the gall-bladder and ducts were distended with thick dark bile; in the remaining two, in one the right lobe was hardened, and the gall-bladder and canals contained concrete bile with fatty masses; in another it was pale and soft, its canals and gall-bladder being empty. In the remaining cases, in one the liver was small and hard; it contained concretions; its canals and gall-bladder were empty. In another case it was small and pale; its canals and gall-bladder empty; in two cases it was quite healthy; the canals empty; in one the gall-bladder was distended with thick bile. In two cases the liver was cancerous.

The Pancreas and Ducts.—In five cases no lesion of the pancreas or its ducts was observed. In two the gland was healthy, but their ducts contained calculi. In two of the cases it was generally hardened, their ducts containing calculi; in one of these the choloidic duct was wide above, but narrow below, from pressure of head of pancreas. In one case it was fatty, its ducts containing calculi. In four cases the gland was cancerous; one contained two tumours—one in centre, the other on its right head—in the other three the right head was affected, the rest of gland being atrophied in one instance; in the other two the liver was cancerous—in one of these the opening of common duct small, in the other the pancreatic duct was obliterated. In one case the right head of the gland was enlarged, the opening of the common duct closed. In another the right head was converted into a cyst, both the pancreatic and choloidic duct being obliterated.

Sex.	Age.	Habits and Occupation.	Causes and Symptoms.	Post-mortem Appearances.
Male.	44 ¹	Free liver.	By exertion injured back two years before, then became subject to palpitation of heart, and cedema of the lower extremities; relieved by treatment; then relapsed; fatty matter appeared after a purge of jalap and senna; later, taking some punch excited slight jaundice; an emetic excited copious purging, with much fatty matter, and the same followed the use of purgatives; the abdomen began to swell; ascites had before existed, but had yielded to treatment; fluid appeared also in the chest; he sank, having lost flesh rapidly during last two months.	Liver large—gall-bladder and ducts distended with thick bile, which contained masses of fat like those passed during life; Glisson's capsule was converted into a lardaceous mass; heart very large; the abdominal and pleural cavities contained fluid.
Male.	55 ²	A marquis.	After great mental anxiety, followed by jaundice, which was neglected; liver large and tender; cedema of the extremities; emaciation; improved; relapse, the cedema increasing accompanied by ascites; he sank.	Liver large, lesser lobe hard; concrete bile in the gall-bladder and hepatic canals, in the last fatty concretions varying in size from a pin's head to a pea.
Male.	52 ³	Corn-dealer.	After severe mental anxiety from imprisonment in the Bastille, digestion became disordered; lost flesh; pains in the abdomen four or five hours after a meal, increasing in severity, and referable to third or fourth cartilage of right false ribs; here a swelling, more apparent sometimes than at others; the pain and the flatulence which accompanied it, and the swelling, would often suddenly cease, and in a short time a substance like the yellow of egg, but more fluid, would pass; at times it would be green and more or less concrete, like oil congealed by cold, in which concretions of the size of shot would be observed; the quantity of the matter discharged corresponded to the amount of pain and distension; various measures tried in vain; he became jaundiced; liver felt large; gall-bladder the same, and before the colic and	Liver very large, in some places soft, in others hard; gall-bladder and canals empty; fluid in the pleural and abdominal cavities; spleen much smaller than felt during life.

evacuations came on it increased considerably; dropsey set in and he sank about one and a half year from commencement.

Female. 60⁺ Madame de Bassompierre.

Extremely sensitive; jaundice after a fit of anxiety, with attacks of pain in the right hypochondrium; pulse full, and had great agitation, and at times drowsiness, with laboured respiration on their accession; at last apoplexy occurred; recovered by evacuations, but paralysis of the right half of the body remained for more than a month; she had been for some time the subject of colic and jaundice on the occurrence of any mental anxiety; these attacks were always relieved by the copious evacuation of bilious stools, which would last some days, and containing a large quantity of white concretions; the paralysis had scarcely subsided before the colic attacks returned but less severe; they were preceded by constipation, gradual and increasing distension of the epigastrium, some mental or dietary disturbance exciting the attack; opiates or antispasmodics given to relieve the pain would excite convulsive agitation of the whole limbs; she had long been subject to agitation of the head when up, and of the limbs when lying down, the anxiety produced by loss of her fortune induced coma, and she sank, having been under my care twelve years.

She was labouring under phthisis; her liver felt large and was painful on pressure; urine scanty and pale; bowels much relaxed, their evacuation being preceded by much pain; motions pale and almost destitute of odour; the fatty

Liver very large, natural, but pale; no bile in ducts or gall-bladder; tubercles in lungs.

¹ Mém. Jour. de Corvisart. Leroux et Boyer. Tome xi. 387.

² Case of Mons. Sorin, Portal 419.

³ Case occurring to Mr Pearson, and seen by Dr Elliottson.

⁴ Portal—Maladies du Foie. Paris, 1813.

⁵ Portal—Maladies du Foie, 423.

Sex.	Age.	Habits and Occupation.	Causes and Symptoms.	Post-mortem Appearances.
Female.	48 ¹	Irregular.	<p>matter passed was 2½ oz. in the course of the day, with ½d of an oz. of oil; but the quantity of the last varied; for some months she had vomited several times daily.</p> <p>Ill some time, from disease of liver, jaundice, constant pain in the right hypochondrium and epigastrium, and at times in bowels; emaciation; motions deficient in bile, and contained a substance like tallow; death.</p>	<p>Liver large, pale, and soft; gall-bladder containing a little urine, like bile; pancreas hard, and its duct containing a number of calculi; intestines contained a substance like rice gruel, which, in the rectum, was like blanch-mange; fluid in the abdominal and pleural cavities.</p>
Male.	45 ²	—	<p>Suffering from diabetes eighteen months, and phthisis of short duration when he entered the hospital. A little blood from arm presented a buffy coat. Soon after admission, severe pain in the back and abdomen, with diarrhoea; his motions often rather pale, contained a substance like concrete oil, which continued up to time of death, varying in quantity; it had been observed in motions one year before the diabetes set in, and then lasted six months; death. Disease attributed to grief.</p>	<p>Liver healthy; gall-bladder distended with dark bile; the pancreatic duct and its branches distended with calculi; the intestines as if bathed in oil.</p>
Female.	57 ³	—	<p>Several years the subject of disorder of liver, with attacks of gall stones.—In September 1850 observed greasy yellow masses in the faeces; health suffered; constipation; frequent attacks of spasms in region of liver; the amount of fat passed to 2 or 3 oz. daily, and could not be distinguished from butter; motions light coloured and offensive; urine scanty and high coloured; increasing emaciation; sank; her legs be-</p>	<p>Liver small and pale; gall-bladder and canals empty; pancreas reduced to a fatty mass; its ducts contained calcareous matter; fluid in the chest.</p>

coming anæsthetic a few days before death, and her mind failing.

Male. 35¹ *Balan*; regular habits.

In 1814, rheumatism; some months later jaundice; the two alternated up to 1818; when under treatment was freed from both; in May 1820 slight rheumatism.—In October reduced much from discharge of fatty matter, existing two months; it had been observed after the first attack of jaundice; when they were at their height it always decreased; at last it had ceased. The use of animal food caused, but a vegetable diet decreased it. It continued up to the middle of November 1823—did not interfere with his health—when it suddenly ceased, and was followed by great weakness; feet began to swell, and he became dropsical, and sank on the 21st December. He had had cough and expectoration since the spring.

Male.

Male. 48¹ Regular; taking from a half to a pint of wine daily.

Long subject to indigestion, and constipation, after some indigestible food—had chronic duodenitis; relieved; an attack of urticaria was followed by marked relief; but on its subsidence increased pain in epigastric region, particularly below the cartilage of the eighth rib of right side. Jaundice appeared (one month from commencement of attack); motions clay-coloured; health otherwise good; yet he emaciated gradually; the hue of the skin became very deep; pulse rose to 120; had great tenderness in the right side of epigastrium,

No fat on the omentum, mesentery, or about kidneys; the intestines of a dark colour, and their mucous membrane generally softened; liver of a bad colour, with black patches here and there on it; no change in texture; the gall-bladder some clear, dark-brown gall; the cystic duct a calculi; the ductus choledochus was greatly widened, above was pressed on by head of pancreas; below this point narrowed; the pancreas was, throughout, hardened, with calculi in ducts.

Liver very large; gall-bladder and canals distended with thick, dark bile; the right head of the pancreas formed with some lymphatic glands a large tumour, due to an inflammatory change; the duodenum, where the common duct opened into it, was so contracted, that the extre-

¹ Mr Eastcott's London Medical Gazette, 1838.

² Mr A. Clark, *Lancet*, 1851.

³ Mr Lloyd, *Medico-Chir. Transactions*, 1838.

⁴ Dr Elliotson, *Medico-Chir. Transactions*, 1838.

⁵ Kunemann *Hufeland's Journal der Prat.* Heilkund, 1821 and 1824.

Sex.	Age.	Habits and Occupation.	Causes and Symptoms.	Post-mortem Appearances.
Female.	31.	Irregular.	<p>and the gall bladder was greatly distended; slightly improved. Two months after this time yellow fatty matter appeared in the motions, mixed with the feces at times, but generally distinct and varying in quantity. The motions became darker, but never had a bilious hue; when they got pale the fatty matter stopped. Later, vomiting set in of the food taken the twelve or twenty-four previous hours; appetite good; death from exhaustion, about eighteen months from the commencement.</p> <p>Entered with jaundice and anasarca; health falling for last two years, but particularly during last two months; motions clay-coloured, and tinged with blood; fetid, with a thin, greasy scum on surface and in its lower part; and pain and distension in the upper part of right side of abdomen.—Sank four days after admission.</p>	<p>mity of the blow-pipe would alone pass; the opening of the common duct into the duodenum was quite closed, and the duct contained a yellowish-brown fatty fluid, like that which had been passed during life; stomach very large.</p> <p>Liver large; gall-bladder and ducts distended; pancreas contained two fungoid tumours, one in the right head, the other in the centre; bronchial and mesenteric glands enlarged; renal capsules enlarged, particularly the left; apex of left lung a fungoid tubercle; small intestines with fungoid ulcerations throughout.</p>
Female.	50.	—	<p>Entered with slight jaundice, of six weeks duration, and much emaciated; seventeen years before an affection of liver; since occasional attacks of sickness and vomiting, which of late frequent; coming on half an hour after meals, and when stooping down; three months ago seized with severe pain in abdomen, with diarrhoea, the food passing undigested; pain still continues, and is relieved by pressure; motions nearly white; after a dose of castor oil, passed fatty masses; but this she had done before, and continued, although no fatty</p>	<p>Liver containing several encephaloid tubercle; gall-bladder distended with thick bile; right head of pancreas affected,—the whole affected, but in a less degree; glands were enlarged; opening of the common duct small, and in the centre of an ulcer; mucus membrane of duodenum soft and</p>

spongy; it contained half a pint of dark grumous fluid; kidneys large and flaccid.

Liver contained cancerous masses; ducts free; pancreas (right head) cancerous; mesenteric glands and kidneys diseased.

Right head of pancreas converted into a cyst, containing sero-sanguinolent fluid, and some calculi; pancreatic duct obliterated by calculi; gall-bladder and ducts distended by bile; from the cyst pressing on the choleoidic canal it was obliterated.

matter taken; three weeks after admission, pain in abdomen severe; nausea and vomiting of ropy, grumous fluid; a few weeks later passed, by motion, dark pitch-coloured matter, and a week before death clear blood.

Three years ago pain in right side of epigastrium set in, to which was added nausea; acid eructations and dark-coloured vomiting; ceased and then returned; the motions then became dark; heart's action violent, but no bruit; one month before death entered hospital; conjunctivæ of a dusky hue, but not jaundiced; pain with a tumour on the right side of epigastrium; frequent vomiting; copious dark coloured motions, in which oil was seen floating.

Subject for last thirteen years to hemorrhage from bowels; very severe during last three, and attended by great tenderness in the abdomen, and alternated with diarrhœa; digestion good.—In December 1836, after exposure to damp, febrile symptoms set in, with abdominal pain, tenderness, and constipation, followed by diarrhœa, the motions containing no bile, but blood.—Fifteen days after fat was first observed, and continued up to May, when it disappeared, after the febrile symptoms disappeared returned to work, the pain and tenderness continuing, and recurring in paroxysms every eight or ten days; appetite soon failed; the motions became colourless, and jaundice appeared; still later a painful tumour appeared; fell into a state of coma on the 16th September, and died on 17th.

37^a

Male.

40^b

Male.

^a Dr Elliotson, Medico-Chir. Transactions, 1833.

^b Professor Greene in Dublin Medical Journal, 1846.

^c Dr Goss in Boston Anatomical Museum, 1847, taken from the Arch. Gen. de Médecine, 1849.

Sex.	Age.	Habits and Occupation.	Causes and Symptoms.	Post-mortem Appearances.
Male.	49 ¹	Clerk, sober and regular.	In March 1827 taken with diabetes.—Early in September jaundice appeared; motions clay-coloured; liver large, and gall-bladder felt distended.—28th December, began to pass fat like butter; had taken none; disappeared on 31st.—January 8, reappeared, and the motion contained undigested meat.—16th, again observed; motions dark and containing undigested food; feet rather cedematous.—In February, pleuritic symptoms set in, and he sunk on the 26th.—The diabetes underwent throughout but little change.	Right head of pancreas formed a large globular mass; liver large, gall-bladder and ducts distended, with thick, dark bile; the common duct ended in a cul de sac in the distended part of the pancreas being stopped up by a deposit of fibrine or cholestrine; scirrhus ulcer in the duodenum; Tubuli of the kidneys prominent, and contained some white spots of either fibrine or calcareous matter.
Male.	35 ¹	Labourer.	Suffering from purging of fatty matter; liver felt slightly enlarged; urine was passed in excess; sp. 8.4030; sugar to tests; under the microscope epithelium scales and fat globules; the diabetes disappeared before death; the motions slimy, deficient in bile, and containing fat; four or six stools a day; death from asthenia, having been under treatment fourteen months.	Head of pancreas converted into a hard scirrhus mass; rest of gland atrophied; its duct obliterated, and forming an imperious cord.

¹ Dr Bright, Medico-Chir. Transactions, 1833.² Mr Marston, Glasgow Medical Journal, cited in Edinburgh Monthly Medical Journal, January 1854.

NOTE.—A case of fatty discharge has been published by Dr Eisenman in the Pragus Vierteljahrschrift. In consequence of an error, I have not been able to obtain this work in time to complete the paper, which, with this exception, and a case in a Spanish journal, the notes of which I have mislaid, contains all, I believe, that has been published on the subject.



ARTICLE II.—*Surgical Cases, with Remarks.* By JAMES SPENCE, Fellow of the Royal College of Surgeons, and Lecturer on Surgery, Edinburgh.

I HAVE selected the following from cases occurring in my practice, principally as they seem to me to illustrate some points in surgical pathology, and to possess interest in a practical point of view:—

CASE I.—*Osteo-sarcoma of Forearm; Amputation; Recovery.*

I was requested by Dr Cruickshank of North Berwick, to visit with him, Elizabeth Watt, residing at Williamston, who was affected with a large tumour of the forearm.

About eight years previously she had first observed a small firm swelling a little above the wrist, which continued to increase gradually and slowly, without much pain or inconvenience. About four years prior to my seeing her she had become alarmed about the tumour, and applied to Dr Cruickshank, who recognised the disease as osteo-sarcoma. The swelling at that time was about the size of an egg, limited distinctly to the lower end of the ulna, and had begun to interfere with the motions of the forearm and wrist. Dr Cruickshank advised her to submit to amputation at the middle of the forearm; but the patient and her friends would not listen to the proposal, and applied for assistance to other practitioners. A variety of remedies, both general and local, seem to have been used, but without any good result. At last she fell into the hands of a bone doctor, and, as far as can be judged from her description of the treatment, he seems to have applied first a strong tincture of iodine, and ultimately some form of escharotic to the tumour, which gave rise to ulceration at one point, and still more rapid increase of volume, profuse discharge, pain, and great constitutional disturbance. She returned home, the tumour having attained an enormous size, and her general health completely undermined by the hectic induced by the profuse discharge and pain. Dr Cruickshank now saw her again, and he found her anxious to submit to any operation to obtain relief.

At my first visit she was lying in bed with the enormous solid tumour supported on a pillow: it felt of the consistence of bone, with here and there softer points, the superficial veins were enormously enlarged, the fingers were firmly clenched, and the nails were elongated and curved like claws. A large, deep, ulcerated cavity in the tumour displayed portions of its osseous substance partly dead, and mixed with unhealthy discharge, and emitting a most insupportable fetor. Her general appearance indicated an extreme degree of debility: she was excessively emaciated, with sunk, anxious features, small, rapid pulse, etc. On examining the axilla, I found decided enlargement of one or two glands.

The extremely debilitated condition of the patient, together with the enlarged glands in the axilla, and the rapid growth latterly of the tumour, taken in connection with her cachectic look, seemed to militate against amputation; but, on the other hand, the originally simple character of the tumour, its slow progress up to the time of being irritated by stimulating applications, and its still retaining a solid osteo-sarcomatous appearance, together with the consideration that the mere irritation produced by the ulcerated portion was sufficient to cause enlargement of the axillary glands, and that its removal, though only as a temporary alleviation of her suffering, would be a boon to the patient, made me decide on amputating. Dr Cruickshank having put the patient under chloroform, I removed the arm by double flap a little below the insertion of the deltoid. There was great venous hemorrhage in making the incisions, and the blood flowed in a stream from the veins of the tumour, even after the bone was sawn through.

Six or seven arteries required ligature. After the patient was put in bed she was excessively weak, but gradually revived. An opiate was ordered to be given after the effects of the chloroform had passed off.

I visited her again on the fourth day after the amputation. The stump was looking well, and her general appearance much improved. Her pulse was under 90, and she had slept well ever since the operation. I continued to see her from time to time. The greater part of the stump was soon firmly healed. About the tenth day a large abscess formed in the axilla and over the pectoral muscle; this was opened, and, as it was diffuse, a counter opening was made in the arm. Under careful treatment she continued to improve, and was soon able to get up and go out.

I saw her in September 1853 in excellent health, and examined the stump and axilla carefully. There was not the slightest trace of any tendency to reproduction of the disease, and she still continues in perfect health.

The tumour, when removed, was found to weigh eight pounds and a half; its length was fourteen inches, its greatest circumference, corresponding to about the middle of the forearm, was eighteen inches, immediately above the wrist sixteen inches, and near the elbow twelve inches. It consisted almost entirely of osseous matter, with an admixture of soft fibro-cartilaginous-looking deposit. The great vessels and nerves of the forearm, together with the muscles and tendons, passed into and through the tumour, or formed grooves for themselves, more or less deep, as represented in the accompanying plate. The diseased growth seemed limited at each extremity by the articular cartilage; but, although commencing distinctly in the ulna, the disease had progressed laterally so as to blend the shafts of the ulna and radius inseparably into one general mass. Neither the humerus superiorly nor the carpus inferiorly were implicated, but the cartilage of incrustation of the bones of the forearm was partially absorbed. The veins on the surface of the tumour were of enormous size, as seen during its dissection.

Remarks.—The foregoing case, independently of the enormous size of the tumour, has interest in a practical point of view in reference to the pathology of similar growths. 1st. The slow progress in development whilst left alone, compared with the rapid increase in volume when the use of deobstruent remedies were had recourse to in the vain hope of discussing the tumour. During the four years which the tumour had existed prior to her showing it to Dr Cruickshank it had only attained the size of an egg, and it was some time after he had recommended its removal by operation that the patient applied for advice to others, and commenced the use of remedies to promote its discussion. Yet, under such treatment, it increased to the enormous size it ultimately attained in a shorter period than it took to reach the comparatively small size of an egg when left to its own natural progress. The period, however, from which the constitution became affected was after the application of the escharotic which had caused the ulceration of the tumour.

This can be readily accounted for by the local irritation produced by the ulceration of the opened out texture of the osseous tumour, the necrosis of portions of it which took place, and the profuse discharge consequent on these conditions. The history of the case in this respect may serve to enforce the good practical rule that in cases of tumours where removal by operation is objected to by the patient or deemed inexpedient, the safest plan is to refrain from meddlesome treatment.

Another point of interest is to determine how far in such aggravated cases the originally simple character of the tumour should influence our views as to whether the local and constitutional symptoms presented are really indicative of malignant degeneration, or merely depend upon excessive local irritation and continued discharge. To this point I have already alluded when stating the circumstances which influenced me in my decision to amputate, even when there was glandular enlargement in the axilla combined with the appearance of malignant cachexy. There can be little doubt that even the most simple tumours do, under certain circumstances, occasionally degenerate and assume a malignant character; but, on the other hand, I believe that careful investigation will show that in a great majority of cases the frequency of such alteration in character in simple growths is more apparent than real, and that the peculiar frightful looking ulcerations noticed depend upon the results of excited action in a growth foreign to the organism, the intimate structure of which at least is endowed with no great degree of vitality or power of resistance to diseased action. The enlargement of the neighbouring absorbent glands is readily accounted for from the amount of local irritation, whilst the cachectic appearance of the patient is the result of the constitution suffering from the continued pain and discharge. This is well seen in large fibrous tumours of the mammæ, where, the skin ulcerating, fungoid masses are produced, which partially slough, and give rise sometimes to repeated hemorrhages. Such tumours present at first sight a most frightfully malignant appearance, and from the sallow complexion and anxious expression of the patient might readily be mistaken for fungus hæmatodes, unless the originally simple character of the tumour was known, and thus a patient might be left to perish from a disease which can be permanently and easily remedied by a simple operation. I think, therefore, that the original simple character of a tumour ought to form a most important element in judging of the propriety of operating in doubtful cases, and the mere existence of glandular enlargement, or the cachectic appearance of the patient, ought not to deter us from operating unless other circumstances, such as an unequivocal alteration in the character of the tumour or the evident inability of the patient to undergo the operation, contraindicate its performance. Lastly, the dissection of the tumour possesses some interest in regard to the growth and progress of osteo-sarcoma. Like other simple growths, it has no tendency to involve the surrounding parts in similar disease, the vessels, muscles, nerves, and tendons, as in this case, pass over or through the diseased mass, and their functions may be impaired by the mere mechanical pressure of the tumour, but they are not involved in its diseased condition. It has been long observed that osteo-sarcoma is limited to the bone originally affected, and does not extend beyond the articular cartilages. A knowledge of this fact enables the surgeon to decide on operations which otherwise would be inadmissible. In the pre-

sent case the tumour was limited below by the wrist joint, and above by the elbow; the carpal bones, the condyles, and the trochlear surface of the humerus being quite unaffected. The diseased condition, however, has not been limited to the ulna in which it distinctly originated, but has involved the radius, so that both bones of the forearm are inseparably blended together. How has this occurred? Possibly by the lateral enlargement of the ulna near the wrist coming in close contact with the radius, the pressure causing absorption of the periosteum at some point; and the surfaces of bone so brought and maintained in direct contact uniting, and the disease then progressing as in one common structure. But if this can occur between the shafts of two distinct bones, may it not also occur between articular extremities of bones, the cartilage of incrustation being removed, as is partially the case in the articular surfaces of the bones of the forearm in the present instance. Union might then occur between the osseous surfaces so exposed and in contact, and thus we might have extension of the disease propagated beyond the articulation. If such extension ever does take place, however, it must be of very rare occurrence, to judge from the invariably limited character of the disease in the recorded cases, and it would be curious to ascertain to what we are to attribute the circumstance that the disease may involve the shaft of another bone with which it comes in contact, and yet is so constantly limited by the articular surfaces.

CASE II.—*Acute Necrosis of Fibula; Hemorrhage; Ligature of Superficial Femoral; Recurrence of Hemorrhage; Amputation; Recovery.*

Mr A. T., æt. 23, first began to suffer pain in the left leg about the middle of July 1852; for the first few days he states it was more a feeling of weight and uneasiness than of actual pain. But on the 20th the limb became very stiff and painful, and he had a shivering. After this the pain increased, and Dr Hislop of East Linton was called to attend him. Dr H. states that, from the absence of any symptoms of local inflammation, and from the febrile symptoms being slight, he first thought the pain neuralgic. On the 22d, however, there was an appearance of local inflammation on the outside of the leg, a little below the knee, whereupon Dr Hislop ordered leeches to be applied, and the limb to be fomented. The inflammatory swelling and tension now increased rapidly, and assumed an erysipelatous character, attended with irritative fever and great general constitutional disturbance. Dr H. ordered acetate of lead and opium fomentations to be applied, and made one or two incisions. About the 15th of August he felt fluctuation about the middle of the leg, and made an incision which evacuated some purulent matter. I saw the patient for the first time on Tuesday the 17th August. He seemed much emaciated and exhausted from irritative fever, pain, and want of sleep. His pulse was 130, and sharp, tongue furred, with red edges, and he had a tendency to dysentery. The affected leg was enormously swollen, red, and tense, contrasting strongly with the opposite leg, which was reduced nearly to skin and bone. On examination, I felt distinct fluctuation deep-seated over the whole length of the fibula, and accordingly I made two incisions about two inches long, the one over the upper part of the fibula, the other above the external malleolus, and also enlarged the incision at the middle of the leg which had been made previously. On passing the finger into these incisions, I at once felt the denuded surface of

the fibula, and thus satisfied myself that the whole length of that bone was affected with acute necrosis. A very large quantity of pus was discharged by the incisions. The limb was ordered to be poulticed, and an anodyne draught was administered. I did not see him again till the 28th August; in the meantime the pain and swelling had greatly diminished, but there was still great discharge, and he had become hectic and was exceedingly weak, nervous, and excited, and complained of a sense of oppression in the præcordial region at night. On examining the leg I found a loose sequestrum immediately above the external malleolus, which I readily removed; it composed rather more than half the thickness of the bone. But the dead portions of the upper parts of the shaft had not begun to separate, and from the state of his general health, I felt doubtful of the result. On Thursday, 9th September, as his health was rather worse than better, I proceeded to remove the dead portions, assisted by my friend Dr Duncan.

Dr Hislop having administered chloroform, I passed a probe-pointed bistoury down to the bone, through the incision over the middle of the fibula, enlarged the opening upwards and downwards to the extent of $5\frac{1}{2}$ inches, and extracted a long sequestrum, consisting at one part of about an inch in length of the whole thickness of the bone. I next enlarged the upper incision to the extent of three inches, and extracted another sequestrum, but some small portions seemed still attached to the head of the fibula. There was no great amount of bleeding, and the wound was dressed with dry lint, and supported by a moderately tight bandage. I saw him again on the Saturday, and dressed the wound; there had been no oozing after the removal of the dead bone, and the wound was looking well. As I happened to be residing in the neighbourhood for a few days, his brother called on me on the Monday forenoon following, and told me that he seemed much easier, and in better spirits than for some time back. About ten p.m., however, I received a message from his brother, stating, that on reaching home he had found that alarming bleeding had occurred about a quarter to nine p.m., and that Dr Hislop wished me to come immediately to the case.

I accordingly went, and reached the patient's house about half-past eleven p.m. He had by that time somewhat recovered from the first effects of the loss of blood under the use of stimuli, but was nervous and excited. I learned that the bleeding had taken place slowly and unnoticed until he felt sick; he was talking with his mother and sister at the time, and on his complaining of faintness, they happened to look at the bed, which they found covered with blood; Dr Hislop was sent for, and arrested the bleeding by pressure. Having applied a tourniquet over the femoral artery, I cut up the bandage, and found the leg more swollen than when I last saw it, as if from coagula. I accordingly removed the lint from the large incision in the leg, with my finger removed the coagula, and then with a sponge cleared the large exposed surface. The edges of the wound being held apart, I directed the tourniquet to be gradually slackened, till all compression was taken off the femoral. No jet of blood was noticed. I again examined the wound with my finger, and as the lymph seemed firm, I placed slips of lint into the deep part of the wound, and then a larger compress, and secured them by a roller applied from the toes upwards. An opiate was then given to procure sleep.

I remained at his house all night, and at nine o'clock, before I left for Edinburgh, I saw him with Dr Hislop; there had been no recurrence of the bleeding. I was still suspicious, however, from the amount of blood lost, and the long-continued diffuse suppuration which had previously existed, that the fibular artery might have been disorganized and given way from sloughing. I accordingly came back from town as soon as I could, and on reaching Linton I found that bleeding had just recurred to a considerable amount. Assisted by Dr Hislop and my friend Dr Littlejohn, who had accompanied me, I again examined the wound. The extent of the incision enabled me to see the whole deep part of the wound; on clearing which of coagula, I found the textures

generally covered with firmly adherent lymph, but some of the deeper parts looked sloughy and unhealthy. The bleeding welled up from the surface of the wound, but not in a jet. The patient's state demanded that some decided measures should at once be adopted, since direct graduated compression had failed. As to direct ligature on the bleeding point, although I suspected the fibular artery, yet, though the large incision exposed the surface of the wound fully, there was no jet or distinct indication of bleeding from any one point, whilst the disorganised sloughy condition of the deep surface of the wound rendered it more than doubtful that a vessel, even if discovered, would have held ligatures. There remained, therefore, only the alternatives of ligature of the femoral artery, or amputation. The latter, from the state of the patient, and the extensive suppurating surface, might ultimately be necessary even for other reasons; but I was very unwilling to sacrifice the limb, and, moreover, from the faint condition of the patient at the time, he would not have been able to bear the operation. I therefore determined to tie the superficial femoral, in order to control the general circulation of the limb, and to assist it by moderate graduated pressure directly applied to the wound. The patient having been brought under the influence of chloroform, I at once proceeded to tie the vessel, which was readily accomplished without removing the patient from bed. All oozing from the wound ceased immediately. Some lint was placed over the wound, supported by a thin flannel roller lightly applied. On the second day after tying the vessel, I saw him again. He was much better, the wound looked healthy, and everything seemed to promise well, as the circulation in the anterior and posterior tibial arteries was fully re-established. All went on well till the 21st September, when I was sent for in the afternoon, as bleeding from the leg had again occurred. I found the patient recovered somewhat from the first effects of loss of blood, which had not been to a large amount; but I felt I could no longer persevere in attempts to save the limb, as any farther bleeding might have proved fatal, since the deep part of the wound was still sloughy, a portion of the necrosed fibula close to the knee had still to separate, and the patient was exceedingly debilitated from the previous exhausting disease, as well as from the hemorrhage. It was decided to amputate the limb at the lower part of the thigh. I had foreseen that this usually simple operation would be complicated in this case, both on account of the greater number of vessels which would require ligature in consequence of the enlargement of the collateral branches after ligature of the femoral, but especially from risk to the deligated artery itself, from any traction exercised on it during the operation, as it was about the period when the ligature begins to separate by ulceration, and accordingly had obtained the assistance of my friends Dr Handyside and Dr Littlejohn. Dr Hislop administered chloroform, and as soon as the patient was under its influence, he was carefully turned round, and drawn towards the edge of the bed, and the limb steadily supported.

Dr Handyside having compressed the common femoral, I removed the limb by double flap above the knee. The mouth of the superficial femoral was readily recognised, but I first secured the enlarged collateral branches, which could not be so completely commanded by compression. Before tying the superficial femoral, Dr Handyside, at my request, relaxed the compression somewhat, and blood flowed from its mouth, but, as might be expected, not in a jet. It was then tied; the flaps were approximated by sutures, and the stump was dressed. There was little blood lost during the amputation, but the patient was very weak, and it was some time before he fully rallied under the use of stimuli, and he required careful watching during the night to prevent the circulation flagging; indeed Dr Littlejohn and I watched him constantly. Towards morning reaction became fairly established, and he took some food; an opiate was given him, which procured him some refreshing sleep. I returned to town, but took the precaution of leaving my senior apprentice, Mr Rhind, in constant charge, in case of accident, till the femoral ligature came away, as Dr Hislop of course was often engaged with cases at a

distance. At first the stump healed very rapidly, except where the ligatures hung out. The ligature of the superficial femoral separated on the 15th day from the time it was applied, or about eight days after the amputation. Subsequently the patient suffered from irritation of the bowels, and one or two attacks of general erythema supervened, beginning on the face, and spreading over the trunk and stump, but gradually, under the use of the tincture of the muriate of iron, and attention to diet, his general health began to amend, and then the stump healed well. Last summer he was in perfect health, and able to walk considerable distances, and still continues, he says, in better health than for many years before his illness.

Dissection of the Limb.—I carefully examined the amputated limb the morning after the operation. I found all the textures covered and matted together with firmly consolidated lymph, so that on removing the gastrocnemius and soleus muscles, no trace could be seen or felt of the posterior tibial vessels and nerve, or of the fibular artery. To avoid all risk of accidental lesion of these vessels during dissection, I cleared the popliteal artery, and being unprovided with a syringe, I passed a long probe gently downwards, so as to guide me in the direction of the vessels. By very careful dissection, I cleared the upper part of the posterior tibial artery, and in doing this I came upon a soft and sloughy portion of lymph, which readily broke down under the handle of the scalpel, and proved to be part of the cyst of a small abscess or cavity about the size of a large filbert, containing pus and grumous blood, in which a portion of the fibular artery lay insulated, and of a dirty green colour. I withdrew the probe from the posterior tibial artery, and passed it gently into the fibular; there was no opening on the posterior surface of the vessel, or that towards the wound. I therefore made an incision so as to remove a considerable portion of the vessels, and surrounding soft parts, so as to be able to examine it more carefully afterwards. On doing so, I found the vessel immediately above and below the small cyst or abscess encrusted (if I may use the term) with firmly adherent lymph; this lymph was continuous with that which formed the walls of the abscess, and opened anteriorly by an irregular ulcerated orifice. On enlarging this opening, so as to expose the anterior surface of the peroneal artery, and on injecting water from the popliteal, a hair-like stream was seen to issue from the forepart of the fibular artery. I then removed the whole anterior portion of the cyst, and found the peroneal artery perforated by a minute ulcerated opening, large enough to admit a bristle. The coats of the vessel, however, were soft, thin, and of a greenish colour, and completely insulated for about an inch, whilst beyond the insulated part the tunics of the vessel were inseparably incorporated with the lymph, so as to defy all attempts to clear the vessel by dissection. Some small necrosed portions of the fibula still remained attached at its upper part.

Remarks.—The principal point of interest in this case is the complication caused by the occurrence of hemorrhage from the ulceration of the fibular artery, in consequence of its being isolated by the suppuration around it. At the same time, to form a correct view of the treatment adopted, it is necessary to keep in mind all the circumstances of the case; for the previous debilitated state of the patient, from the long-continued irritative fever; the subsequent profuse discharge, hectic, and general exhaustion, together with the disorganized and altered condition of the structures of the affected part of the limb, were all complications requiring to be carefully considered, in deciding on the measures to be adopted when the secondary hemorrhage occurred.

I have said that I suspected that the bleeding might come from the fibular artery, from the relative position of that vessel to the dis-

eased bone, and the unhealthy suppuration around. Hence my first object was to try and secure the vessel directly at the bleeding point. But, on examining the exposed surface, which the extensive incision in the leg readily admitted of, there was no jet of blood or other indication to guide me, while the altered condition of the structures in the wound, the matting together of some parts, and the sloughy condition of others, caused me to desist, as I felt that, under such circumstances, without some direct indication of bleeding, a tedious dissection would be required to reach the vessel, with no certainty of reaching it at the open point; and, after all, my surmise as to its being the fibular artery, might be wrong. Again, if an opening in the vessel were found, its coats might be so unhealthy as not to hold a ligature above and below the opening. The subsequent dissection of the limb proved the difficulties of direct ligature to be even greater than I had anticipated. The portion of the vessel where the opening existed, was surrounded by an encysted abscess, and had only bled indirectly into the wound; hence the reason that no jet could be observed on removing the pressure on the femoral, when examining the wound. The arterial tunics, where they were isolated, were so soft and sloughy, that they would not have held a ligature for a few hours, if at all; whilst immediately beyond the isolation, the whole track of the vessel was so incorporated with adherent lymph, as even to defy its separation to any extent, by careful dissection, when removed from the limb. What probability, then, would there have been of treating the vessel by direct ligature?

As to the method of indirectly controlling the bleeding, by weakening the general circulation through the limb, by ligature of the superficial femoral, no one can be more impressed than myself with its uncertainty, as compared with direct ligature, owing to the free anastomoses in the thigh, and around the knee, above the bleeding point. But when, for the reasons above stated, the latter could not be adopted, I considered it right to give the patient that chance, rather than amputate, as I had seen it succeed in cases of a similar character. I had likewise, by somewhat similar means, successfully arrested hemorrhage from ulceration of the brachial from sloughing, after a severe burn, by tying the third portion of the axillary artery, as recorded in this Journal for February 1848. It is true that in that instance I, at the same time, applied ligatures above and below the wound of the brachial; but these ligatures separated by ulceration on the 5th day; so that except for the ligature of the axillary artery controlling the circulation, bleeding would have recurred. Hence I felt warranted in trying it in this case; and, indeed, there was little room for hesitation, for the patient was so sunk from the recent bleeding, as to put immediate amputation out of the question; and as oozing was still going on, and direct pressure had failed, something required to be done.

There can be no doubt, I think, as to the generally admitted pro-

priety of giving preference to direct ligature above and below the opening, in all cases of wounded arteries, as a great general rule, and it is one which can scarcely be too much insisted on; but at the same time, we must keep in mind that there are exceptions to this, as to all other general rules, and that much must depend upon the state of the vessel opened. If its coats are diseased and sloughy, if their vitality and that of the surrounding parts be impaired, as after extensive unhealthy suppuration or severe burns, then direct ligature, if trusted to alone, without controlling the force of the circulation by ligature on a healthy point of the vessel higher up, must often prove abortive, and repeated hemorrhage from rapid ulceration of the deligated part of the artery be the result. In fact, everything depends upon the state of the arterial tissue in the neighbourhood of the lesion, and the probable destructive power of the agency producing it. I have instanced burns by fire; on the other hand, ulceration from caustics generally leaves the parts, whence the slough has separated, healthy, and curiously enough, the very patient, whose case I have just narrated, nearly lost his life on a previous occasion from ulceration of the radial, caused by nitrate of silver applied to a bite. On that occasion I also saw him, but, knowing the limited action of the caustic, I exposed and tied the radial above and below the ulcerated point with success. But it would have been a very different matter in the sloughy condition of the fibular in the present instance, even if there had been any indication to guide me to the bleeding point. In the present case, the ligature of the femoral did effectually prevent all hemorrhage for a time; it allowed the patient time to rally; and when, on the circulation being fully established, the bleeding recurred, it was both less active and less in amount, and had the patient been even tolerably strong, I doubt not but that moderate and regulated pressure might have ultimately succeeded in arresting further hemorrhage. But, debilitated as he was by the previous exhausting disease and the repeated bleeding, and peculiarly depressed by the fear of hemorrhage from a remembrance of the risk he formerly ran, I felt that even a very trifling loss of blood might prove fatal, and the danger to life seemed so great, as to forbid any further attempts to save the limb.

CASE III.—*Disease of Bones of Tarsus; Amputation at Ankle Joint; Recovery.*

M. Rigali, ætat 25, when a boy, had suffered from disease of the os calcis; portions of diseased bone were removed at that time, the wound healed and continued well until the present attack. About five weeks prior to my seeing him, he felt pains in the foot which soon became very acute; the foot became inflamed and swollen, and he was treated for erysipelas, but without relief. I saw him for the first time on the 9th of February 1852. The foot was swollen, tense, and of an erysipelatous appearance; he was suffering from irritative fever, pulse 120, dry furred tongue, great general depression and exhaustion from want of sleep. The local pain was most severe over the region of the heel, but extended generally over the foot. From the history of his case, I felt satisfied that the symptoms depended on disease of the bones, and this

opinion was confirmed on introducing a probe into an opening at the old cicatrix on the heel, when I felt the bone bare for a considerable extent. On grasping the anterior part of the foot and moving it laterally, the tarsal bones were distinctly felt grating on each other. As there was much tension and inflammation over the dorsum of the foot, I made two longitudinal incisions, which gave vent to unhealthy purulent matter, and the probe introduced into these incisions at once passed down into the diseased bones. I ordered him an opiate draught, and directed the foot to be dressed with lint dipped in warm water, and covered with gutta percha membrane. Next day the pain, swelling, and redness of the foot were diminished, and there had been a considerable discharge of foetid pus. I directed the foot to be enveloped in a large poultice at night, the warm water dressing to be continued during the day, and directed him to have an opiate at bedtime to procure sleep, and also to arrest diarrhoea, which had commenced. The tension and inflammation gradually yielded to this treatment; but profuse foetid discharge continued, with great pain on the slightest movement of the foot or leg, and the irritative fever subsided into hectic, with loss of appetite and profuse perspiration, alternating with diarrhoea. The use of quinine and sulphuric acid, with wine and beef-tea, was had recourse to, in order to support his strength till the erysipelatous form of inflammation, which had involved the leg, was sufficiently subdued to allow me to remove the foot. Having obtained his consent to the operation, I amputated the foot at the ankle-joint, on the 18th February. Owing to the previous inflamed state of the parts, a very considerable number of vessels required ligature. The stump was covered with lint dipped in cold water, and an opiate given. He complained a good deal of smarting pain in the part, but slept better after the operation than he had done for some weeks. There was no tendency to erysipelas attacking the stump, which healed very well. His general health improved steadily, though at first slowly, and at the end of two months he was able to walk about on the stump. He has since continued in excellent health, and is able to walk considerable distances without difficulty.

Dissection of the Foot.—On examination the whole of the tarsus was found more or less affected with caries, whilst the os calcis presented extensive necrosis of its outer fibrous lamella, as did also some of the other bones. The part of the os calcis from which portions had been removed when the patient was a boy, seemed firmly healed by deposition of new bone, and that part did not seem affected by the more recent disease.

CASE IV.—*Necrosis of Head of Tibia; Secondary Affection of Knee-Joint; Amputation; Accidental Attack of Scarlatina; Death.*

On the 20th October 1852, I was requested by Mr Falconer, surgeon, Loanhead, to visit J. B., residing at Lasswade, on account of disease of the left knee-joint. On seeing him with Mr Falconer, I found the knee much swollen; over the upper part of the tibia the skin was discoloured, and an opening surrounded by unhealthy granulations led into a cavity in the head of the bone, the walls of which felt rough, and the probe detected some loose portions of dead cancellated texture. There was considerable boggy swelling and redness over the knee-joint; but the principal swelling was evidently caused by great effusion into the cavity of the joint, the patella moving on the surface of the deep swelling. Some years prior to my seeing him, he had had an operation performed for removal of a sequestrum from the head of the tibia, and after its removal continued well for a considerable period, but from time to time he suffered from severe pains in the head of the tibia; the cicatrix opened up, discharged matter, and then a temporary relief occurred. Of late, however, the pain had been very severe. About three months before I saw him, swelling and pain began in the knee-joint, attended with febrile irritation and loss of sleep; and for some weeks before my visit, his health had been rapidly sinking from hectic. His pulse was 130 and weak, besides the swelling of the knee, the affected limb was œdematous, and he suffered intense pain on

pressure or movement of the knee. His extreme weakness and the œdematous state of the limb gave me an unfavourable opinion of the case ; but as both he and his friends were anxious he should have the chance of relief afforded by removal of the limb, I amputated it at the middle of the thigh, by double flap, on the 25th October. During the operation a large escape of serum from the cellular tissue took place, but he lost little blood. He was directed to have forty drops of morphia, when the effects of the chloroform should have passed off. I saw him on the third day after the operation, and dressed the stump, removing some of the stitches, and applying adhesive straps to support the flaps. His pulse was still quick, about 120, but he stated that he had slept well. His bowels had been opened, and he passed water freely of natural appearance, and not coagulable by heat. His tongue was furred and rather glazed on the edges and tip. I was informed that one of his children had taken scarlet fever (then very prevalent) on the evening of the operation, but that the child had been kept away from him in a different room. On the next day I heard from Mr Falconer, that B. had had a slight shivering and complained of sore throat, and that towards the following morning the eruption of scarlatina appeared over his body, accompanied with increased sore throat, very rapid pulse, and delirium, and he gradually sank and died on the sixth day after the operation.

Dissection of the Amputated Limb.—On examining the joint I could not by pressure on the knee succeed in causing the contained fluid to pass out by the opening over the head of the tibia. On opening the joint it was found filled with turbid glairy fluid mixed with pus ; the cartilages had suffered but little, there was great injection of the synovial membrane, with patches of recent lymph, and considerable gelatinous deposition near the lateral ligaments on each side. On opening the cavity in the head of the tibia, it was found to contain a portion of dead cancellated texture lying loose ; the walls of the cavity were of a pale brown colour, and at some points covered by a lardaceous looking substance. On carefully passing a probe upwards, it was found to pass into the knee-joint through an ulcerated orifice with round thick edges, but overlapped internally by a fold of the synovial membrane, which had acted as a valve, preventing fluid from passing out of the joint when pressure was made on the knee.

CASE V.—*Caries of Os Calcis ; Previous Operation for removal of Diseased Bone ; Amputation at Ankle-Joint ; Recovery.*

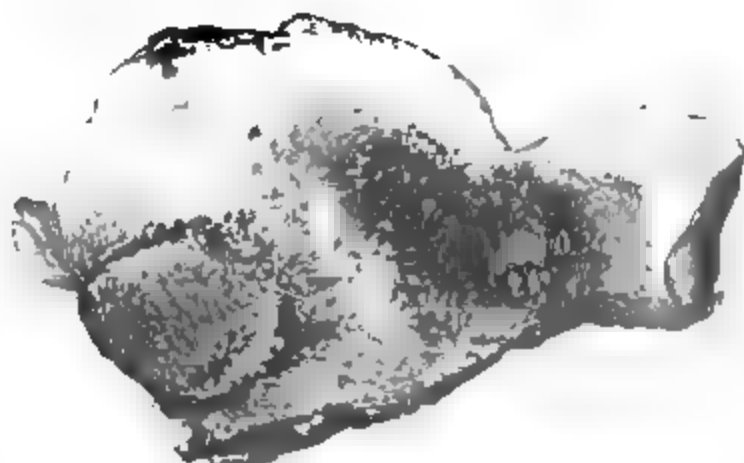
In May last, I was requested by Dr Thomson of Dalkeith to visit Mr L., who was suffering from disease of the os calcis. I was told that for some time previously he had been under surgical treatment, and that an operation had been performed with the view of removing the diseased portion of the bone, but that the wound had never healed. The pain had become intense, scarcely alleviated by large opiates, to which he had recourse ; and his general health was evidently sinking under the continued irritation. On examining the foot, I found the soft parts about the heel and ankle swollen, discoloured, and excessively painful on pressure. Deep-seated fluctuation was felt on the plantar aspect of the os calcis, on the posterior and outer side of the heel there was the citatrix of a crucial incision still partially open, and discharging thin fetid pus. On introducing a probe into the sinus, it passed readily round the os calcis, which felt rough and bare, and at one point it penetrated the substance of the bone itself. The general health of the patient had suffered greatly, both from the irritation of the disease, and also from his excessive use of laudanum. His pulse was 140, and weak ; he was greatly emaciated ; whilst his appetite and digestive powers were much impaired. He stated his age to be fifty, but appeared to be considerably older.

On taking all the circumstances of the case into consideration—that the previous operation for the removal of the diseased portion of bone had proved abortive, that in all probability the greater part of the os calcis was involved in

the disease, together with his age, debilitated constitution, and the risks of further partial operations, I proposed amputation at the ankle joint. He willingly consented to this, and I performed the operation on the third of May. The existence of the cicatrix of the former operation appeared rather unfavourable to the ordinary plan of operating; but any difficulty from that cause seemed more than counterbalanced by its advantages over other methods. I therefore operated as usual, and experienced little or no difficulty from the adhesions in separating the flap from the os calcis. The stump progressed favourably, but erysipelas appeared on the seventh day on the anterior part of the leg, and required incisions to be made to relieve the tension, and evacuate purulent matter. A considerable portion of the tendon of the tibialis anticus sloughed, and when this separated, the tendency to erysipelas disappeared, and the case went on favourably. The opiates were gradually withdrawn by diminishing the doses, and this was soon followed by a marked improvement, as regarded his appetite and general health. The discharge continued for some time from the orifice of the old cicatrix.

The stump is now solid and well formed, and can bear considerable pressure, though the patient complains of the weight of the artificial limb.

Dissection of the Amputated Foot.—On examination, the disease was found to be confined to the os calcis, with the exception of a slight ulceration of the cartilages of the corresponding surface of the os cuboides. On looking at the os calcis, its posterior and external surface was nodulated and rough, from the development of new osseous matter. Immediately below the insertion of the



tendo achillis, a circular black portion was observed, and on making a longitudinal section of the bone, this was found to be a circumscribed sequestrum of the cancellated texture; whilst at the anterior part of the bone was a carious cavity, containing portions of partially dead cancellated texture, as shown in the accompanying woodcut.

The pale brown colour and general appearance of the ulcerated surface in contact with the sequestrum, indicated its weak vitality and carious character, whilst at some points the cancellated texture had become dense and hard.

Remarks.—These three cases afford examples of certain peculiarities attendant on necrosis, occurring in the cancellated texture of the short or heads of the long bones, which renders that condition in it less amenable to treatment by removal of the sequestrum, than similar disease occurring in the shafts of bones. At first sight, this appears contrary to what we might expect, for whilst the looser texture of the cancellated structure would seem less likely to suffer from the pressure of abnormal congestion or exudation than the denser structure of the shaft, its larger amount of vascular supply

would also lead us to expect not only a greater degree of vitality and power of resistance to disease, but likewise of reparative power after the sequestrum was removed. I think the unsatisfactory result of operations for the removal of sequestra, and the recurrence of disease so frequently met with in such cases, are mainly attributable to a peculiar condition of the bone in patients of a scrofulous diathesis, predisposing it to disease, and rendering it less capable of reparative action. The condition I allude to is well seen in cases where amputation has been performed for disease of several of the tarsal bones. In such cases we generally find not only that parts of the diseased bones, but that other bones, not affected by positive disease, exhibit this tendency to degeneration. On making a section of such bones, we find the cancellated texture more opened out than usual, and filled with a peculiar lardaceous deposit or altered medulla; at other times by a deposit of cheesy or tubercular consistence, varying in colour from a white to a dark reddish brown. At other points, we often find the cancellated texture obliterated, and its place occupied by a dense hard structure, similar to what has been noticed in dental caries. Most generally, however, the bone is opened out and softened in texture; whilst in most cases the outer fibrous lamella is attenuated to a mere shell, and often perfectly diaphanous.

When actual disease does occur in a texture so altered in structure and impaired in vitality, its character must necessarily be of a low type, and with little tendency to repair. Hence if, under such circumstances, a portion of the cancellated structure be deprived of its vitality, and form a sequestrum, on its removal by operation, it does not leave behind it a healthy granulating surface, but a surface of bone affected by a weak form of ulceration. In fact, there seems little doubt that necrosis occurring in the spongy texture of bone, is, in the great majority of cases, the result of a form of interstitial caries, in which the ulcerative process has so isolated a portion of the cancellated texture, that it perishes—the original diseased condition still existing, to a greater or less degree, in the immediately surrounding parts.

The section of the os calcis in L——'s case shows this condition in the bone surrounding the sequestrum at the heel, whilst more anteriorly and distinct from it, we have another carious cavity, showing the predisposition to disease. From this state of matters, we can readily understand how, after the removal of one sequestrum, other portions of the cancellated texture, after a time, perish from the continuance of the morbid action, necessitating further operative interference, or leading to secondary disease of neighbouring articulations, as in B——'s case, where the carious cavity contained a new sequestrum, and by ulceration had opened into the knee-joint. At the same time, whilst these considerations modify our prognosis as to the favourable result of operations for the removal of sequestra, or portions of diseased bone in the class of cases

referred to, in practice we must have recourse to them, as in many cases we cannot be perfectly certain as to the extent of the disease, and it is evident we are not warranted in resorting to amputation till we have first given a fair trial to less severe measures for removal of the diseased portions of bone, which in many cases may prove sufficient. The case of Rigali seems to be an instance of this strumous condition in the bones of the tarsus, leading to a mixed form of extensive caries, and necrosis of acute character, and attended with local and constitutional symptoms similar to what we meet with in acute necrosis of the shafts of the long bones.

ARTICLE III.—*Reflections on the Duration of Pregnancy, with Remarks on the Calculation of the date of Confinement.* By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E., Lecturer on Midwifery, Physician-Accoucheur to the Royal Dispensary, etc., etc.

(*Read to the Royal Medical Society, January 13th, 1854.*)

IN the numerous elaborate essays which have been written on the subject of the duration of pregnancy in woman and in the inferior animals, it has always appeared to me that an important source of error has lain concealed. The exposition of it will, I trust, throw some light on this interesting subject; and I am sure that, when it comes to be completely investigated, our notions as to the duration of pregnancy will be much more definite and satisfactory than they now are. My object in the present communication is to make a few remarks on this particular point, and then briefly to discuss the general question.

In the beginning it will be useful to define the meaning to be attached to some important terms frequently recurring in this discussion, viz., insemination, conception, and impregnation. By the word insemination is to be understood simply the injection of semen into the genital passages, the conjunction of the two sexes. By conception is to be understood the more hidden and mysterious union of the semen and ovum; while the word impregnation implies both of these processes.

The confusion of the two former of these different processes is so general among obstetric writers, that it is needless to quote authorities for the assertion. That they should always be held distinct in studying this subject will, I hope, be made apparent. For, in fixing the commencement of pregnancy, it is necessary to date only from the period of conception. Authors, in discussing this subject, have delighted to quote as crucial examples those cases where the date of an only connection, or of connections within a short and limited time, could be satisfactorily decided. But it is evident that such a date only fixes the time of insemination, and not the time of the

commencement of pregnancy. For a woman cannot be said to be pregnant whose body merely contains seminal matter. Pregnancy is a state of fertility, of breeding, which, as Leeuwenhoek long ago pointed out,¹ cannot be said to commence until such time as may have elapsed after insemination, before the union of the ovum or ova and semen has taken place. This period of time, whatever may be its possible length, must be subtracted from all these supposed crucial cases of the duration of pregnancy. The interval described as the duration of pregnancy, that is, between successful insemination and parturition, must be considered as, in strict language, a false period; and it is so because it contains the period between insemination and conception, during which a woman is not pregnant. Of this interval, then, all such cases must be curtailed.

Very little has as yet been ascertained as to the possible length of this interval. It was my intention to have attempted to make it out in regard to some of the lower animals; but my inexperience in such investigations, and the pressure of other avocations, have hitherto deterred me from the pursuit of this object. There is, then, at present no resource in this question but to facts already known. Now it has been ascertained by physiologists that for impregnation it is not necessary the semen should be newly expelled by the male.² Animals have been frequently impregnated, by Spallanzani and others, with semen, which has not only been kept for some time, but has even been variously altered, in mechanical properties at least, in experiments. And there seems to be no limit to the time during which the semen may be kept without losing its virtues, except the term of the life of the spermatozoa.

That this period is not insignificant, and cannot be passed over without risk of important error; in fact, that it may extend to many days or weeks, will appear from the following observations. We omit the facts in regard to animals so low in the scale as insects, in the females of which the semen is laid up in cavities where it retains

¹ Hinc, hæc animalcula diutius in tuba sive matrice posse vivere, animo præsumebam meo, ac quoque nostræ mulieres non præcise eo die sive tempore, quo cum viro rem habuerunt, fecondas sive gravidas fieri; sed easdem post octo, aut decem, imo plures quidem dies, postquam coiverunt, gravidas posse fieri, quia post aliquot coitus dies ex multis saltem animalculis, unum animalculum eousque pervenire potest, ut punctum sive punctulum istud, animalculum fovendo aptum, attingat.—*Arcana Naturæ, etc.*, tom. ii. p. 150, edit. in 4to. Lugd. 1708.

² "On opening the body of a female mammal, one or more days after it has received the male, semen may be found not only in the body and horns of the uterus, but also in the oviducts, and on the surface of the ovary. The spermatozoa are in vigorous movement. These may retain their activity for a week or more in the female organs. And in many insects this period of time is much greater. Here the ova are only expelled long after copulation. The females, therefore, possess a special receptacle in which the moving spermatozoa are preserved until the ova finally reach them. In this receptacle their activity remains uninjured for many months."—*Valentin. Text Book of Physiol.* Eng. Tr. p. 641.

its power for months. In regard to the dog, Leeuwenhoek¹ pointed out that these animalcules might live for more than seven days preserved in a glass tube, and if such be the case in a rude experiment, it may be expected that they would retain vitality considerably longer in the passages of the bitch where they have heat and moisture supplied under favourable circumstances. That they do live for some days in the genital passages has been proved by abundant observations, although the possible length of this period is not certain. The decision, indeed, of this point by microscopic observations would be a very difficult matter, as it would involve the almost impossible search for spermatozoa over every part of a long tract of mucous membrane. And this search would be necessary, for we know by the experiments of Spallanzani, that semen highly diluted, or, in other words, the smallest quantity of semen is sufficient for successful impregnation.²

Again, the elaborate experiments of Haighton,³ long ago performed, show that in the rabbit conception generally does not take place till about fifty hours, or more than two days after insemination. He found that division of the fallopian tube earlier than this time prevented conception, and that, by waiting longer, the conception was not prevented by the mutilation. It thus appeared that the conjunction of the ova and semen in the rabbit generally did not take place till more than two days after insemination. In the rabbit, then, there was found in Haighton's experiments, this long interval between insemination and conception; and in some cases it is possibly much longer. In the rabbit the interval between insemination and parturition is ordinarily thirty days. The observations of Tessier upon 161 rabbits, give five days as the extreme limit of the protraction of this term, a period of time which may be accounted for without any stretch of the space during which the semen may retain its fructifying power. And in this way it may have happened that the real period of gestation, that is, from conception to parturition, may not have been at all protracted in these cases. The cases also in which the period was less than thirty days may be explained by supposing the ova to have been further matured or even advanced into the uterine horns before impregnation took place, so that conception may have happened very soon after insemination. And in Tessier's observations it is remarkable that in none of the rabbits did labour anticipate the usual time more than two days, the period which Haighton's experiments seem to show to be the usual interval be-

¹ "Si enim animalcula plures quam septem integros dies in tuba vitrea vivere possint, quantum temporis illa in matrice, his animalculis recipiendis ac fovendis unice constituta, vivere quidem possent."—*Arcana naturæ, etc.* Tom. ii. p. 150.

² These observations of Spallanzani have lately been considerably modified and corrected, by the researches of Mr Newport upon the quantity or number of spermatozoa required to fecundate an ovum in the frog, etc.—See his paper in the London Phil. Trans. for 1853. Part ii.

³ Philosophical Transactions. 1797.

tween insemination and conception in this animal. In the present state of our knowledge, however, these explanations cannot be absolutely established.

Experiments of Cruickshank upon the rabbit and doe, experiments of Wharton Jones, Martin Barry and others, might be adduced as throwing light on this point.

For reasons which do not require to be stated, there is great deficiency of evidence in regard to the analogous subject in the human female. But there is every reason to believe that the circumstances of conception in her, closely resemble those in the higher animals. It has of late years been shown that, in woman, at every menstrual period an ovum is matured and expelled from its graafian vesicle, and that she is liable to conceive during its progress along the fallopian tube. How long after its maturation the ovum can retain its vitality and susceptibility to the seminal influence is not known, but probably the time is short. Nevertheless, cases might be easily adduced from the works of eminent obstetricians to prove that a single insemination at any period of the interval between two menstrual periods may result in the fertilization of the female. Of such cases those only are important in our present point of view where conception has resulted from insemination shortly before the return of a period. They admit of explanation in three different ways.¹ Either the ovum has remained up till this time entire and susceptible of being influenced by the semen, a supposition which is very improbable as regards the ovum,² and is at variance with what we know of the history of the decidua or nidus prepared for the egg's further development. Or, the excitement of connection may have hastened the maturation and rupture of a graafian vesicle, a view which is in itself improbable and inconsistent with what we know results from similar circumstances in the lower animals. But it may also happen that the seminal animalcules may remain in the passages till the ovum is prepared and discharged from its vesicle. An objection at once appears to this explanation, namely, that these spermatozoa would be removed by the menstruation contemporaneous with the discharge of the ovum. When menstruation does supervene on a single recent coitus, this will probably happen unless the semen have permeated the fallopian tubes, and thus advanced beyond the scope

¹ As a good example we may refer to a case of Dr Montgomery's (*Signs, etc. of Pregnancy*, p. 258.) The last menstruation was on the 18th October. Impregnation took place on the 10th November; parturition on the 17th August. The interval between insemination and parturition was thus 280 days; between last menstruation and parturition it was about three weeks more.

² "The passage of the ovum from the ovary to the uterus occupies, M. Bischoff says, three days in the rabbit and four or five days in ruminants, and therefore, probably eight or ten days in the human female. M. Bischoff believes that the ovum escapes from the graafian follicle at the time when the menstrual discharge is about to cease, and he is of opinion, that in order to be fecundated, it must be acted on by the semen while it is in the fallopian tube." — *Baly and Kirkes's suppl. to the 2d vol. of Muller's Physiol.*, p. 58.

of the menstrual flux. But the study of such cases as recorded by authors¹ reveals this interesting fact, that under such circumstances menstruation often does not take place at all, or only very scantily; the uterine system, as it were, anticipating the conception, and preventing the failure which might result from a free discharge of blood. It is evident that such cases occurring in married women would be very liable to be considered cases of gestation protracted a month.

THE INTERVAL BETWEEN INSEMINATION AND PARTURITION

Is a period of the greatest importance in a medico-legal point of view. It is discussed by obstetric authors as the period of gestation, or as the term of the duration of pregnancy. We have already shown that the present state of our knowledge requires us to make a distinction between the date of insemination and that of conception, and it strongly appears to us that the full comprehension of the bearings of this distinction will go far to equalize the discordant views as to the term of pregnancy in the human female, and to account for many of the so-called cases of prolonged gestation. But with our present ignorance of the possible interval between insemination and conception, the exact attainment of this result is impracticable.

In attempting to settle this point, authors have resorted to numerous sources of evidence, the fallacy of which they themselves well knew. For instance, we find Dr Montgomery in his classical essay on the period of human gestation, and many other authors, quoting examples based upon the evidence of peculiar sensations felt at the moment of conception, on the last appearance of the menses, and on the time of quickening, phenomena which, however important in aiding the accoucheur to make a good guess of the day of confinement in single cases, can never be for a moment relied upon in deciding such an exact question as that before us. An excellent story illustrating the fallaciousness of such evidence is related by Dr Reid, of an expert midwife who, when examined in the celebrated Gardner Peerage case, "deposed that she had once gone ten months with child, that she was always right in her calculations, that she always fainted away at quickening, etc., so that she could not be deceived."² Some time after the trial she applied to Dr Reid, convinced on such grounds that she was seven months pregnant. But on examination there was found no pregnancy at all.

No reliance can be placed but upon accurately ascertained dates of parturition and of fruitful connection. In regard to the latter of these dates, no confidence can be placed in the statements of women living habitually with males, however truthful they may be, or

¹ Mauriceau (*Maladies des femmes grosses, obs.* 676) mentions a case interesting in this point of view, in which a woman was impregnated during the flow of menses!

² "Lancet," vol. ii. p. 78. 1850.

whatever additional evidences they exhibit. We are therefore reduced to a limited class of observations, namely, those where the pregnancy resulted from a single coitus, including those where this never took place but on a single day, and those where it was removed on both sides from other similar occasions by months, or such other period as would render it absurd to refer the parturition of a fully developed foetus to them. With those dating from a single day, we have included some dating from one of two days, but in such cases our calculations commence from the coitus of the first day only. This statistic (for the details of which we refer to the note)¹ contains 46 cases, which yield the period of 275 days as the average interval between insemination and parturition.² While 275 days was the average interval, it may be remarked that the largest number of cases at any particular day was 7 at the 274th day.

THE INTERVAL BETWEEN THE LAST MENSTRUATION AND PARTURITION

Is a period which, for obvious reasons, can be much more easily and frequently ascertained than that last under discussion. It is one, the knowledge of which is of the greatest practical importance in the every-day life of the married female, and of the obstetric practitioner, seeing that by aid of it he attempts to predict the date of the expected confinement. In the vast majority of cases, it is the only fixed point from which the calculation can be made, and hence the necessity of accurately ascertaining it, if possible.

Authors have frequently neglected the discussion of this important period, the only one available in most cases of pregnancy. They generally decide the term of pregnancy theoretically, and upon insufficient grounds, and direct that, in calculating for the

¹ Raciborski (*De la Puberté, etc.*, p. 460, etc.) relates 5 cases which come within this category. The intervals were 275, 270, 268, 273, and 274 days respectively. Montgomery in his work on the signs, etc., of pregnancy, quotes or relates 7 cases. The intervals were 281, 280, 287, 289, 288, 284, and 291 days respectively. These cases differ manifestly from those of Raciborski, but this is accounted for by observing that like some of those yet to be quoted, they are selected by Montgomery as proofs of the prolongation of pregnancy in some cases. Rigby, in his *System of Midwifery* (p. 84) mentions 3 cases. The intervals were 260, 264, and 276 days respectively. Reid, in his elaborate essay on the Duration of Pregnancy (*"Lancet,"* vol. ii., 1850), notices 25 cases. The intervals were 276, 274, 274, 275, 273, 271, 274, 274, 278, 263, 280, 264, 274, 276, 274, 276, 280, 266, 265, 266, 272, 275, 271, 287, and 293 days respectively. Besides many of those already mentioned, he adds 5 cases from the *American Journal of Medical Sciences*, which were 270, 272, 276, 284, 272 days respectively, and Mr Skey's case of 293 days. All the above are carefully selected cases, where the date of coitus taking place only during a single day, and the date of parturition, were accurately ascertained. They are in all 46 cases. The average interval is 275 days. More than two-thirds of the cases have an interval of 276 days or less.

² In France 270 days is the ordinarily accepted duration of pregnancy. See the Works of Jacquemier, Velpeau, etc.

day of confinement, this term should be told off from some day after the last menses, which day they conceive to be that on which conception most frequently or most probably takes place. For instance, Montgomery states, upon the evidence of a very few cases only, that the natural period of human gestation is 280 days, and in calculating the date of parturition, recommends this to be added to any day within a week after the last menstruation. He thus includes between the last menses and the date of parturition a period varying from 281 to 287 days—a period which, we shall show, considerably overreaches the mark. Other authors and teachers, considering that a woman is equally liable to conceive on any day between two menstrual periods, direct that the middle day of that interval be taken, and the supposed period of gestation, 280 days, added thereto—thus including the exaggerated space of 290 to 295 days between the last menstruation and parturition.

The exact decision of this interval, as of that last under discussion, can be obtained only by a reference to actual observations. Modern researches have shown that it is at the menstrual period that the ovum quits its graafian vesicle, and traverses the fallopian tube on its way to the uterus. It is in the course of this passage that it encounters the semen, and conception results. This passage occupies about three days in the rabbit, and in M. Bischoff's opinion, it occupies eight or ten days in woman. During all this time, then, the woman will be liable to conceive. It will, therefore, be expected that the interval of which we are at present speaking, will be some days, at least, longer than the last.

The statistical calculations on this subject (for details, see footnote),¹ give on an average 278 days as the interval between the last menstruation and parturition—a period less even than the 280 days which we have generally been taught in this country to be the interval between impregnation and parturition, or the duration of pregnancy.

The largest number of cases on particular days conglomerate about the 280th. Among Dr Reid's 500 instances, 283 were within the 280 days, and 217 beyond it. So far is it, then, from 280 days being the ordinary duration of pregnancy, that a woman generally does not go more than 278 days after the last menstruation is over. This period exceeds the average interval between insemi-

¹ The valuable statistics from which these results have been derived, by a tedious calculation, are published by Drs Merriman and Reid. The observations of the former were originally published in the 13th volume of the *Medico-Chirurgical Transactions*, and subsequently extended in the edition in 1838, of his work on *Difficult Parturition*. The observations of Dr Reid are to be found in the 2d volume of the "*Lancet*" for 1850. In Dr Simpson's paper on the *Duration of Human Pregnancy*, these and other allied statistics will be found carefully elaborated. See "*Monthly Journal*" for July 1853. In a statistic which I have made of the cases having sufficient details, recorded in the books of the Royal Maternity Hospital, a result comes out similar to that derived from the far more extensive records above mentioned.

nation and parturition by three days; and we may argue from this with some little probability, that conception takes place generally a few days after menstruation is finished—a view which is confirmed by numerous other physiological observations.

THE PREDICTION OF THE DAY OF CONFINEMENT

Is one of the functions ascribed to the accoucheur; and apart from the comfort and convenience to the mother attending the foreknowledge of it, she often makes its failure or success a test of the more subtle acquirements of the physician. The foregoing statistics, however, will always justify the latter in never giving a decided prognosis of the day of confinement; and if he have been guarded and careful, will afford him asylum, showing, as they do, that with certain knowledge of the termination of the last menstruation, or even of the date of a single coitus, no safe prediction can be made unless within limits so extended as to deprive it of much of its value. At the same time, there is no doubt it will always be a desideratum to know the most probable day of confinement—and this can generally be settled with some exactness.

If the date of a single connection is ascertained, which is, of course, very rarely the case, then the process of deciding the probable day of confinement simply consists in telling off 275 days (the average interval between insemination and parturition) from that date. Now, any nine consecutive calendar months include 275 days, if February is not in the number. If February is in the number, the nine calendar months include only 273 days, and the correction necessary is apparent. The whole process of calculation, then, consists in attaching the number of the day of connection to the name of the month, ninth succeeding, and adding two additional days if February is included in the interval.¹

In the vast majority of cases, the day of confinement is predicted from the date of the termination of the last menstrual period. In many cases, the calculation can be aided and corrected by comparison with former pregnancies in the same female. But when this source of information is wanting, the nearest approach to truth will be made by adding to the day of the disappearance of the menses 278 days (the average interval between the end of menstruation and parturition). The prediction will, of course, prove erroneous in a great number, nay, in the majority of cases, but it forms the nearest approximation which the mother can obtain to guide her. If a woman, then, knows the last day of her last period, she has only to tell the same day for the ninth month following (most mothers do so on their fingers, which thus form an admirable periodoscope), and

¹ Nine months do not always contain 275 or 273 days. Dating from December and July, nine months contain 274 days, and from May 276. The statements in the text, although sufficiently correct for general use, require this correction to be exact.

add three days, or if February is in the interval, five days. She thus has the most likely day of her confinement; or, perhaps better, she has the middle day of the week, on which she will probably be laid up.

I have already casually shown how this varies from the calculations ordinarily recommended by most British authors and teachers. It would be tedious to enter further on this subject. I may merely remark that a more correct plan prevails on the Continent. And from some inquiries and observations I have made in Scotland and England, I find that, popularly, a more correct calculation is extensively in use than that recommended in the schools. For instance, in Edinburgh, and some parts of Scotland, it is common to find women calculate in this way. They find the last day of being menstruated, and they hold that the same day nine months after will be the day of confinement. The celebrated Harvey's opinion on this subject was also very correct. His remarks tally with Dr Tyler Smith's ingenious views on this subject, and are deserving quotation:—"Unquestionably," says he, "the ordinary term of utero-gestation is that which we believe was kept in the womb of his mother by our Saviour Christ, of men the most perfect; counting, viz., from the festival of the Annunciation, in the month of March, to the day of the Blessed Nativity, which we celebrate in December. Prudent matrons, calculating after this rule, as long as they note the day of the month in which the catamenia usually appear, are rarely out of their reckoning; but after ten lunar months have elapsed, fall in labour, and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place."¹

PROTRACTION OF THE PERIOD OF PREGNANCY

Beyond the common or natural term is a phenomenon which most obstetricians are now willing to admit. But, although believing in its possibility, I am, at the same time, convinced that it is not so frequent an occurrence as late writers on this subject seem to think, and that most of the cases of this kind which are recorded have not sufficient evidence to support them. They are mostly based upon the signs of the disappearance of the menses, of the sympathetic phenomena of pregnancy, and of a physical examination of the uterus; all of which, it is needless to say, are abundantly liable to create misapprehensions and fallacious reasonings, and singly or combined can justify no absolute conclusion from them. One great reason for discrediting the evidence of most of the cases recorded by authors, is that we hear nothing of great development of the uterus, or of large size of the child or of the placenta in such cases, results which, to say the least, might be expected. On the contrary, we find such authors stating that in

¹ Harvey's Works. Willis's Transl., p. 529.

these so-called cases of protracted pregnancy the child is no bigger than usual, or is even smaller than ordinary. "Although in some of the cases of protracted gestation," says Dr Montgomery,¹ "the child was of enormous size, it by no means follows that it should be so in all such instances; and, in point of fact, we find it expressly mentioned in some of them, that the child was smaller than usual, as happened in one of Dr Hamilton's cases; and Foderé says, that in three instances in which gestation was evidently prolonged, the children were undersized and ill-thriven; while, on the other hand, the largest children are often produced where no extension of the term could have taken place." Dr Burns also says,² that "some causes which we cannot explain nor discover have the power of retarding the process (of gestation), the woman carrying the child longer than nine months; and the child, when born, being not larger than the average size." In further corroboration of these views, the valuable observations on cows and mares by Tessier and Spencer have been cited as showing that there was no marked coincidence of increase of size and weight of the foetus with protraction of gestation. But this reasoning from analogy between the cow and woman appears to be very much overstretched, and there are evident reasons for expecting, *a priori*, that the period of gestation in woman should be limited on the side of protraction more than in the lower animals. Of these the strongest is based on a consideration of the adaptation of the well-developed nine-month foetal head to the maternal passages, and the evils that are so well known to result from even slight disproportion between them. And unless it be supposed that pregnancy is protracted for the special behoof of small and ill-developed children, it must be admitted that an extraordinary development of the ovum is to be looked for in such cases. The acknowledged absence, then, of this extraordinary intra-uterine development is a strong evidence against the reality of the great mass of so-called cases of prolongation. On the other hand, the presence of this sign in addition to others is, in my opinion, powerfully corroborative of the supposed protraction in any instance. In illustration of this, I may state, that the best example I have met with of probable protraction occurred in a female who had borne several children, and who had previously always been correct in the calculation of the period of confinement from the cessation of menstruation. On the occasion in question she passed her calculated time four weeks, and before confinement expressed her conviction all the more strongly in consequence of my incredulity, that she had passed her time a month. The labour was more tedious than usual, in consequence of the great size of the foetal head. The child proved of very large size and advanced development. It weighed 10 lb. 4 oz. The placenta was 2 lb. in weight. Other cases similar to

¹ Signs and Symptoms of Pregnancy, p. 282.

² Principles of Midwifery, p. 199.

the above have been communicated to me by professional friends, and some are to be found recorded.

In these cases the ordinary sources of evidence were confirmed by the evidently exaggerated development of the ova, the results of these protracted pregnancies. I have lately had under my care two cases in which gestation was supposed to be prolonged, but which I reject from this category, because, although the ladies were in good health at the time of falling in the family way, yet the infants born were not at all larger than their former children. The ladies were sisters, and in each of them their calculation and mine was passed by nearly a month. The data founded upon were the cessation of menstruation and the occurrence of morning sickness. In both cases the respective nurses were residing with them for about a month before the supervention of labour.

Such cases as those of the two sisters just mentioned, and numerous other so-called cases of protraction, are easily explained by supposing simply that that menstrual flux was suppressed which should have occurred about the probable time of the fruitful intercourse; or, in other words, the decidua prepared for the ovum destined to be impregnated did not as usual throw off the bloody fluid. In these cases we must suppose either that the suppression for this one period arose from some ordinary constitutional cause, or, what is more likely, that the fruitful intercourse occurring shortly before the ordinary menstrual period anticipated and prevented it. This phenomenon we believe not to be very rare, and to be sufficient to explain away many cases of protracted gestation. In further illustration of this circumstance, we must be satisfied with referring to those cases of pregnancy after a single coitus taking place shortly before menstruation, the coitus producing, firstly, the partial or complete suppression of the menses at the approaching period, and secondly, the fertilization of the ovum discharged in coincidence with the suppressed period. Some careful observations of this sort are recorded by Raciborski and Montgomery.

The evidence of highest value in regard to this subject which we possess is founded upon cases where pregnancy resulted from a single connection. The results of these cases go far to establish the well-founded opinion of Dr Montgomery, that the cases most deserving of confidence are those in which the usual term was not exceeded by more than three or four weeks. But the cases referred to give us the interval between insemination and parturition, a period which I have elsewhere remarked requires a correction, which physiology has not yet enabled us to decide, for the possible interval between insemination and conception. In a practical and medico-legal point of view, however, the interval obtained is of great importance. In the collection of cases of this kind (see p. 225), the longest duration found is in one case where the period was 293 days. The other cases of protraction will be observed by a reference to the table.

The theory of the duration of pregnancy is still unknown. Some

authors, believing that labour comes on at the tenth menstrual period, explain the protraction by the female's having a longer menstrual interval than usual, ten of which will make up a period exceeding the usual term of pregnancy. Others have supposed that from some cause a female might miss the usual period and go on to what would have been the next menstrual period, had she not been impregnated. Others have connected it with tardy development of the foetus, with the influence of depressing emotions, etc. But all these are mere hypotheses.

In conclusion we beg to state the following propositions :—

1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

3. That the average interval between the end of menstruation and parturition is 278 days.

4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet that there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or even four weeks.

ARTICLE IV.—*Contributions to Teratology.* By A. MERCER ADAM, M.D., Dumfries, Member of the Physiological Society of Edinburgh.

It is the author's intention to lay before the profession, from time to time, a few contributions to our knowledge of foetal monstrosities and malformations. These will consist, not only of original observations and researches, but also of commentaries on the labours of other investigators; of corroborations of their opinions where he has confirmed them, and corrections of their statements where it seems to him they are fallacious. They are offered with much deference to the opinions of others, in the full consciousness that the subject is one still involved in much doubt and difficulty.

NO. I. OF THE FREQUENCY OF THE OCCURRENCE OF MALFORMATIONS IN GENERAL, AND OF CERTAIN VARIETIES IN PARTICULAR.

Happily for the human race monstrous births are comparatively rare. We are not, as yet, in possession of sufficient data by which to calculate with much exactitude the frequency of their occurrence; for many, doubtless, happen which are either unrecorded through

negligence, or are concealed from motives of false shame; and many must escape observation altogether, especially in cases of abortion and premature birth. St Hilaire, in 1836, reckoned the average occurrence of monstrosities in Paris to be 1 in every 3000; and, estimating the annual number of births in the whole of France at one million, he calculated that in about 3300 cases the infants were malformed. According to Riecke 230,939 births occurred in Würtemberg in four years, 50 of which were recorded as monstrous; i.e., in the proportion of 1 in every 4618. The records of maternity hospitals ought to afford us more accurate and valuable statistical information than the returns of Registrars-General, which are well known to be very defective and inaccurate in a medical point of view. Let us, therefore, examine those of the Imperial Lying-in-Hospital at Vienna (the *Gebäranstalt*), and the *Maternité* at Paris. Between the years 1832 and 1840, 23,222 births occurred in the former institution; and of these 68 are reported as being monstrous, or 1 in every 341 children.¹ Among 23,293 infants born at the *Maternité* at Paris, Chaussier found 132 malformed in different ways; that is, in the ratio of 1 in every 176. Now, it will be seen that considerable discrepancy exists among these statistics. The computations of St Hilaire and Riecke are evidently too low; but it is worthy of remark that their statistics must necessarily be more liable to fallacies than those of maternity hospitals, where the particulars of every birth are noted with accuracy and care, and moreover they probably include merely the graver types, and not the milder and less conspicuous forms of malformations. Hence we are of opinion that the statistics we have cited of the lying-in-hospitals of Paris and Vienna are most to be depended on. If, therefore, we take the mean ratio of both hospitals, we shall find that *malformed children occur in the average proportion of 1 in every 232 births.*

This computation of mine *may* be fallacious; but it seems to me as near an approximation to the truth as, in the present state of our knowledge, can be obtained. It may be asked with much reason, why it is that our statistics on this point of obstetrics are so very scanty and defective? We apprehend that this is to be accounted for, not by any carelessness on the part of accoucheurs, or by any deficiency of their opportunities of observations, but solely by their defective knowledge of teratology, and by the want of a proper understanding among them as to what is, and what is not, a malformed or monstrous child. As regards this latter cause, however, they are not much to be blamed, for certainly in no department of medicine have we greater need of an accurate and distinctive nomenclature. The word "monster" ought to be entirely abolished from medical literature as indefinite, confusing, and unscientific. It arose in those dark ages, when *lusus naturæ* were regarded as omens of evil import, indicative of the vengeance of an offended Deity, or grimly foreshadowing some impending

¹ Quoted from Mr Wilde's work on "Austria."

calamity.¹ The word thus coined unfortunately continued current in after-periods of greater enlightenment, creating no small amount of confusion and misunderstanding. What do we understand by the term "Monster" in these latter days? "We call monsters what things soever are brought forth contrary to the common decree and order of nature." So said worthy old Ambrose Paré; but, according to this definition, the slightest deviations from the specific type, such as the abnormal course of an artery, or the unusual configuration of a limb, might be ranked as monstrosities. Now-a-days this word "monster" (*monstrum*, *Latin*; *Τέρας*, *Greek*; *missgeburt*, *German*; *monstre*, *French*) seems only to be applied to the graver cases of anomalous formations as *Acephalia*, *Anencephalia*, *diplogenesis*, etc.; and the term "malformation" (*lusus naturæ*, *Latin*; *naturspiel* or *missbildung*, *German*) to such *vitia conformationis* as are less defective and conspicuous, e.g., harelip, supernumerary extremities, imperforate anus, etc. Now all structural abnormalities ought to be called generically "malformations" (for a child with harelip or talipes is as truly malformed as one with anencephalia); and these again should be individualised under their own proper and specific teratological designations. The word "monster," having so long held a place in our language, cannot well be dispensed with in treating of congenital deformities, but I think that medical men would do well to avoid using a term so very unscientific and confusing. And until obstetricians, paying a little more attention to teratology, carefully record the minor as well as the graver types of malformations, we can never hope to obtain correct statistical information on this subject.

As regards the frequency of certain special varieties of malformations, I may remark that Meckel, long ago, pointed out that those *per defectum* were of more frequent occurrence than those *per excessum*, and that among the former class those defective conditions of the cranium, called *Acephalia* and *Anencephalia*, formed a considerable number. These observations have been confirmed by subsequent researches; and the peculiar types specified have been found to be more common than is generally supposed. Among malformations by excess of development, we find cases of *diplogenesis* (or double-monstrosity) to be very common. Among those *per excessum* the malformation occurs more frequently on the upper than on the under half of the body; thus we oftener find two heads joined to one body than two bodies attached to one head. But, as there is some exception to every general rule, supernumerary extremities and consolidations of parts generally occur in the lower half of the

¹ Some have considered the word "monster" to be derived from the Latin verb *monere*. I think, however, that the verb *monstrare* is more probably its radical. The following words of Cicero seem to warrant this etymological hypothesis:—"Monstra, ostenta, portenta, prodigia appellantur quoniam monstrant, ostendunt, portendunt, et prædicunt."—(*De Divinatione*, Lib. i.) And Isidore von Savilla defines the word thus:—"Quæ aliquid futurum monstrando homines monent."—(*De Etymologiis*, Lib. ii.)

body. Otto,¹ Isenflamm,² Courmette,³ and other authors assert that the *left* side of the body is weaker, and consequently more prone to disease than the right, and that on this side also malformations most frequently occur. This statement I have frequently tried, but in vain, to confirm, for in the majority of cases where the deformity was unilateral I found it on the *right* side.

NO. II. THE RELATIVE FREQUENCY OF MALFORMATIONS IN THE TWO SEXES.

Malformations occur much more frequently in the female than in the male sex. Haller, Meckel,⁴ Tiedemann, Otto, and Burdach have all observed this fact; and I shall place here, in a tabular form, the results of some of their investigations on the subject:—

	Females.	Males.	Authority.
In 42 cases of Diplogenesi there were	30	9 ⁵	(Haller)
In 80 do. do. do.	60	20	(Meckel)
In 46 do. of Malformations do.	31	15	(Tiedemann)
In 473 do. do. do.	270	203	(Otto)

In diplogenesi especially the female sex is found to predominate; thus, in 142 cases Otto found only 42 males, and in the 122 cases cited above there were only 29 males. Burdach⁶ is very near the truth when he says that in these cases males occur in the ratio of 1 to every 3·2 females; for according to the above statistics the males form only 27 per cent. of the whole, or, in other words, occur in the proportion of 1 to every 3·70 of the other sex. It has, however, been alleged that in some varieties of diplogenesi, the twin-fœtuses are generally of the male sex; but the statistics I have quoted militate against the truth of this assertion. In certain rare species of malformations,—as in those caused by parasitical growths,—males are most frequently found; occurring, according to Burdach,⁷ in the ratio of 1 male to 0·40 of the other sex.

It may not, perhaps, be out of place to mention here that in malformations by duplicity the children are generally of the same sex, being either two males or two females. Although this may be, in some measure, due to the symmetrical *lex proprietatis* by which even abnormalities are governed, we shall find, on analysing the statistics of common twin-births, that in most cases the children are of the same sex. In the kingdom of Prussia, during a period of twenty-three years (from 1826 to 1848), there were recorded

¹ Pathol. Anat. Band i.

² Beiträge für die Zergliederungskunst. Band i. Part

³ Journal de Médecine. Part 85.

⁴ Sex est generalis, paucis tantum exceptionibus subjecta, monstra femina longè sæpius occurrere masculinis."—MECKEL, *Comment. de Duplic Monstros*, p. 14.

⁵ Of the remaining three, two were hermaphroditic, and in one the sex was unascertainable.—HALLER, *Opusc. Anatom.* Götting. 1751. P. 176.

⁶ Physiologie. Vol. i. p. 379.

⁷ Loc. cit.

141,715 twin births. The particulars of these, as regards sex, are thus tabulated by Dr Meckel :¹—

STATISTICS OF 141,715 TWIN BIRTHS.

Same Sex.		Different Sexes.	
2 Boys.	2 Girls.	1 Boy.	1 Girl.
47,074	43,413	51,228	
Total, 90,487		51,228	

From the admirable statistics of Dr Collins, I have drawn up the following table :—

STATISTICS OF 240 CASES OF TWIN BIRTHS.

Same Sex.		Dissimilar Sexes.	
2 Boys.	2 Girls.	1 Boy.	1 Girl.
73	67	100	
Total, 140		100	

These tables sufficiently show the resemblance between normal twins and malformations by duplicity in regard to similarity of sex ;² but there is this point of difference to be noted, that while in the latter *females* greatly predominate when the children are of the same sex ; in the former, *males* are most commonly met with in the same circumstances.

How are we to explain this preponderance of the female sex among malformed children ? Meckel accounted for it by supposing that, in early intra-uterine life, the foetus was, in all cases, of the feminine sex, and that defective development of the genitals was the cause not only of there being so many female monstrosities, but of there being also so many feminine hermaphroditisms.

¹ *Vide* Müller's Archives. Jahrgang, 1850. p. 236.

² I may remark that in triplets and quadruplets, the proportion of equal sexes is not so great. Thus Mayer found that of 1594 triplets, 719 were of the same, and 875 of dissimilar sexes ; and of 35 quadruplets 11 were of the same, and 24 of different sexes.

Although Burdach and others justly opposed Meckel's erroneous doctrine as to the primitive sex of the foetus, Andral and many authors, accepted this hypothesis as explanatory of the facts alluded to; and probably they inclined to this opinion on account of the assertions of Soemmering, Ruisch, and Antenrieth, that abortions and early embryos were generally of the female sex. I am of opinion that Meckel, and the physiologists who adopted his views, must have mistaken the *sinus urogenitalis*, in the embryo they examined, for the vulva. Be this as it may, it is now well ascertained that their assumptions are incorrect; that the external genitals are primarily the same in both sexes (as has been shown by Tiedemann, Müller,¹ etc.) appearing first in the form of a small wart-like protuberance in front of the sinus urogenitalis; and that it is only in the fourth month that the sex is clearly distinguishable. In what manner then, are we to account for the fact? Is the female foetus more weakly, and consequently more susceptible to disease and malformation than the male? I do not think we are warranted in saying so. The only hypothesis which seems to me at all tenable, is *not* that the sex induces the malformation, but that the *malformation determines the sex*. I am of opinion that it may occur thus:—A higher degree of formative power is probably requisite for the conversion of the embryo into a male than into a female child, for the margins of the sinus urogenitalis need to be developed more largely, and to be united more extensively in the constitution of the male perineum and scrotum. If, therefore, from the fifth to the fourteenth or sixteenth week, anything happens to disturb or arrest the development of the embryo, its *autotrophy*, or inherent formative power, becomes so weakened that it cannot possibly accomplish the perfect closure of the primitive cloacal fissure, and the foetus consequently becomes a female. This hypothesis may not stand the test of future researches,—I merely offer it because it seems to me capable of explaining the great frequency of females among malformed children.

NO. III. THE RELATIONS BETWEEN TERATOLOGY AND ZOOLOGY.

Transcendental anatomy has shown us that throughout the whole of the animal kingdom *unity of organisation* is the great principle governing the development of the species. After the model of a great archetype or types² all animals are fashioned; and although

¹ Müller's Physiology, Trans. by Baly, vol. ii. p. 1639.

² *One* type according to Geoffrey St Hilaire: *four* according to Cuvier, etc. The former idea seems at first sight absurd; for how can an invertebrate animal be formed after the model of one having a complex osseous skeleton? Have a polype and a man the same archetype? But both theories are fundamentally correct. Cuvier looked merely to characteristic generic distinctions; Geoffrey's philosophy is more searching and subtle, for he passes over all the superficial differences constantly presented by specific classes, and looks merely to one grand *principle*, in accordance with which they were organised. His doctrine, to quote an able writer in the Westminster Review, is, that "unity of composition is not to be sought in the *form* of animals, nor in their assem-

their various species present many distinct generic characters and great structural diversities, the unity of organic formation which has presided over their development gives rise to many very striking and unexpected analogies, existing even between creatures far removed from each other in the scale of being. In the various stages of their development the higher animals present transitory *organic* resemblances, more or less striking, to others which are lower; and the inferior animals again—when regarded in the light of a primitive archetype—are but the permanent embryos of species which are higher. A remembrance of these facts will enable us all the better to understand teratological phenomena in general; and more especially those cases where one species presents, in its malformed condition, a close resemblance or analogy to the normal and perfect development of another.

Bearing in mind this unity of organic type, and symmetry of design which pervades the animal kingdom, we need not wonder that malformations occur among all living species. All organic processes are liable to mutation and disturbance; and all development, animate or inanimate, is subject to mysterious deviations and abnormalities.¹

I cannot agree with those writers who assert that, as a general rule, the higher animals are in the scale of being, the more frequent are malformations among them. In the same species, however, we know that their frequency is increased by domestication. St Hilaire has stated that about three-fourths of the malformations in the animal kingdom occur among mammalia, and the remainder among birds.² The same author likewise avers that they happen very rarely among fishes, and seldom or never among the articulata, radiata, and mollusca. These observations are obviously incorrect, for among the lower animals structural abnormalities and imperfections occur very frequently,—probably oftener than among mammals. Fishes are very often malformed. I have seen a cyclopic cod, and have frequently examined fishes in which the dorsal fins were deficient or redundant, or in which the tail was bi-pinnate. Gold-fish and carp are very often thus imperfect; shortening or arrested development of the face is said by Otto to be very common among the latter species. Supernumerary toes I have often noticed among frogs; and abnormalities in the number of the thumbs on the claws of crabs are familiar to all.³ Among snails also anomalies in

blage of parts, but in the *progressive repetition and complication of parts issuing from a common centre*, and formed according to a common plan or process."

¹ "Rien n'est monstrueux dans la nature, parce que les lois qui la régissent émanent d'une intelligence supérieure qui met dans toutes ses œuvres la même sagesse et la même harmonie."—*Breschet*.

² Hist. des Anom. iii., p. 354.

³ Otto says:—"There are *a few specimens* of three thumbs on the claw of a crab." Now, with all deference to so great an authority, I think I can pronounce such cases to be by no means rare. I may mention that in the Anatomical Museum of the Edinburgh University there are many interesting specimens of malformations among fishes and crustacea.

the windings and shape of their calcareous volutes are far from being uncommon. Malformations are met with among the Reptilia; thus Mitchell relates cases of three rattlesnakes, all of which were bi-cephalous.¹

Among Mammalia we find malformations occurring more frequently in certain species than in others. Thus cats are more subject to them than dogs; they occur oftener among oxen than among sheep, and among sheep and oxen than among swine, goats, and horses. Moreover, certain varieties are common to certain species. Diplogenes and malformations of the cranium occur very frequently in the human species; the former is common among cats, which are multiparous, and also among oxen, which are properly uniparous. Cyclopia is most common among swine, and acrania among birds. I have seen diplogenes occurring in the domestic fowl by the fusion of a double yolk; and the shells of two eggs may be united together.²

Many of the malformations of the human embryo are analogous to the normal conditions of some of the lower animals,—arrest or disturbance of its development having occurred at a period when the foetus presented some of the specific transitory organic similarities to which I have alluded. The shortened limbs and deficient arms of the malformations called *phocomeles* resemble the condition of the extremities in seals and certain *cetacea*; a duplex uterus, a cloacal fissure, and a small-sized brain approximate the deformed embryo to the characters of the *rodentia*; and diaphragmatic incompetency is analogous to the normal organisation of oviparous animals. These resemblances of the malformed foetus to some of the lower animals have been, by the vulgar, attributed to the effects of some impressions made on the maternal imagination during pregnancy (*Das Versehen* of the Germans), rather than to accidental and mysterious circumstances arresting the growth of the embryo, and thereby giving permanence to certain of its rudimentary conditions. Thus, it will be seen, to quote the words of Serres, that “L’ordre est dans le desordre;” and that even malformations are governed by certain regular, definite, and symmetrical laws.

(To be continued.)

ARTICLE V.—*Case of Cholera, in which the Blood was remarkably altered.* By JAMES M. COWAN, M.D., Edinburgh.

SINCE the researches of Bennett and Virchow, on the structural alterations of the blood, every observation connected with that subject has acquired a degree of interest hitherto unknown. Hence

¹ American Journal of Science and Art. 1825. P. 48.

² A beautiful specimen of this exists in Professor Simpson's Obstetrical Museum in the University.

the following case, though an isolated one, may not be undeserving attention :—

Mrs G., æt. 51, wife of a shoemaker, living in No. 48, Candlemaker Row, was admitted into the Edinburgh Cholera Hospital at 1.5 p.m. of the 8th of February 1854.

Immediately thereafter I found her pulseless at the wrist, complaining of cramps in the calves of both legs, recurring every four or five minutes, and when present, giving rise to great pain. She had also, almost incessant purging and vomiting. The stools presented the usual appearances of rice-water discharges, and the matter vomited seemed to contain some bile. The surface of the body generally felt cold, the skin doughy, the tongue and breath were also cold, the former being coated with a thick brown fur. She had a peculiar sunken expression; countenance of a deep sallow tint; eyes much depressed in the orbits, and surrounded by a very pronounced dark areola. The voice was very weak, almost whispering.

At 10 o'clock p.m. of same day, the heat of surface is reported as good; the pulse barely perceptible at the wrists; occasional vomiting; excessive thirst; purging almost incessant. She has made no water. It is doubtful when urine was last passed—according to her own account, not since the day of seizure, four days before admission.

Feb. 9th, 10 A.M.—Had slept during the night; vomiting is abated; purging continues much the same. Pulse small and thready, but quite perceptible; temperature good; thirst not so urgent. 2 p.m.—Pulse evidently rising in strength. I drew off by catheter about 3ij. of turbid dark-coloured urine, which was found highly albuminous, and presented the same reaction on the addition of nitric acid, as Dr Parkes has pointed out in the urine of cholera. 11 p.m.—Pulse is described as of natural strength, 96 per minute. Within the last two and a half hours has had three stools, chiefly fluid of a dark green colour, and containing a very small proportion of solid flocculent matter. Other symptoms have disappeared.

Feb. 10th, 2.30 A.M.—I was sent for very hurriedly, being told a great change had almost instantaneously come over this patient. On arriving at her bedside, I found her face of a death-like paleness; respiration very slow and tranquil; pulse quite imperceptible, the pulsations of the heart even being scarcely recognised by the hand applied over the pericardial region. The extremities were cold; she had no vomiting. After some doses of diffusible stimulants and dry frictions steadily applied to the chest and extremities, she somewhat rallied, and at the end of an hour, heat having returned to the extremities, the pulse being quite perceptible at the wrists, and the expression of extreme debility partially gone, I left her. Another half hour had scarcely elapsed when the nurse returned saying, "the patient was dying." I again found the same alarming phenomena as before. In addition, there were now an occasional rattle in the chest, a glazed appearance of the corneæ, and the eye-balls were turned upwards. She expressed a fear of impending dissolution. Through the œsophagus tube (for swallowing could be effected but with great difficulty) I poured some brandy and beef-tea. It, however, had no effect in rousing the depressed energies of the system, and at 4 A.M. of the 10th she quietly sank.

Treatment.—This consisted of the hot-bath soon after admission, frictions with anodyne liniments, to allay cramp, and one of Dr Steven's saline powders every half hour; the first six were rejected, the rest were retained. She had also occasionally turpentine and saline enemata.

Preceding history.—The most of the following particulars I learned from her daughter. Mrs G. has had no children since her 28th year. During the last six years she has led a very dissipated life, being in the habit of spending the greater portion of her husband's wages in procuring whisky; she has enjoyed

but very scanty fare. Her daughter described some kind of fits to which her mother, when very intoxicated, was subject; the description was rather vague, but somewhat analogous to that of epilepsy. She never had attacks of syncope. Her legs and ankles sometimes became swollen to such an extent as, to allow of the entrance of her feet, the shoes had to be cut. For the last eighteen months she has been subject to attacks of diarrhoea and nausea, being so affected at least twice or thrice every month. She never complained of palpitation at the heart. Eight days before admission into the hospital, she and her husband, both at the time suffering from diarrhoea, went down to Leith to attend a son-in-law, who, they were told, lay ill of cholera. They remained by their son-in-law's bed four days and nights, at the end of which time, convalescence appearing established, they came home to Edinburgh. Soon after their arrival the husband was seized with the symptoms of severe Asiatic cholera; and next day Mrs G. was attacked with cramps, vomiting, purging, etc. In the flat at Leith above the son-in-law's house, two men died of cholera the week previous to the illness of their relation. This is authenticated by a medical gentleman of Leith.

Sectio cadaveris.—Nine hours after death; rigor mortis inconsiderable; peculiar sallow tinge of body; little posterior lividity; one and a half inches of fat in the abdominal parietes.

Head.—Considerable congestion of posterior portions of hemisphere of brain; considerable amount of sub-arachnoid effusion not coagulated.

Chest.—Pleuræ moist; lungs highly emphysematous, and very anæmic; both together weighed not more than 1 lb.; bronchi of both contained some frothy mucus; their lining membrane appeared of a rosy tint.

Pericardium contained a little fluid quite clear; one small spot of ecchymosis not exceeding the size of a pin's head was seen at the base of the right ventricle.

Right side of heart fully distended; left comparatively empty; cavities contained dark thick blood, semi-coagulated; some of the coagula were partially decolorized, and extended into the larger vessels; valves healthy; heart when emptied seemed small, especially when contrasted with the size and condition of the patient; its fibre was not fatty.

Abdomen.—Liver weighed 2 lb. 11 oz.; it was not at all congested, of a rather fair colour, and of softer consistence than normally; gall bladder contained about two ounces of a thin green bile; spleen weighed 4½ oz.; it appeared of normal size, but felt softer than in a state of health; on section it presented a homogeneous mass, of a colour which can only be described as deep brown black, with a shade of vermilion (mahogany colour); not a trace of the healthy structure of the spleen; no malpighian bodies could be observed; stomach and intestines healthy; mucous membrane very pale, nowhere injected, except a small portion of the lower end of the descending colon (consequence of enemata).

Kidneys together weighed 9¼ oz.; right kidney was congested; external surface slightly irregular, presented portions of atrophied texture and a few small cysts; tubular character of cortical substance not very distinct; the left, on section, showed some cysts; consistence firm; otherwise healthy. Urinary bladder firmly contracted; mucous membrane injected and thrown into rugæ; external surface of cervix uteri showed a rough, irregular ulcer, of an oval shape, extending nearly all round the os; no thickening of the adjacent tissues; on pressure some gelatinous mucus escaped from the os; lining membrane of uterus, near fundus, injected; ovaries normal.

Microscopic Examination.—On examining a drop of the blood under a power of 240 linear diameters, the red corpuscles appeared very faint; seemed to contain little, if any, colouring matter, and did not present, as is their wont, the characteristic appearance "of rouleaux's of coin." The white corpuscles seemed to bear a normal proportion to the red. A few granules were seen here and there in the field of the microscope. In addition to these, however,

were numerous other bodies, which could not fail to attract notice—generally circular in shape; some, however, oviform; a few caudate, and composed of a well defined membrane, not at all puckered, enclosing one or two distinct granules; these were very small, quite round in form, and possessed of clear centres; they appeared to be attached in general to one of the extremities of the circumference of the corpuscle; in some cases it was difficult to say whether they were adherent to its interior or exterior. When observed in motion some of these bodies appeared as if flattened on either side. They seemed to bear the proportion of one to seven or eight of the red blood corpuscles. The long diameter of the corpuscles measured about the 100th of a millimetre; their transverse 150th of a millimetre.



The corpuscles observed in the blood. The fainter bodies, which are the normal corpuscles partially dissolved, have been drawn by the wood-cutter rather too small—240 diam. lin.

On the addition of acetic acid they gradually swelled up, their external wall becoming fainter and fainter, until at last it appeared to rupture, and the included granules were set free. The acid, with the exception of rendering them more faint, had no other effect. A strong solution of muriate of soda diminished the size of the corpuscles, rendering them more distinct. On adding aq. potassæ the corpuscles increased in size, their external wall becoming fainter. In the tissue of the liver the same bodies were seen, identical in appearance and behaviour under chemical reagents; the true hepatic cells were healthy. In the tissue of the heart, amid the muscular fibrillæ, similar bodies were observed. The same bodies, with the exception of a few true spleen corpuscles, were seen to constitute the whole mass of the spleen. In this instance they appeared rather larger in size than in the blood, and presented a much greater diversity of shape—the caudate being the most predominant form at the extremity of whose tail-like projection, a nucleus could be observed as if pushing the cell wall before it.

Remarks.—It was observed at the time of the examination of the body of this woman, “there was not a single morbid appearance which could be held as accounting for the cause of death.” And it cannot fail to attract notice that although we had all the most characteristic phenomena of true Asiatic cholera developed during the lifetime of the patient, we did not discover after death one of the usual morbid appearances, such as they are, met with in the bodies of individuals, the subjects of malignant cholera.

Various questions of great interest arise from the study of the case, which, I regret to say, are difficult of solution. In the first place, what led to these bodies? How is their presence to be explained?

Only three hypotheses are admissible—1st, This diseased state existed antecedent to the attack of the fatal disease; 2dly, It was the result of the morbid influence of the cholera itself; 3dly, It was consequent on the treatment employed. Now, it seems to me the first of these is the most probable explanation. This becomes apparent when we pass in review the preceding history of the patient—her delicate state of health, irregular habits of life, liability to diarrhoea and sickness; when we consider the peculiar appearance presented by the spleen, quite unusual in Asiatic cholera, and the mode

of death, which also is comparatively rare, we can come to no other conclusion, but that these bodies, found in the blood and tissues, unconnected with the essential disease "cholera," nevertheless had a powerful influence on the disastrous issue of the case. The other hypotheses, I regret to say, from want of data, I can neither support nor refute. They form proper subjects of inquiry.

In the second place, what are these bodies? That they are, in some mode or another, connected with the blood is incontestible, from their being found in that fluid, and so universally amid the other tissues of the body, as can be explained on no other hypothesis. Are they, then, a modification of the red corpuscle? Are they an early or retrograde stage in the development or decay of the white blood corpuscle? Or have they any other relation to these bodies? I regret to say I have no answer to give these questions. All I can do in the meantime is merely to chronicle the fact of such bodies as described having been found in the blood and tissues of a woman who died presenting the true Asiatic type of cholera, and concurrent with a peculiar state of the spleen.

ARTICLE VI.—*Note on the Treatment of Diabetes Mellitus by Rennet.* By H. FEARNSIDE, M.B., London, Physician to the Preston Dispensary.

IN the number of the MONTHLY JOURNAL for January 1853, there is an interesting article by Dr Gray of Glasgow, on the use of rennet in the treatment of cases of diabetes mellitus. In two instances which are detailed, the saccharine impregnation of the urine was removed, and the patients regained their former health; and in a third, reported whilst under treatment, a great improvement appears to have been effected. Meeting with an example of the disease, shortly after the perusal of Dr Gray's paper, I put his treatment in practice, and the result was so encouraging, that although averse to the publication of isolated cases, I think no apology is needed for placing it before the profession. For, at all events, be the precise merits of the treatment what they may, it tends to confirm the opinions of Bouchardas, Schutzenberger, and other continental pathologists, that the complaint is not so inevitably hopeless and incurable as is generally supposed in this country. As my object at present is simply to narrate a fact, and not to speculate on the nature of the disease, or criticise the opinions of others, I proceed without farther preface to give an outline of the case in question.

Mrs H., a tall, spare woman, aged 55 years, had suffered from bad health for some time, without being able to indicate the existence of any special ailment. She had lost strength and flesh, and for some months before she fell under my notice, her debility had increased so much that it was with difficulty that she attended to her ordinary domestic duties. For a considerable time she had remarked that the quantity of urine passed was excessive, and she had been

harassed by constant thirst. When I first saw her the expression of the countenance was haggard and anxious; the skin was hot and dry; the pulse quick; the tongue was loaded with a yellow fur; she complained of inodorous eructations, heart-burn and flatulence; the bowels were confined; the thirst inordinate. There was great muscular weakness, and severe pains in the back and limbs. The quantity of urine passed in twenty-four hours was five quarts; it was acid; specific gravity 1046, and gave evidence on the application of the potash and copper tests of containing sugar.

After the use of some gentle aperient medicine, the diluted mineral and hydrocyanic acids were prescribed, and with more or less regularity, were taken for some months. The diet was strictly limited to butcher-meat, fish, eggs, milk, and bran bread. Fresh vegetables, as cabbages, were taken occasionally. Brandy and water was allowed as a beverage. A dose of rennet was taken after each meal.

A fortnight after the adoption of this plan, the patient became so conscious of its good effects, that notwithstanding the vigorous exercise of self-denial which it required, no further exhortations from me were needed to ensure its steady employment. The digestion improved, the thirst subsided; the quantity of urine passed in twenty-four hours fell from five quarts to two quarts, and eventually to three pints, and its specific gravity descended in three months from 1046 to 1020. It has now for some months been free from sugar, although the patient has cautiously and gradually returned to her ordinary mode of life. She has recovered in a considerable degree her strength, but remains spare and thin.

Part Second.

REVIEWS.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By JAMES PAGET, F.R.S., lately Professor of Anatomy and Surgery to the College, etc. Vol. II. Tumours. London. 1853. 8vo. Pp. 637.

Traité des Maladies du sein de la region Mammaire. Par A. VELPEAU, Membre de l'Institut; Professeur à la Faculté de Médecine de Paris. Paris. 1854. 8vo. Pp. 727.

Treatise on Diseases of the Breast and of the Mammary region. By A. VELPEAU, Member of the Institute, Professor at the Faculty of Medicine, Paris, etc. Paris. 1854. 8vo. Pp. 727. Eight coloured plates.

On the Modern Philosophy of Cancer. By ROBERT DRUITT, Member of the Royal College of Physicians, London, etc. Association Journal, January 13, 1854.

WE now resume our notice of Mr Paget's Lectures, the second

volume of which is complete in itself, and treats of tumours. We were deep in its consideration when the work of M. Velpeau arrived, which, although it describes only diseases of the breast, necessarily speaks much of tumours, and discusses many of the topics dwelt on by Mr Paget. We have thought it would interest our readers to contrast the opinions of two surgeons, moving in different spheres, having but one object in view when writing on the same subject; and we shall find it useful, in addition, to pay some attention to the remarks of Dr Druitt. We may state at once our opinion of the merits of these three contributions to the pathology of tumours, and then endeavour to gather from them what is the present state of our knowledge on this important subject, and what we consider ought to be done to advance our scientific and practical acquaintance with morbid growths generally.

Mr Paget's account of tumours is characterised by the same careful description of facts, the same admirable knowledge of histology, physiology, and pathology that pervades all his writings. We will undertake to say, that every one who has carefully investigated the subject of tumours, will at once recognise and highly estimate the accuracy and value of Mr Paget's labours, however he may differ from him as to certain generalizations, or as to the propriety of artificial divisions. Tumours he separates into innocent and malignant, observing that this distinction is probably one not of mere visible structure, but of origin and vital properties; it is, therefore, less falsely expressed by terms implying quality of nature, than by such as refer to structure alone.—(p. 10.) It is not, he thinks, in the likeness or in the unlikeness to the natural tissues that we can express the true nature of tumours; it is not enough to consider their anatomy; their physiology also must be studied; as dead masses, or as growths achieved, they may be called like or unlike the rest of a part, but as things growing, they are all unlike it.—(p. 5.) As to the characters which distinguish malignant from innocent tumours, we have nothing definite, for every one that is given is immediately negatived, and shown not to apply. Thus we are told that malignant tumours have a peculiar structure, but immediately informed that this rule is only general, as malignancy may be sometimes observed in tumours that have apparently the same structure as innocent ones. Hence the idea which we should attach to malignancy it is difficult to arrive at, unless it be recurrence after operations. Fibrous tumours, we are told, return after removal, ulcerate, infect neighbouring parts, and appear in internal organs. These, says Mr Paget, must be regarded as malignant growths, though in structure resembling innocent tumours and natural tissues. But why must they be so considered, unless their recurrence and appearance in internal organs be the reason? and yet we are nowhere distinctly told that this is what constitutes malignancy. Other five characters, equally vague, and which occur occasionally in innocent tumours, are given as distinctive of malignant ones; but what do they avail when it is acknow-

ledged that they are only general, and can therefore be of little use in doubtful special cases. On this point, however, it is only just that Mr Paget should speak for himself:—

“ Now the distinctive value of each of these characteristics of malignant disease may be depreciated : indeed, I have myself lowered it, by showing that each of them may be absent in tumours having all the other features of malignancy, and that certain of them may be observed occasionally in tumours that in other respects appear non-malignant. But objections against each character separated from the rest are of little weight against the total value of all these characters of malignancy, or of a majority of them, concurrent in one case. Similar objections might be made against even the classifications of natural history : and none but such as are disposed to cavil at all nosology, could fail, in watching a series of cases of tumours through many years, to observe that the great majority of them could be classed according as, in their course, they did or did not present the characters that I have enumerated. Some cases would be found in which one or two of the signs might be wanting, or, if I may so speak, misplaced ; but, putting these aside, as exceptions to be regulated by future inquiry, and looking broadly at the whole subject, no one could doubt that this division of tumours into innocent and malignant may be justly made, and that the outward marks by which they are discriminated are expressions of real differences in their properties and import.”—Pp. 17, 18.

All this may be very true, but still these outward marks are in many cases totally insufficient for the purpose, and it must be confessed that such a division of tumours leaves much to be desired.

When, now, we come to inquire what are those tumours which, in Mr Paget's opinion, should be considered as innocent on the one hand, and malignant on the other, we find them enumerated as follows. Among the former are placed, 1st, cystic tumours, simple or compound, and their varieties ; 2d, fatty ; and, 3d, fibro-cellular tumours. These latter growths are the soft polypi ; and if the classification be based on structure, the name fibro-cellular is correct, although cancers are also fibro-cellular. 6. Painful subcutaneous tumours. Here the classification rests upon the symptom of pain. 7. Fibrous tumours. 8. Malignant fibrous tumours, so called because they return after excision, and in one case between twenty and thirty fibrous masses were found in the lung. We cannot resist the persuasion that these so-called malignant fibrous growths were examples of hard cancer. 9. Recurring fibroid tumours, a term first applied by Mr Syme to express the return of a fibrous tumour after excision. In both the last kind of tumours, return after operation is essential to constitute the variety, and we have no means previously of detecting the peculiarity. In the first kind there is a further tendency to occur in internal organs, and *hence*, we presume, it is malignant. 10. Fibro-nucleated tumours, so called by Dr Hughes Bennett, after their structure. Some of these are recurrent. 11. Cartilaginous tumours. Some of these are also recurrent. 12. Myeloid, or marrow-like tumours, comprehending some of those described by Lebert, as fibro-plastic. Here the classification depends on resemblance to well known substances. Of this form of tumour a very remarkable example is given, in the case of a boy who had the

cranial bones and brain attacked. The growth to the naked eye, in its mode of development, its infiltration of tissues, its involving of important parts, its apparent dissimilarity from any natural structures, was identical with cancer. And yet it certainly, says Mr Paget, was not cancer; the microscopic elements were like those of natural parts; not a lymphatic or any other organ was affected by similar disease, and death seemed to be due solely to the local effects of the growth.—(p. 225.) Here, then, structure constitutes the grounds of distinction, and although it attacked two such different textures as bone and brain, it is not malignant. 13. Osseous tumours. 14. Glandular tumours. These are important hypertrophies of gland tissue, though Mr Paget objects to calling them such. 15. Erectile or vascular tumours.

Such is the arrangement of innocent tumours, in which we can detect nothing definite, nothing systematic. The ground of distinction is most variable, now structural, now symptomatic, here framed according to vague resemblances, there determined by natural progress, recurrence, and propagation to internal textures. Yet, in this evident confusion, we are far from denying but what the author, after all, indicates the present state of our knowledge better than those who make clear distinctions, and point out broad lines of demarcation.

The malignant tumours, according to Mr Paget, are, 1st, Scirrhus or hard cancer; 2d, Medullary cancer; 3d, Epithelial; 4th, Melanoid; 5th, Hæmatoid; 6th, Osteoid; 7th Villous; and 8th, Colloid cancers. Hence the division is founded on physical properties, the presence of black pigment, blood, or colloid matter, on form, or its occurrence in bone. Why cancer of bone should be different from cancer in other texture, is not explained; but under this term osteoid cancer, that is cancer resembling bone, is meant the fibrous osteo-sarcoma, and foliated exostosis of surgical writers.

On the whole, we have been exceedingly puzzled with Mr Paget's arrangement of tumours. It cannot be said to simplify those which previously prevailed, and its admixture of recurring and malignant tumours with innocent ones, and the increased number of cancers, cannot, scientifically speaking, be said to have advanced our knowledge of the subject. His facts and observations are most valuable; his generalizations and groupings are, in our opinion, faulty.

The work of M. Velpeau is, in the present state of our knowledge concerning morbid growths, one of the most valuable that could have been made to pathology. It is the contribution of an eminent practical surgeon, who instead of holding himself aloof, or of sneering at the labours of those who, with the aid of the microscope, have endeavoured to remove some of the difficulties connected with this great subject, has, on the contrary, done everything he could to encourage and facilitate their researches. We are in this way supplied with a large number of instances where tumours of various kinds have been excised, and where the French histologists, M.M.

Lebert, Robin, Broca, and Follin, after a careful microscopic examination of the tumour, have pronounced their opinions concerning it. M. Lebert, as is well known, maintains the idea that the cancer-cell is a something specific and characteristic of cancer, and this idea, though combated by later and more cautious observers, has given an impress to histological theory in France. M. Velpeau has consequently demonstrated that these gentlemen have been frequently in the wrong—that many tumours pronounced by them to be malignant, were, in point of fact, not so; and that others, too confidently maintained by them to be innocent, rapidly returned after extirpation, in various parts of the body, destroyed the patient, and were consequently malignant. Although M. Velpeau evidently thinks that in thus overthrowing the French "*Micrographes*," he is benefiting surgery, it must be evident that he has only demonstrated what was long ago pointed out by ourselves, and what Mr Paget's work has confirmed, namely, that so-called malignancy was not peculiar to this or that texture, and that the most innocent growth might repullulate and kill, as well as the most undoubted cancer. But M. Velpeau has done more, for he has shown that tumours undoubtedly cancerous, both in a surgical and histological point of view, may be excised with the most perfect success. In this also he has confirmed the statement made long ago by ourselves, viz., that cancers are not uniformly fatal, and that it was unpathological to maintain that any kind of growth whatever, could not, under certain circumstances, be successfully checked or extirpated.

As we consider this point to be by far the most important one, both theoretically and practically, in M. Velpeau's work, we shall allude to some of his facts. At page 565 a case is given of a lady, Madame D., forty-eight years of age, who had a tumour of the breast in every particular resembling a scirrhus, which by means of occasional leeching, frictions, with a pomade, and pressure by means of a plaster, completely and permanently disappeared. This, says M. Velpeau, he has seen to occur in several women. But of the case which is given in detail, he says—

"It has so rarely happened for me during the last fifteen years to be mistaken as to the nature of scirrhus, that it is difficult not to believe that Madame D. had a scirrhus of the breast. On the other hand, after what I have often seen, it so difficult for me to admit a radical cure of scirrhus without operation, that I communicate this observation more for the sake of instruction than as irrefragable proof." "I could give other analogous facts, but I believe it useless to multiply the number, because my reason for citing them is less to prove the curability of scirrhus, than to engage practitioners not to deny absolutely the possibility of the fact at the commencement of the disease."

This struggle of the surgeon, between consciousness of a correct diagnosis, and his unwillingness to believe in the fallacy of a dogma to which he has so long adhered, is excellent. But we accept your facts, M. Velpeau, and believe them to be genuine. Still, it is pos-

sible that cases of this kind are, after all, not truly cancerous. Mistakes may occur even in the hands of the most distinguished and experienced surgeons, although their lives have been spent in teaching clinical surgery, and examining patients in the extensive wards of La Charité. It is satisfactory therefore to know, that cancer may be successfully extirpated, where there could remain no possible doubt of the nature of the disease, and of such the present work of M. Velpeau gives us many instances. Thus, he tells us that in 1852 he had already seen six cases of cancerous bone, with large fungoid tumours, in which both M. Broca and M. Lebert found abundant cancerous cells, and which, after amputation, had not returned. He then proceeds to relate several others of a like kind, in which not only he himself as a surgeon, but M. Lebert, after the most minute microscopic investigation, could have no doubt as to the cancerous nature of the disease, yet in which a permanent cure was obtained by its removal. One of these cases it may be useful to quote.

“ Vast mushroom encephaloid, discharging daily a frightful quantity of liquid sanies in a lady fifty-eight years of age; Extirpation; Microscopic examination; Cure without return.

“ Madame de J—— consulted me at the commencement of 1850 for a tumour which had been forming for some months outside the left breast. It was then the size of a nut, and the patient refused to have it extirpated. Subsequently she was treated by M. Guersant and M. Cruveilhier with various remedies, but the size of the tumour increased rapidly. Gradually the integuments were destroyed, and a grey reddish bleeding fungus attacked the whole of the axillary region. The patient became very feeble, from the frequently abundant hemorrhages; and when I again saw her she was in the following state:—The pulse ninety-six, small; the skin everywhere as if united to the bones; digestion difficult, and admitting only of the lightest aliment; the tumour still moveable, sending no root beyond its base, either under the edge of the great pectoral, or into the summit of the axilla. It formed externally, however, a fungus of the volume of two fists, and discharged a sanious serosity, of a nauseous and repulsive odour, in such quantity as to saturate from ten to fifteen towels every day. For fifteen days more than a quart of matter was so discharged.

“ The patient, her family, and M. Cruveilhier, earnestly demanded an operation; and, notwithstanding the slight hope of success and the size of the tumour, the abundant hemorrhages and discharge allowed of no temporization. Its performance therefore was decided on, even although life might be only preserved for a few days. On proceeding to the operation, on the following day, the difficulties of the case were increased on discovering that erysipelas had been induced over a considerable surface of the thorax, by the sanious discharge. With the assistance of etherization, however, the operation was completed. The first twenty-four hours subsequently were passed between life and death, but on the second day her strength increased notwithstanding the spreading of the erysipelas. From this time her general health rapidly improved, and the large cavity was completely cicatrized at the end of the eleventh week. Since then Madame de J. has become fresh-looking, strong, and scarcely recognizable, and up to this time nothing induces me to suppose that the disease will return.

“ The structure removed appeared to the naked eye to be formed almost entirely of cerebriform tissue of the purest character I ever saw, one might truly have called it cerebral pulp. It consisted of masses of reddish grey substance, more or less mingling together, in certain points reduced to a *bonillie*, fungoid, and easily crushed; in others containing a vascular net-work. MM. Follin and

Lebert, who examined portions of it with the microscope, also stated it to be composed of encephaloid matter.

"M. Cruveilhier and myself were so forcibly convinced that this cancer would soon return, that we advised and undertook its removal, simply to retard death for some days, and to diminish for a little a disease that we believed incurable. It was, in a word, by necessity, and not to acknowledge defeat—it was an operation *in extremis*, such as might be done for strangulated hernia, in a case labouring under the last stage of phthisis, or the amputation of a crushed limb in a cancerous person altogether incurable."—Pp. 586–8.

Several cases of this kind are given, with others where the tumour was genuine scirrhus, found to be so by the microscope, and yet where removal induced permanent cure. One case especially may be alluded to, recorded page 594, in which the microscopic characters are figured, plate 8. The operation was performed in 1847, and she is at this moment perfectly well. M. Velpeau also tells us that the return of the tumour is no necessary obstacle to permanent success. "So long," he says (p. 684), "as the secondary tumours are moveable, easy to extract, and that the woman does not present any symptoms of general infection, they ought to be removed on the same principles that we operate on the primitive swelling. In this way I have cured, radically, women after three successive operations, and some others after the second." Of the former, a well-marked case is given, the patient first operated on in 1841 has been in perfect health since 1843. M. Roux has succeeded in bringing about a permanent cure, after six returns of the cancer.

What we have now learnt positively, then, is, that true cancerous tumours, both hard and soft, not merely said to be so as the result of experience, but *proved* to be so by careful histological examination, are admitted and described by one of the first surgeons of the day, to be capable of being successfully extirpated from the body, and causing a restoration of perfect health from at least three to six years, and in a few cases even longer. But, Mr Paget says, that neither he nor Lebert have yet met with a case in which recurrence was delayed beyond eight years (p. 347); and as to ultimate cure, he adds, "without saying it is impossible, it is so highly improbable, that a hope of its occurring in any single case cannot be reasonably entertained" (p. 351). It is of the utmost importance, therefore, in reference to this question, that M. Velpeau should continue to watch his cases, because on him seems to depend the speedy solution of the great practical question, whether a tumour *proved* to be a cancer, may or may not be permanently eradicated.

With regard to arrangement, M. Velpeau, like Mr Paget, divides tumours into benignant and malignant. In reference to the breast, the former are thus arranged:—1. Engorgements; 2. Hypertrophies; 3. Lipomas; 4. Induration; 5. Cold or Tubercular Tumours; 6. Osseous or Calculous Tumours; 7. Milk Tumours or Galactocoele; 8. Cystic Tumours; 9. Adenoid Tumours, which comprehends some of those described by Lebert as fibro-plastic.

The malignant tumours are arranged as follows:—1. Scirrhus, which is divided into ligneous and lardaceous. The ligneous is again subdivided into A, proper or globular scirrhus; B, ramified or ramous scirrhus; cuirass-like or tegumentary scirrhus; C, scirrhus in mass, or ligneous scirrhus; E, atrophic scirrhus; F, pustular or disseminated scirrhus; G, scirrhus affecting the milk ducts; 2. Encephaloid; 3. Melanosis; 4. Chondroid, colloid, and fibroplastic, which he thinks to be modifications of one another. Cases are given, where, like the malignant fibrous tumour of Mr Paget, after excision, the growth has returned in various internal organs. 5. Epithelial Cancer, or Epithelioma; 6. Keloid. This growth never returns in internal organs, but always in the cicatrix. It is then recurrent, but is it malignant? 7. Anomal cancer, of which one instance only is given.

This classification is very different from that of Mr Paget, but equally unsatisfactory. In fact, we may ask what is to become of the science of medicine, if every author is to give us a new division with new names of morbid growths, without in the first place endeavouring to establish those principles on which all classifications should be founded. To be continually proceeding in this manner is a positive libel on the cultivation of pathology. What seems to us now to be required, is that we should come to some understanding as to the meaning of cancer. Let us, if it be only temporarily, agree to call one growth cancer (if that name must be used), and those which resemble it in one or more points, but which are not identical with it, either cancroid, or a more definite name, if one can be found. The difficulty is to fix upon a standard, and to fairly describe the type of cancer. Dr Druitt has attempted this in the following words:—

“Cancer is a disease of the blood, manifested by the evolution, in some part of the animal economy, of a structure called the cancer nucleus. This is a bright oval well-developed plump body, averaging a little less than one two-thousandth part of an inch in its long diameter, and containing one or more large and distinct nucleoli. In soft, *intense*, rapidly growing cancerous tumours, these nuclei may be free, floating in a liquid, or may be embedded in a soft transparent amorphous substance; but, usually, this amorphous substance is gathered about the nucleus in a more or less definite spherical mass, called a cell. One cell may enclose one or more nuclei. It becomes much more visible by the addition of water, but vanishes on the addition of acetic acid, which renders the nucleus more bright and distinct. Taken together, the characters of the cancer nucleated cell are, the large size of the nucleus, and of its nucleoli, and the fact that the cells, if abundant and adherent, and ill developed, are heaped together amorously; that if few and well developed, they have no adhesion to the tissues around them, but readily exude from a cut surface in a milky liquid, called the cancerous juice. This juice in general mixes readily with water; and the cancer cells being, like the similar bodies in milk and pus, not adherent to each other, are equally diffused throughout the liquid, and do not usually clot together as some other cells do.”

Now, this cancer nucleus is no better than Lebert's cancer cell. No histologist can possibly accept it as a ground for distinction, know-

ing, as he does, that the description of it by Dr Druitt applies to the normal structure of epidermic, epithelial, ganglionic, and cartilage corpuscles. Still the *idea* of Dr Druitt we have always held to be the correct one. If we could fix upon a structural element, whether the cell or nucleus, and prove it to be characteristic of the most common forms of cancer, let us agree to apply that term to it alone. Then future progress would be easy, for, in the words of Dr Druitt—

“ If, in any growth, after thorough examination by competent persons, no cancer cell or nucleus can be detected, that is evidently not a cancer. Let its anatomical elements be figured and described, and let its clinical history be drawn up for comparison with similar specimens which may hereafter be met with, and let some new and positive name be given to it ; but if in any disease, no matter what it is, and how it proves fatal, nothing is found which can be identified with the cancer nucleus, that disease ought to be called by some other name than cancer.”

Now, it will be remembered Mr Paget objects to this, and, as we have already seen, thinks we should base our distinctions not so much upon structure as upon vital properties. But as we think Dr Druitt has very ably shown, these, though numerous, have nothing really exact or definite. They are, 1. Constitutional origin ; 2. Rapid progress ; 3. Constant progress ; 4. Pain ; 5. Return after extirpation ; 6. Secondary deposits ; 7. Diffusion ; 8. Cachexia ; 9. Resistance to treatment ; 10. An infiltrating mode of growth ; 11. Degeneration and softening ; 12. Ulceration ; 13. Invasion of lymphatics ; 14. Heterology of structure ; 15. Death.

Of these, says Mr Druitt, we find—

“ That cancer is the only malady which possesses them all ; that epithelioma, the Greek elephantiasis, lupus, and tuberculosis, come the nearest to it ; that the glandular, fibrous, cartilaginous, and fibro-plastic usually display only four or five ; yet that whilst there are exceptional cases, which prove the possibility of a cure of cancer, so there are conversely exceptional cases which prove that tumours, usually innocent, may destroy life in the same way in which cancer does.

“ Therefore I argue, that the vital characteristics of these growths being variable, and the structure constant, the latter alone can be taken as a basis of classification ; and that since cancer is, as a rule, the only *malignant* disease, using that word in the sense of incurable, it were better, when cancer is spoken of, to call it cancer ; because, if the term malignant be applied to any other disease, it occasions confusion and mischief ; it may lead to the belief that a disease is cancer when it is not ; and that it is incurable when perhaps persevering treatment might be successful.

“ But, in truth, the word malignant, applied to a disease, is like the *Græcum est, non potest legi*, of the mediæval ecclesiastics ; it is an excuse for non-interference ; it appeals to the imagination, invests disease with mystical supernatural qualities. Thus the practitioner, instead of treating his patient, lulls himself to sleep with dreams of heteromorphosity.”

With these remarks we fully concur, always, however, protesting against the idea that cancer is necessarily incurable. At the same time, it should be remembered that before a structural character can be fixed on, the development and natural progress of the textures

must be understood. We must not forget that nuclei may give rise to cells, and that these may degenerate and disintegrate. Hence we must not only learn to recognise the structural element in its perfect form, but be enabled to determine whether it be young or old, whether advancing or retrograding. The nucleus character of Dr Druitt is probably only the young cell form of Lebert, and the reticular bodies found in cancer are perhaps the degenerated results of both. On this point, however, we cannot now dwell, and would only observe that we prefer the terms epithelioma and enchondroma, to epithelial and cartilaginous cancers. Their separation from true cancers is a great step in pathology. When the other forms of cancroïd are as well studied, doubtless names of equally valuable import will be applied to them.

When, now, we consider the difference in opinion which exists as to the curability or incurability of cancer, and the various degrees of liability which different tumours possess, of being recurrent, or of spreading to analogous or other tissues, it may well be asked if no cause for this can be sought for. Although it may depend on constitutional circumstances, this cannot absolutely be affirmed, until efforts have been made to determine whether no cell germs remain in the part operated on. We have ourselves seen a case where it was demonstrated that the muscles forming the flap of a stump were at the time of operation to be infiltrated with innumerable cancer cells. Under such circumstances, the disease could not be said to have been removed with the amputated limb, and the return of cancer was not to be wondered at. We were much struck with the following passage in M. Velpeau's work :—

“Two sisters, superb women—Madame de V——, and Madame L'h——, who lost their mother from a cancerous affection, were both attacked, one at the age of thirty-four, the other at the age of thirty-five, with tumours in the left breast, occupying the external side of the mamma. The axillary glands were unaffected in both. I operated at the interval of a year in the two cases ; on one in 1848, on the other in 1849. In Madame de V—— the tumour was a partial scirrhus, a little lardaceous ; in Madame L—— it was an encephaloid, uniformly lardaceous at the base, soft, and almost liquefied at the summit. In the last the tumour was a little larger than the fist, in the former the size of a hen's egg. The results of the operation were simple and regular in the two cases, and the general health in both became re-established. But in nine months the cancer reappeared in Madame de V——, and a new operation became necessary six months later. The wound of this second operation was scarcely cicatrized, when scirrhus tubercles, and disseminated cancerous pustules appeared around the cicatrix. Madame L——, on the contrary, has remained well, and up to this day enjoys perfect health.”—Pp. 608, 609.

In this instance the disease was equally hereditary, equally constitutional. The two women were sisters, and nearly of the same age. The operation was performed by the same surgeon, and treated in every particular in the same manner, yet the encephaloid, or what is usually considered to be the most malignant tumour, has not returned in four years, whilst the scirrhus tumour came back in nine months. Whether this patient is still living is not

stated. So much for our ideas of "malignancy!" Most assuredly our present notions on this point are faulty in the extreme, and a far more extended series of researches is required before we are warranted to approach a correct theory of, much less dogmatize on, morbid growths.

In the meantime we must regard Mr Paget's work as a most valuable contribution to our knowledge of this important subject, and the facts it furnishes us with, as constituting a solid basis for future research and generalization. M. Velpeau's facts, also, are of the utmost value in enabling us to arrive at the natural history of tumours; while Dr Druitt's paper contains an admirable and well-timed argument in support of that line of investigation which we think all pathologists and surgeons ought to follow.

The Diseases of the Heart and the Aorta. By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, etc. Dublin. 8vo. 1854. Pp. 689.

EVERYTHING from the pen of Dr Stokes naturally arrests our attention. His fame has long been European. His skill as an auscultator has added celebrity to the Dublin school of medicine. The long and close study with which he has applied himself to the investigation of the particular objects treated of in the volume named at the head of this article, at once secures for him the serious attention of his professional brethren. "This work," he tells us, "embodies the results of my clinical experience, continued almost unremittingly for upwards of a quarter of a century."

However much we may differ on certain matters from the author of this work, it is impossible for us not to express, *in limine*, our admiration of the careful patience with which he has been so long contented to labour in the field of experience, and our gratitude for his having had the courage at length to consummate his task, by collecting together for the benefit of science the results of a "quarter of a century's experience." The reviewer's work becomes fresh and grateful, when he is engaged in the discussion of the results of such experience; his labour is one of love.

And here, if we might venture for a moment to digress from our subject, we would express a regret, that age and experience so seldom give utterance to their matured and better knowledge. The press groans with a teeming progeny, and the ink in the reviewer's pen is never dry; but how comes it all the while that those who could enlighten, could correct errors, teach wisdom, and give to the profession that most invaluable of lessons—the results of matured experience—how comes it that they are silent? that in the arena of discussion, at our societies, where youth so frequently exacerbates its crudities, the staid voices of our Nestors are so sel-

dom heard? The time has been, we fancy, when physicians wrote out of the fulness of their knowledge. Hippocrates, Harvey, Sydenham, Boerhaave, Corvisart, and such like men, spoke because they *felt* they had utterances to give which would profit the world; they wrote not because they had naught else to *do*, but because they had so many things to *say*. Why are our Harveys and our Sydenhams silent? Do we possess no brethren of equal experience?

But to return to our author. Throughout his work there is one prominent lesson, which he would desire his readers to keep steadily in view, and that is, excessive caution in the indulgence of minuteness in diagnosis as to the particular nature and character and situation of diseases of the heart:—"Too great positiveness marks some of the statements in our standard works, and the difficulties of special diagnosis are still infinitely greater than many might be led to observe." Whether Dr Stokes has not carried his caution even to sceptical lengths, we shall hereafter have occasion to discuss; but, in general terms, we willingly admit the justice of his rebuke. In prying too curiously after the particular seat or nature of this or that auscultatory sound, the physician is very apt to let slip from his mind, when he is about to form his diagnosis, the general symptoms, or to render these unduly subordinate to the facts revealed by his stethoscope; and perhaps, somewhat in proportion to his skill in the use of this instrument, to give to the auscultatory phenomena a predominance in the scales.

Dr Stokes commences his work with the matter of pericarditis. Now, concerning the history, symptoms, and diagnosis of this affection, we believe that most of us are, in all material particulars, tolerably of one mind; respecting, however, the most material fact, the treatment, opinions are still very varied. Hence, upon this matter, we at once turned anxiously to Dr Stokes, to participate in the benefit of his large experience, and we regret to say that *he* does not speak in glowing terms of the results of his practice; he is in fact, and perhaps wisely, somewhat meagre of details on the subject, not even telling us whether he has or has not found the treatment which he adopts generally successful. What is his treatment? Bleeding, local and general, mercurials, blisters. The good old honest antiphlogosis still holds its own, somewhat modified however. Pericarditis, Dr S. sets down as idiopathic and rheumatic. The activity of treatment requisite in the former is not necessary in the latter. Whether the rheumatic species demands any special modification of treatment is still an open question; it will, however, be generally right to use mercury pushed to salivation in cases of the rheumatic form, attended by excitement of the heart and valvular murmur, and the signs of a progressive liquid effusion. Opium is generally useful, but Dr S. has never found that colchicum has any beneficial effect either in pericarditis or rheumatic arthritis, while the inflammatory fever continued. With respect to general bleeding, this should be very cautiously employed; our author is "convinced that

the fatal result of some cases of pericarditis is mainly attributable to the perseverance, beyond the proper time, in the antiphlogistic treatment." Regarding the mercury, Dr Stokes recommends its administration after Dr Graves' plan, viz., by giving large doses of calomel—10 to 20 grains—at long intervals; this plan, he says, will probably best answer our expectations; we must say we should have preferred something more definite from our author's vast experience, than is involved in this word *probably*. He seldom uses mercury in rheumatic pericarditis when the symptoms are mild.

Of specific kinds of treatment he has nothing to say. He points out clearly the signs of that enfeebled state of the organ in which stimulants are required, stating, very truly, that there is "no local inflammation, the mere existence of which should prevent the use of wine, if circumstances require it," and adds that he "is convinced that cases are often lost from want of stimulation at the proper time." We must regret that Dr S. does not give the impression which the effects of this method of treatment has left upon his mind; certainly we understand that he holds this to be the best treatment, but he gives us no clue to his own judgment of its actual efficacy. Bouilland found an infallible cure in the lancet; some learned men have found, or thought to have found, a like cure in mercury; others, in particular saline drugs; and so on; but then others again have found all these things fail, and, if the truth be spoken, we believe that there is, in reality, very little agreement as to the remedies best adapted for the treatment of pericarditis even amongst authorities in high places; one shall still extol the lancet, and another carefully foster every globule of the vital fluid; one shall swear by opium, and another look upon mercury as a swift-footed fiend; one shall purge, another retire his faith upon alkalis; and we understand there are those who content themselves with a calm contemplation while the disease progresses. Who is right? It would be something to know who is wrong. Certainly some of the most fatal cases we have seen have been those which have been treated after the most orthodox fashion; and assuredly we have known many very severe cases recover under simple treatment. Upon the whole, our own experience quite leads us to follow in the steps of our author, in eschewing specifics, and adapting and applying the remedies he adopts according to the nature of the disease, and, above all, *to the condition of the patient*. Dr S., in thus pointing out what he considers the best treatment, has, in our opinion, exercised a very wise reserve, in refraining from a comparative discussion of its merits. The results of treatment of disease in its acute forms are still, unfortunately, but of necessity in the present state of our knowledge, over much matter of individual belief; in most cases more so indeed than we care to admit. He has left the subject in an unsatisfactory condition, and, we believe, in its true condition.

As we have said, Dr Stokes' account of the diagnosis and history

of pericarditis agrees in the main with that in general acceptance ; perhaps in his relation of them he has been too diffuse, and too free in subdivisions of his subject. We cannot, for instance, see the utility of making three classes of pericarditis, according as in the 1st, There is slight but general effusion of coagulable lymph ; in the 2d, In addition thereto, secretion of serum distending the sac ; and in the 3d, Superadded to these, signs of muscular excitement. Surely such splitting of the subject is neither possible in itself, nor, if it were, of utility, or applicable in practice. We certainly must suggest, admitting willingly the large amount of practical matter which it contains, that the whole of this first part of the work might have been condensed and better arranged.

We will endeavour to point out to our readers those particulars which we consider most worthy attention, as being the results of our author's experience, or his opinions on disputed points. We must necessarily pursue the subject in a disjointed manner, for thus our author leads us along.

"The mere occurrence of a murmur, even though immediately consequent on pericarditis, does not necessarily indicate progressive valvular disease ; it may disappear, and the natural sounds of the heart remain." This, we believe is true, but probably only in the case of an aortic valvular murmur, or of a murmur heard in those situations where we look for what are called anæmic murmurs. Dr Stokes does not specify any particular murmur in referring to this fact ; and we are inclined to the statement of Dr Ormerod :—"A murmur heard loudest at the apex, and thence transmitted upwards to the left of the mamma, I have never, but with one exception, found to depend upon anything but organic disease of the heart." Our own observation agrees with that of our author, that systolic bruits much more frequently exist in fevers and acute diseases than appears to be generally supposed, and disappear as these and their consequences recede.

Too much importance, Dr S. considers has been ascribed to the effect of pressure of fluid upon the heart, in explaining the mode of death in pericarditis ; and in support of this position he instances the great pressure which the heart will suffer, with impunity to its functions, in certain of its dislocations ; so long as the contractility of the fibre is not weakened by disease, the muscles will bear great pressure without their functions being suspended. It seems to us that here one great element has been omitted in Dr Stokes' explanation. The pressure of the fluid on the heart is *universal and equal*,—it acts upon the auricles as on the ventricles ; but in dislocations of the heart the pressure is not uniform over every part, and in all probability never so great as in the case of a distended pericardium. Also Dr S. has omitted to mention the difference between the injurious *effects* of the same amount of pressure upon the ventricles and upon the auricles ; it is manifest that they must be far greater upon the latter than upon the former ; in fact, when we call to mind the very moderate amount of muscular power exerted by

the auricles, and their naturally collapsed condition when not distended by causes extrinsic to themselves, we seem forced to the conclusion that a very moderate amount of pressure is sufficient seriously to disable their expanding power—an injury instantly resented by the systemic and pulmonary circulations, as indeed we actually witness in this disease (pericardial effusions). We dwell on this point, for when pressure upon the heart by pericardial effusions is spoken of generally, little or no distinction seems to be accounted of between its effects upon the auricles and upon the ventricles; Dr S. has not drawn this distinction.

Speaking of the friction sound, he says, that though it may be hard to prove, yet there seems to be no reason why a merely dry state of the membrane may not produce the sign. Of this opinion we cannot doubt the correctness; the gentlest rubbing of two dry surfaces will produce a distinct sound. Let any one, for instance, place the palm of one hand firmly over his ear, and rub the back of it with a finger of the other hand, and he will hear a rubbing sound, however light be the pressure of his finger. We remember on one occasion hearing the distinctest pleural frictions, in a lad a few hours before his death, in whom afterwards merely a dry condition of the particular points of the pleural surface, with congestion, were visible, appearances which would have most assuredly been overlooked after death, but for the signs heard during life.

What are the effects resulting from adhesion of the pericardium? We are not exactly in a position capable, from facts, of replying to this question. Dr Stokes does not admit Dr Hope's dictum, that general hypertrophy and dilatation are invariable results; on the very contrary, he concludes that obliteration of the pericardium does not necessarily induce any manifest change in the condition of the heart. On this head we would refer our readers to the translation of a valuable paper by Skoda, published in the July number, 1852, of this Journal, as containing some original facts upon this subject. We are not aware whether it has ever been suggested, that an endocardial murmur might arise through adhesion of the pericardial surfaces. We are desirous of calling attention to this point, because a case, a well observed case, has lately fallen under our notice, in which the murmur cannot be accounted for it seems on any other hypothesis. A loud systolic bruit was heard for six or eight weeks *persistently* over the apex and left of the heart; no bruit over or along the aorta, or down the sternum; the second sound of the pulmonary artery (and to this sign we shall hereafter call more particular attention) was remarkably increased, and its vibrations were perceptible to the touch; the general symptoms were markedly those of organic disease of the heart; the girl, the subject of the disease, had suffered about two months before her death rheumatic fever and pericarditis, on which this endo-cardial bruit had supervened. After death, and this was manifestly produced by the heart disease, *all* the valves were found healthy; the pericardium *partially* adherent, and firmly

so across the middle of the left ventricle, other parts of this ventricle being free; firm adhesions from inflammation had taken place also in the tissue between the sternum and the pericardium. Is it not very fair to suggest, in the absence of all other means of accounting for the existence of this bruit, and mitral regurgitation, that some irregularity was produced by this partial adhesion of the pericardium, in the action of the columnæ carneæ to which the valves are attached? Excellent observers have noted the existence of permanent mitral murmurs, in cases where death has ensued, with the symptoms of cardiac disease, and in which, nevertheless, all the valves were afterwards found healthy.

Dr Stokes himself, at page 152, quotes from Dr Graves a case of this description, and he does so for the purpose of showing "the accidents which may be in store for those who are over confident in special diagnosis." In this case the patient died from the usual symptoms of heart disease. A loud systolic bruit had been heard (constantly, we presume, from the context) over the cardiac region, and loudest to the left of the nipple. The heart was found hypertrophied; its valves all healthy, but *the pericardium universally adherent*. Some rough earthy matter was found in the ascending aorta. Now, we venture to think, that here, as in the case above related, so far from accusing the stethoscope of leading us astray, we ought rather to accept the facts revealed by it during life, as aids to enlighten us as to the obscurity of the pathological condition; and that we are justified in the belief (both from the stethoscopic signs and the general symptoms) that the mitral valves, though in themselves healthy, were yet defective in their function; that the cardiac bruit was a truly regurgitant mitral murmur; and that such "special diagnosis" would have been correct. Is it not much more rational in such a case as this, to believe rather that our ideas as to the causes which give rise to cardiac murmurs, are too restricted, that there may be other besides those which have been accepted hitherto, than to reject such a manifest sign as a loud and permanent systolic mitral murmur (accompanied by all the symptoms of organic heart disease, and followed by death), and to refuse to admit it as the indicator of regurgitation of the blood through the mitral opening, *because* the valves are found healthy after death. Here, as in the former instance, in the absence of all other apparent causes, is not the hypothesis of the murmur being the result of an adherent pericardium, fair? What more reasonable than to suppose that the contraction of the ventricle might be so interfered with by the particular nature of the pericardial adhesions (and especially where inflammation and adhesions have, as well, taken place external to the serous sack), as to cause defect or irregularity in the contraction of the papillary muscles to which the valves are attached. Here, we suggest, the stethoscope leads us further than the knife of the anatomist; the bruit it revealed indicated defective valvular function. The anatomist can trace no sign of valvular disease.

Dr Mayne has shown, in reference to the diagnosis of pericarditis, that well marked symptoms of the disease may exist for some time, even for as long a period as three days, before any friction sound is audible. The practical lesson to be drawn from this is self-evident.

(*To be continued.*)

The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations. By JOHN ERICHSEN, Professor of Surgery, University College, and Surgeon to University College Hospital. London. 8vo. Pp. 951.

OWING to the late period at which we received this book, and the press of other matters, our limits only afford us space for a very brief and general notice of its contents.

The work professes to be a system of the science and art of surgery. The author, however, in his preface states that his original intention was to have limited the work to a consideration of surgical injuries, diseases, and operations, or what is usually termed practical surgery, and that the idea of converting it into a systematic treatise by prefixing general principles was an after-thought. This, whilst it explains the inequality observable in the execution of the different parts of the book, scarcely excuses it, for we think that the author, when he did alter his original intention, was bound, both in justice to his own reputation and to the students for whom it seems to have been more specially intended, to have taken time to render the general principles worthy of the practical portion of the work.

It is in reference to general principles that we consider it rather defective, not that there is anything positively erroneous stated, but there is a meagreness of detail observable, which contrasts unfavourably with the care bestowed upon the special injuries and operations. We are by no means admirers of long verbose treatises on general principles, still less do we desiderate erudite and abstruse disquisitions on minute structural pathology in a work like this; on the contrary we prefer brevity of description if it be attained by condensing much information into small space. But what we consider as absolutely demanded is, that it shall be sufficiently full to convey to the student such a knowledge of general laws as will enable him to understand the phenomena of disease, to judge somewhat of disputed points of doctrine, and to draw sound practical deductions. Now, one of our objections to Professor Erichsen's method of treating general principles is, that he is often dogmatic in his statements of doctrine just in proportion to the uncertainty of the subject, and hence he occasionally enunciates rather startling doctrines without affording his reader sufficient data to judge from. Thus, for example, in speaking of the repair and regeneration of bone we find him stating:—

"When the whole of a shaft dies, the reproduction takes place from various sources, principally perhaps from the periosteum, and the medullary membrane, if that is left, which become inflamed, vascular, and detached from the necrosed bone; the periosteum indeed takes the principal share in the reproduction, and has been considered by some pathologists as the true organ of reproduction of new bone. Then again the soft tissues of the limb generally, if thick, as in the thigh, contribute to the formation of plastic matter, which gradually ossifies, and so tends to strengthen the new case; and, lastly, the articular ends of the old bone still preserving their vitality, throw out sufficient osseous matter to consolidate themselves firmly to the new shaft that is formed. Thus it will be seen that the new bone is formed by the vascular and healthy tissues generally that surround the seat of disease, though in this reparative action the periosteum and medullary membrane take the chief share."—P. 578.

Again, in speaking of the union of fracture, he says:—

"That neighbouring parts participate in the inflammation set up around the fracture, and throw out callus, is evident by what takes place occasionally when one of the bones of the forearm or leg only is broken. Periostitis is then set up in the unbroken bone, opposite the seat of fracture, and osseous matter sometimes deposited by it. We have specimens illustrating this point in the University College Museum."—P. 168.

That the periosteum under any circumstances acts directly as the formative or reproductive organ in depositing true osseous matter is a pathological doctrine which has been much questioned. As to the formation of the substitute bone in necrosis we think the investigations of Professor Goodsir have settled the controversy by the admirable manner in which, by tracing the mode of nutrition in healthy bone and the modifications which occur in consequence of disease or injury, he has shown the source of repair to be in the proper texture of the bone, and at the same time has pointed out the sources of fallacy in experiments which at one time appeared conclusive in favour of the opposite views. But it is not so much the opinion we object to, as that Mr Erichsen neither states the opposite opinions held by others, nor adduces any reasoning in support of his own, so that the student has nothing but a dogmatical statement on which to rest his belief.

We dislike also the unqualified manner in which our author occasionally enforces a great general rule, as if there were no exceptional cases. Thus, in speaking of the propriety of direct ligature in cases of hemorrhage from an open artery, which every one admits as a great general principle, he says:—

"The rule of cutting down on the injured part of the artery applies to all cases in which the wound is still open, whatever be its condition. However deep, inflamed, and sloughy the wound; however ill-conditioned and infiltrated with pus or blood the neighbouring parts may be, there is no safety to the patient unless the vessel be cut down upon and tied at the part injured. This must always be done at any period after the receipt of the injury, so long as there is an external wound communicating with the artery."—P. 139.

Now, this is going rather far; for there are cases, occasionally, though rarely, met with, where, on account of the sloughy state of the vessel and of the surrounding parts, direct ligatures would not hold, and their application would be futile.

In regard to the special injuries and operations, the manner in which that part of the work is executed is most praiseworthy, and gives evidence of great care and research; and from the ample detail and explanation of the symptoms of disease or injury, and the careful description of the operations, it may be consulted with advantage by practitioners as well as students. The chapters on the treatment of special aneurisms and hernia are particularly worthy of study. We differ, however, from Professor Erichsen as to his appreciation of Mr Gay's plan of operating in femoral hernia; but having already fully stated our objections to it when we reviewed Mr Gay's work in this Journal, we deem it unnecessary to enter on the subject here.

In conclusion, viewing the book as a whole, we consider it highly creditable to the author. The defects we have considered it our duty to point out, seem to depend on hasty execution consequent on his change of plan, and we doubt not, will disappear in future editions. The practical part is superior to that of most systematic works on surgery, and its literary merits are of a high class.

Notes on the Condition of the Indian Medical Services. By JOHN MACPHERSON, M.D., Assistant Surgeon, Bengal Army.

AT a time when it is understood that a considerable addition is about to be made to the numbers of the Indian army, and when a good many medical appointments may be expected, a pamphlet on the present condition of the service cannot fail to possess some interest for those who are aspirants to this honourable field of professional employment. A good part of Dr Macpherson's brochure refers to plans for bettering the remuneration of some departments of the East Indian Medical Service. Not being personally conversant with the subject, we do not venture to express any decided opinion as to whether the arrangements between the Company and its medical officials are good, bad, or indifferent. We certainly should be inclined to judge, from the very temperate statement of our author, that there are some things which might easily be altered for the better.

As every body, however, who is called upon to consider the arrangements for going to India, desires to profit by the experience of those who have practical experience of the matter, we subjoin some hints as to outfit, etc., which may be useful. We only wish that Dr Macpherson had gone a little more into detail, as he thereby would much have enhanced the value of his pamphlet.

"Some of the questions which an assistant surgeon usually puts to himself are—how had I better go to India?—what uniform am I to take?—what books and instruments?—what professional degrees should I have?—are letters of introduction worth having? Let us see whether we can give any satisfactory replies to them.

"To the first of these questions the answer is very simple. If you are not

straitened for means, if you have any cash in hand, come out overland,—save yourself a wearisome voyage, and gain two or three months' service in India. But if the *res angusta domi* presses you, or you are burdened with the incumbrance of a wife, endeavour to get charge of troops coming round the Cape. You will feel the receipt of some Rupees, 1500, head-money, on your arrival, a most comfortable help to your ulterior proceedings, especially as you will find, on your landing in India, that heavy donations to all the funds are cut from your small modicum of pay.

“As to the matter of uniform, no precise directions can be given; the last few years have seen the departmental uniform of the Madras Medical Service done away with, and that of Bengal established,—in the latter case a very plain and ugly uniform being adopted, in preference to a more appropriate one recommended by the Medical Board. In Bombay, as in Madras, we believe that surgeons continue to wear the uniforms of the regiments to which they belong. On the whole, the less uniform brought out the better; the money for it will be more usefully spent in India, and in Calcutta, at least, the prices of tailors are not now much higher than at home.

“It is better for the assistant surgeon not to spend any large sums of money on books, or to bring out any great number of them, as there is time enough to form a library when he begins to lead a settled life, and during the first few years he is so often moved about, that, if he does not lose his books entirely, he is pretty nearly sure to have them riddled by white-ants, or soaked in some river or nullah. The same advice may be given on the subject of instruments: the Company is liberal enough in the supply of surgical instruments, and such he need not bring out; but if he can use the microscope or blow-pipe, or has any turn for natural science, he will do well, if he can afford it, to bring out a few of the more useful instruments. It is not very long since all the surgeons of civil stations were ordered to make accurate meteorological observations, without any instruments wherewith to make them being supplied.

With regard to introductions, the author tells us that too much dependence should not be placed upon them, but that, for his part, he found them on the whole very useful.

Dr Macpherson, however, has hints for actual incumbents and seniors, as well as for aspirants and “griffs,” and as our motto is *sum cuique*, we append a little extract worthy of being duly pondered in the proper quarter:—

“Of the Calcutta practitioners, it may be said with truth, that they are anything but an united body; the Medical and Physical Society, which Mr Egerton did so much to keep together, has ceased to exist; no Medical transactions are published, there is no Medical journal, and the Service is entirely without an organ for the expression of its opinions.

“We must hope that some day fresh vigour will be infused, and it is only from the younger men in the profession that literary efforts can be well expected—for a variety of reasons which it is unnecessary to specify. It is certainly not creditable to the capital of India, that it cannot support even a Quarterly Journal of Medicine—or that the mass of information which the Medical Board Office can probably supply, should remain undigested, and thus lost to the Service. If the Medical Board are unable themselves to use it, have they ever suggested to Government any mode of making it available? Officers who think they have any observations of importance to give to the world, cannot be expected to send them to the Medical Board, merely to add to its mouldy records.”

This pamphlet, though published in Calcutta, can, we believe, be procured from Smith and Elder of Cornhill, London.

The Anatomy and Diseases of the Prostate Gland. By JOHN ADAMS, Surgeon to the London Hospital. Second Edition. Pp. 178. London: Longman, Brown, Green, and Co.

THIS is a second edition of a work already favourably known to the profession, and it is satisfactory to find that its merits have been so generally appreciated. Whilst many of the diseases of the prostate admit not so much of cure as of palliation, yet much depends upon an exact knowledge of their pathology, and on the practical experience of the surgeon in such cases, to enable him successfully to treat the more acute diseased conditions, and to afford relief in those exacerbations, which occur from time to time in the more chronic forms of the disease. Mr Adams is eminently a practical man, and conveys the results of his experience and observation in a quiet unpretending manner, without any unnecessary verbiage; and his pathological remarks are free from the besetting sin of the present day—scientific mysticism.

We might, perhaps, take exception to the views expressed in Chap. xix. with regard to the dilatability of the prostate gland. For repeated experiments on the dead subject, with reference to lithotomy, have convinced us, that after limited incision the gland does not dilate, but splits, as it were, in the direction of the incision. The safety of the limited extent of incision of the prostate in lithotomy is, we believe, now very generally admitted; but the term dilatability is an unfortunate one, as leading to such modifications as those proposed by Dr Willis and Mr Lloyd, and referred to by our author. But this chapter can scarcely be considered more than a few incidental remarks appended to the proper subjects of the work.

In conclusion, we have great pleasure in recommending the treatise as one containing much valuable practical information, and as a book of reference which the surgeon may often consult with advantage.

An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science, including a Complete Medical and Medico-Legal Vocabulary, etc. By R. G. MAYNE, M.D., Surgeon to the Leeds Lock Hospital, etc. London, 1854. Royal 8vo. Part I.

THERE are few medical men who do not at some time or other require a dictionary of medical terms, with their derivations, synonyms, and definitions. The work of Dr Mayne is admirably calculated to supply their wants. The mode of printing the terms in well-marked thick capitals, whereby they readily catch the eye, the accuracy of the details, and the general literary merits of the book, must ensure for this publication a large amount of patronage.

Part Third.

PERISCOPE.

PHYSIOLOGY.

EXPERIMENTAL RESEARCHES ON THE GREAT SYMPATHETIC, ESPECIALLY ON THE INFLUENCE WHICH THE SECTION OF THAT NERVE EXERTS OVER ANIMAL HEAT. BY M. CLAUDE BERNARD.

[To these important researches the Academie des Sciences has awarded the annual prize of experimental physiology for 1853.]

Historical Notice.—The remarkable phenomena produced in animals by section of the sympathetic nerve in the neck, were first observed by Pourfour du Petit (1727), and were confirmed subsequently by Dupuy (1816), Brachet (1837), and John Reid (1838). Whether the nervous cord was divided, or the cervical ganglions excised, the results which ensued were, contraction of the pupil, redness of the conjunctiva, retraction of the eyeball within the orbit, and the projection of the cartilage of the third eyelid in front of the eye. Of these phenomena the contraction of the pupil chiefly arrested attention. In 1846, M. Biffi, of Milan, observed that when the pupil is contracted in consequence of section of the sympathetic, we can restore its dilatation by galvanising the cephalic end of the divided nerve. Dr Ruete of Vienna having noticed, in paralysis of the third pair, that the dilated pupil could be further enlarged by belladonna, concluded that the sympathetic produced the dilatation by stimulating the radiating fibres of the iris. In 1851, MM. Budge and Waller found experimentally that the influence transmitted by the sympathetic is derived from a region of the spinal cord, comprised between the last cervical and sixth dorsal vertebra inclusively, to which they gave the name of cilio-spinal region; their experiments, however, had reference only to the action on the pupil. In addition to the phenomena previously noted, M. Bernard had observed the narrowing of the opening of the eyelids, which becomes more elliptical and elongated transversely—the flattening of the cornea, and the consecutive diminution of the eyeball—the narrowing of the nostril and mouth of the same side; and, finally, in March 1852, he announced to the Academy of Sciences his important discovery of the great increase of animal heat and of sensibility on the whole of that side of the head on which the sympathetic had been divided in the neck. He ascertained subsequently that the application of galvanism to the upper end of the sympathetic caused the disappearance of all the phenomena produced by the section of that nerve. At the same time Budge, Waller, and Brown Séquard continued the novel investigations originated by Bernard. Budge referred the increase of calorification to the cilio-spinal region of the spinal cord; Waller explained the increase of heat and vascularity by the paralysis of the arteries caused by section of the sympathetic; and Brown Séquard claimed priority in regard to the arterial paralysis, as the cause of the increase of heat, and announced that, while in America, he had been the first to show that galvanisation of the sympathetic diminished the temperature, and produced contraction of the arteries.

After this historical review, which fixes the position of his discovery of the influence of the sympathetic nerve on calorification (a discovery which he had arrived at in a series of researches on the influence of nervous lesions on the temperature of paralysed parts), M. Bernard discusses the conditions of the phenomenon in a series of inquiries.

§ I. *Is the great sympathetic the only nerve whose section produces increased evolution of heat?*

This question is tested by experiments on the 5th pair, the facial, and the

spinal nerves. After section of the fifth pair of the left side, within the cranium in the rabbit, the ear of the paralysed side was found colder than the opposite one; and next day, although the usual inflammatory symptoms had commenced in the left eye, the temperature was 3° centigrade lower than on the other side of the head. At this stage of the experiment the left sympathetic was divided in the neck, and immediately the temperature rose in the paralysed parts, and became higher than on the sound side. The temperature, as shown by the centigrade thermometer, in the ear was as follows:—

	Left side operated on.	Right side uninjured.
1° After section of the 5th pair,	31° cent.	34° cent.
2° After section of the sympathetic,	37° cent.	31° cent.

It will be noticed that the elevation of temperature on the left side coincides with a decrease on the right.

In order completely to paralyse the sensibility on the left side of the head, the auricular branches of the cervical plexus were next divided, yet the temperature continued higher on the side operated on during the whole time that the animal was observed, viz., till the sixth day. In similar experiments, frequently repeated, paralysis of the fifth pair was always followed by diminution of temperature on the corresponding side of the head, even although the phenomena of calorification could still be produced there by the section of the sympathetic.

After section of the facial nerve at its exit from the cranium, the effects were different. An elevation of temperature took place on the paralysed side; this was increased if the sympathetic were also divided; but, if the facial alone is cut, gradually after some days the temperature returned to an equality on both sides of the face. Thus—

After section of the facial—

Left ear paralysed,	. . .	33° c.
Right ear healthy,	. . .	30° c.

After division of the left sympathetic in addition—

Left ear paralysed,	. . .	36° c.
Right ear healthy,	. . .	31°·5 c.

When the facial alone was divided, the temperature was—

Immediately after,	{	Left ear paralysed,	. . .	33° c.
		Right ear sound,	. . .	31° c.
Next day,	{	Left ear paralysed,	. . .	32°·5 c.
		Right ear sound,	. . .	31°·5 c.
Six days after operation,	{	Left ear paralysed,	. . .	31° c.
		Right ear sound,	. . .	31° c.

The calorification, produced by section of the facial, M. Bernard attributes to the junction of sympathetic filaments with this nerve during its course through the temporal bone; for he observed that in paralysis of the facial produced by wounding (accidentally) its origin from the nervous centres, the temperature on the paralysed side generally diminished by one or one and a half degrees, the sensibility being uninjured in the paralysed parts. For a more direct solution, however, of the action of motor nerves on calorification, the experiments on the spinal nerves are decisive.

In a large, strong dog, the spinal column was laid open in the lumbo-sacral region, and the anterior roots of the six nerves (four lumbar and two sacral) which form the lumbar and sacral plexuses were divided on the right side. Two and a half hours after the operation the temperature of the paralysed limb had very sensibly diminished. The thermometer gave—

Left thigh sound,	. . .	36° cent.
Right thigh paralysed,	. . .	34° cent.

At this time the wound was reopened, and the posterior roots of the corres-

ponding nerves were divided on the left side. A diminution of temperature was then observed on both sides after an interval of time.

1st observation after	{ Left thigh paralysed of sensation,	35° cent.
half an hour,	{ Right thigh paralysed of motion,	34° cent.
2d observation after	{ Left thigh paralysed of sensation,	34° c.
one hour,	{ Right thigh paralysed of motion,	32° c.

In order to show that the diminution of heat did not arise from debility, the sympathetic was divided in the neck of the same dog, and in twenty-five minutes the temperature of the ear rose 3° cent. higher than on the sound side.

From these experiments the following conclusions are clearly deducible :—

1. The section of the nerves of sensation, in addition to the abolition of sensation, produces a diminution of temperature in the parts to which they are distributed.

2. The division of the motor nerves, together with the abolition of motion lowers the temperature in the paralysed parts.

3. The destruction of the sympathetic, which produces neither paralysis of sensation nor of motion, causes a constant and very considerable elevation of temperature.

4. When a mixed nervous trunk is divided, containing nerves of motion, sensation, and filaments of the sympathetic (the great sciatic nerve, *e.g.*), the whole of these effects will be produced—paralysis of motion and sensation, and increase of temperature; but the calorification will be less apparent from the counterbalancing effects caused by the paralysis of the nerves of sensation and motion.

5. This augmentation of the animal heat is a property peculiar to the great sympathetic.

§ II. *Description of the Phenomena of Calorification which accompany the Section of the Cervical part of the Sympathetic.*

In mammiferous animals, as the dog, cat, horse, rabbit, or guinea-pig, when the communicating filament between the inferior and superior cervical ganglia is divided about the middle of the neck, the evolution of heat is increased in the whole of the corresponding side of the head. The elevation of temperature begins instantaneously, and is so quickly developed that sometimes in a few minutes a difference of temperature amounting to 4° or 5° centigrade exists between the two sides of the head. The heat, which is quite appreciable to the hand, is best measured by introducing a thermometer into the nostril or ear of the animal. The extirpation of the superior cervical ganglia produces the same effects of temperature, only they are more rapid, intense, and lasting. In rabbits, after section of the sympathetic, the excess of calorification and sensibility is not observed beyond the fifteenth or eighteenth day; and in dogs it lasts about six weeks or two months. But after extirpation of the ganglia, the effects last almost indefinitely; in a dog the whole phenomena of increased calorification and sensibility were still very intense a year and a half after the removal of the superior cervical ganglion.

The difference of 4° or 5° (cent.) in these experiments is very remarkable as existing between the two sides of the face; but the temperature so raised is still nearly the same as that of the central parts of the body, as the thorax, abdomen, or rectum. Not unfrequently, however, the temperature of the ear, after extirpation of the sympathetic, rises to 40°, while the normal temperature of the rectum in the same animal does not generally exceed 38° or 39°. The whole of that side of the head which becomes warmer is the seat of more active sanguineous circulation, as is well seen in the rabbit's ear. But some days after, when the vascular turgescence has diminished, or even disappeared, the heat of the face still continues in a very marked degree. By means of incision, it may be ascertained that the increase of temperature, so marked on the exterior, extends to the deeper parts of the head, to the cranial cavity and cere-

bral substance ; and even the blood which returns from these parts is found to possess a higher temperature.

The section of the sympathetic changes the effects of external heat and cold upon the parts. When exposed to heat; the side which has been already warmed by the section of the sympathetic gains no more heat, while the sound side gradually warms, so that the two sides are restored to an equal temperature. When exposed to cold, on the contrary, the side on which the sympathetic is cut shows great resistance, and loses heat far more slowly than the sound side, so that the difference of temperature between the two sides is greatly increased, and may even amount to 6° or 7° centigrade. It would, therefore, be interesting to try the effects on hibernating animals to delay the winter sleep.

The greater resistance to cold is attended also by a sort of exaltation of the vitality of the parts, so that in an animal killed by poison or section of the eighth pairs, the side of the face where the sympathetic has been divided, retains a higher temperature, and preserves the involuntary movements and other signs of life to a later period than the rest of the body.

If the animals remain in good condition after section of the sympathetic, no oedema or morbid action of the nature of inflammation takes place. But if they fall sick, either spontaneously or in consequence of other operations, the nasal and ocular mucous membranes of the affected side become red and swollen, and secrete pus in great abundance. The inflammation of the conjunctiva described by Dupuy, John Reid, etc., is therefore not a normal but an accidental phenomenon, produced by the debility of the animal.

For those who may desire to verify the effects of calorification produced by section, ligature, contusion, or destruction of the cervical portion of the sympathetic, although the experiments are not difficult, the following conditions appear the best for success :—

1st, It is preferable to make the experiment when the surrounding temperature is low ; the difference of warmth on the two sides of the face will then be more marked. 2d, The animals should be vigorous, and in the act of digestion. 3d, We must avoid great suffering or agitation on the part of the animal during the operation. 4th, The phenomena are more marked and durable when the cervical ganglion is excised. 5th, The phenomena are more marked during the period of digestion, and more feeble during abstinence. It may be added, that similar effects as to heat follow the excision or section of the sympathetic in the thorax and abdomen.

§ III. *Effects of the Galvanization of the Cephalic end of the great Sympathetic Nerve on the Phenomena of Calorification in the Head.*

When the cephalic end of the divided sympathetic nerve is galvanized by a strong electro-magnetic machine, the whole of the phenomena caused by its section disappear, and reverse phenomena to a certain extent are produced. The pupil, from being contracted, becomes larger than on the sound side ; the eye, which was sunken, projects from the orbit ; the vascularization of the parts disappears, and their temperature falls below the normal standard. When the galvanization is interrupted, the phenomena produced by the section reappear, and on the renewed application of the galvanism, are again dispelled. The reverse phenomena may thus be repeated at pleasure.

To render the comparison more exact of the effects of galvanization of the sympathetic on the evolution of heat, very delicate thermometers (*thermomètres métastatique à déversement*) of M. Walferdin were used, by which fractions smaller than the 100th part of one degree centigrade could easily be appreciated. The following numbers are therefore only comparative :—

Experiment.—The right sympathetic was divided in the neck of a small bitch ; the temperature nine minutes after section was—left ear = 280, right ear = 287 ; difference 7.

The cephalic end was then galvanized, alternating one minute of galvaniza-

tion with a minute of repose. The diminution of temperature took place as follows:—right ear at the commencement = 287 ; after 7 minutes = 269 ; after 11 minutes = 255 ; after 15 minutes = 245 ; after 16 minutes = 240—that is, about 48 parts below the normal temperature.

The galvanization was then stopped, and the temperature gradually rose. Right ear = 240 ; after 16 minutes of repose = 245 ; after 19 minutes = 259 ; after 22 minutes = 268 ; after 25 minutes = 276—the temperature still continuing to increase.

During this galvanization, while the temperature fell on the right side, the left ear, which was normal, underwent the reverse change, increasing in temperature nearly as much as if the sympathetic on that side had been cut. Thus—

Before galvanization	{	Left ear, normal,	= 280
		Right ear, corresponding to the divided sympathetic,	= 287
After galvanization,	{	Left ear, normal,	= 286.5
		Right ear, sympathetic divided,	= 240

This remarkable antagonism of the calorification on the two sides is met with in other classes of these experiments, and especially in those with chloroform.

§ IV. *Effects of Chloroform on Calorification.*

The anæsthetic effects of ether and chloroform take place after destruction of the sympathetic ; but their action is a little retarded in consequence of the increase of sensibility in the parts.

1st Experiment.—In a small bitch, in which the sympathetic had been divided on the right side, the wound had healed up in fourteen days, but the phenomena of calorification persisted very manifestly—the right ear being warmer and more injected than the left. This animal was put completely under chloroform, and immediately the right ear rapidly lost temperature, and became cold and pale, while the sound left ear became warm and injected. The centigrade thermometer indicated—

Right ear, after section of sympathetic, during complete insensibility from chloroform, = 36°,8 c.

Left ear, normal, at the same moment, = 37°,2 c.

The chloroform was stopped, and the animal having recovered, the temperature after 1½ hour was—

Right ear, side of operation, = 37°,8 c.

Left ear, normal, = 34°,4 c.

The chloroform was again given, and at the moment of complete insensibility, the temperature on the two sides was again nearly equal ; right ear 37°,3 c. ; left ear, normal, 37°,8 c.

2d Experiment.—The right sympathetic was divided in the neck of a bitch, and the temperature compared by the thermometer of M. Walferdin.

1. Left side, normal,	{	Ear,	165
		Nostril, during expiration,	165,5
2. Right side, the sympathetic divided,	{	Ear,	177,5
		Nostril,	174,2

After insensibility from chloroform the results were—

1. Right ear, sympathetic cut, fell from 177,5 to 175,3

2. Left ear, nerve uninjured, rose from 165,0 to 174,3

Chloroform, therefore, acts very differently on the sound parts, and on the parts where the sympathetic has been divided.

§ V. *Relation between the Vascularization and the Calorification of the parts after section of the great Sympathetic.*

The section or excision of the great sympathetic gives rise to vascular tur-

gescence on the corresponding side of the head ; and the phenomena of circulation and absorption are increased in activity. Is the increase of heat to be attributed to the greater vascularity ? Without overlooking the intimate relations of these phenomena, the explanation appears insufficient to account for the great increase of temperature (6° to 7° cent.); and moreover, when the vascularity diminishes a few days after the operation, the temperature of the ear does not vary in a notable degree. Thus, in a rabbit, one quarter hour after extirpation of the right cervical ganglion, the temperature was, right ear, 39° c., left, 33° c.; next day after the vascularity had greatly diminished, the small vascular ramifications and capillaries being alone more apparent than in the right ear, the temperature was still, right ear, 37° c., left ear, $30^{\circ},5$ c. On the other hand, the increased vascularity after section of the fifth pair is accompanied by a diminution of temperature. When stasis of the blood in the ears of a rabbit was produced by ligature of the veins, the temperature was found to diminish ; but when the sympathetic was divided, the corresponding ear became warmer in spite of the congestion ; the artery was next tied, so as to confine the blood in the ear, and the temperature diminished a little, but was still higher than in the opposite ear. If the arteries be first tied, the parts become colder from the absence of blood ; but in this case the temperature is not raised by section of the sympathetic, showing that the calorification cannot be produced in parts where the vessels are empty of blood, although it may take place in parts where the blood stagnates, and independently of its renewal, as proved by the preceding experiment.

From these experiments, M. Bernard is opposed to the theory of the paralysis of the blood-vessels as the cause of the increase of vascularity. Indeed the carotid is generally observed to contract after section of the sympathetic, and the subsequent distension seems rather to arise from the increased afflux of blood. In galvanization, the contraction of the arteries and veins is caused by the deficiency of blood ; and, as already noticed, although the arteries return to their usual size, and the vascularity diminishes the day after the section, yet the increase of heat continues. In short, the phenomena of vascularity which succeed division of the sympathetic are active and not passive ; they are of the same nature as the vascular turgescence which arises in a secreting organ when it passes from a state of repose or inactivity to an active discharge of its functions ; they resemble the afflux of blood, and increased sensibility around a recent wound, or a foreign body in the living textures ; and no one in these instances would attribute the phenomena to the mere paralysis of the arteries.

Conclusion.—I have only wished, says the author, in this communication, to establish one point in the complex history of the great sympathetic, namely, that the section of the filaments or ganglions of that nerve possesses constantly the privilege of increasing the calorification of the parts to which it is distributed. These phenomena are only the exaggeration of what naturally takes place in the production of animal heat. In giving the means of increasing the production of heat and localizing it in external parts which are easily observed, I had the thought of rendering more accessible to our means of research the study of that important function which is still so little known, but which cannot be investigated elsewhere than in the greater or less activity of the chemical metamorphoses which the blood undergoes in the living textures under special influences of the nervous system.—*Gaz. Med.* Jan. 7, 14, 1854.

ON THE RESULTS OF SECTION AND GALVANIZATION OF THE GREAT SYMPATHETIC NERVE. BY M. BROWN SEQUARD.

The results differ in some respects, particularly in their interpretation from those of M. Bernard. The author attributes the effects of section of the sympathetic to paralysis of that nerve, and the effects of galvanization to the increase of its normal action, and he claims priority in regard to the contraction

of the vessels, and diminution of temperature which follow galvanization of the sympathetic.

I. Effects of Paralysis or Cessation of Action of the great Sympathetic Nerve, after its section or excision of the superior Cervical Ganglion.

1st, Contraction of the pupil, which, however, still acts on exposure to light. The author considers that the abundance of blood in circulation has a great share in the contraction of the circular fibrils of the iris. 2d, The muscles of the eyeball, eyelids, and face, are also contracted, causing the retraction of the eye within the orbit and the protrusion of the third eyelid (John Reid), and the alteration in form of the aperture of the eyelids, which Brown Sequard attributes to the contraction of the levator palpebræ superioris. The ear is more erect in rabbits from contraction of its levator muscles. 3d, The secretion of the tears and of the palpebral mucus is increased. 4th, Certain pathological lesions (inflammation of the eye, etc.) are sometimes produced. 5th, When the animal is killed, the latest voluntary and respiratory, as well as reflex, movements are observed on the side where the sympathetic has been divided. 6th, The blood-vessels are dilated on the whole side of the head; the afflux of blood is greater and the temperature is increased *in consequence* of the increased determination of blood (contrary to the opinion of M. Bernard.) 7th, The vital properties of the optic, auditory, and sensitive nerves of the face are increased. 8th, After death the motor nerves and muscles of the face and eye, including the iris retain their vital properties longer by one quarter or one half hour on the affected side. 9th, The cadaveric rigidity comes on later and lasts longer than on the healthy side; and putrefaction is later and less rapid. The whole of these phenomena, the increase of temperature and vitality, depends upon the afflux of blood which is caused by the paralytic dilatation of the vessels.

II. Effects of the Energetic Action of the great Sympathetic, produced by the application of Galvanism some time after its division in the Neck.

The pupil dilates; the eyeball returns to its natural situation; the eyelids open, the contraction of the muscles of the nostril, mouth, and ear, ceases; the vascularization diminishes by contraction of the arteries and veins; and the temperature and sensibility decrease.

III. Effects of the Energetic Action of the great Sympathetic, produced by the Galvanic Excitation of the Undivided Nerve, or immediately after its Section.

Several of the effects are the same as in the preceding case, but the following effects are more marked:—1st, If the animal be asphyxiated, the voluntary, respiratory, convulsive, and reflex movements cease in the wounded before the healthy side of the head. 2d, During asphyxia, the sensibility disappears first on the side operated on. 3d, After death, the excitability of the motor nerves, and the muscular contractility disappear soonest. 4th, The retina and iris lose their vital properties sooner; and 5th, The cadaveric rigidity is earlier, and less lasting, and putrefaction is more rapid on the side operated on.

Similar effects are produced by galvanizing the sympathetic filaments on the abdomen. The author maintains that analogous results may be obtained by the lesion of certain parts of the nervous centres, or after the section or galvanization of the cerebro-spinal nerves; and he concludes, therefore, that there is nothing special in the action of the sympathetic in these experiments.

Conclusions.—1st, The paralysis or cessation of action of a part of the great sympathetic nerve, after section or excision, is followed by dilatation of the blood-vessels, next by a considerable afflux of blood, an elevation of temperature, a manifest augmentation of the vital properties of the muscles and nerves of the face on the side operated on. 2d, The energetic action of the great sympathetic on the neck, when it is galvanized, produces the contraction of the blood-vessels, which is followed by diminution of the quantity of blood in

circulation in the face and ear, and decrease of temperature and of energy of the vital properties of the muscles and nerves of the side of the head which has been operated on.—*Gaz. Med.*, Jan. 21, 1854.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIII., 1853-54.

February 1, 1854.—J. Y. SIMPSON, M.D., President, in the Chair.

[PATHOLOGICAL MEETING.]

Occlusion of Cerebral Artery by a Cardiac Vegetation.—Dr Simpson mentioned that since the last meeting he had seen a case with Dr Alexander, the symptoms of which were, he believed, referable to the occlusion of a cerebral artery by a cardiac vegetation. The patient, who was near her confinement, on coming home felt indisposed, and called her servant to send for her husband. Complete paralysis of the right leg, arm, face, etc., supervened. The patient was quite intelligent though she could not speak. There were no symptoms of cerebral pressure, nor was the urine albuminous. A loud valvular sound, however, was heard over the heart, and it was ascertained that she had suffered from rheumatism about a year before. A symptom was well marked in this case, to which attention had been drawn by Mr Tufnell, viz., strong pulsation of the vessel on the cardiac side of the occlusion. On examining the two carotids, the left presented an extraordinary impulse.

Dental Apparatus of the Aztec Children.—Dr Reid exhibited casts of the dental apparatus of the "Lilliputian Aztecs" lately exhibited in Edinburgh, and gave a *resumé* of his paper on the subject, which, together with engravings of the casts, appeared in our last number.

Microscopic Characters of Hair an Indication of Differences in Race.—Dr Simpson mentioned that in a letter which he had received from Dr Daniel Wilson, late secretary to the Society of Antiquaries of Edinburgh, and now of Toronto, Dr W. stated that on passing through New York he had found the scientific world there busy in the investigation of the discovery of racial differences by means of the microscopic characters of the hair tubuli. He (Dr S.) was not aware whether this test had been applied in the case of the Aztecs. The passage in Dr W.'s letter ran as follows:—"Another ethnologist I met with, a Dr P. A. Browne, has been carrying on some curious microscopical observations on hair, by means of which he conceives he has arrived at an important type of varieties in races. The section of a Red Indian's hair is cylindrical, that of the white man oval, and its greatest diameter little more than two-thirds of the former, while the pure negro's is a very much elongated ellipse, nearly flat. The necessity of the hair submitted to examination being from a full-blood Indian or negro, is proved from his affirming that in the case of a half-blood negro, for example, the woolly hair is not found, as one might expect, more nearly approaching the oval; but it is a mixture of the pure elliptical wool, and the pure oval hair. The same, Dr Browne affirms to be the case with sheep crossed with animals having hair. The great difficulty must be, however, in procuring satisfactory sections of the hair, for nothing would be easier than for an unskilful operator to convert the same cylindrical hair into ovals and ellipses."

Dieffenbach's Operation for Ununited Fracture.—Mr Mackenzie exhibited a thigh-bone on which he had practised Dieffenbach's operation for the cure of ununited fracture, the drilling of the ends of the bone and the insertion of an ivory peg.

The patient, a seaman, had fractured his thigh between two and three years before he was admitted into the hospital under Mr M.'s care. He was treated in a public hospital in America, and he attributed the want of union to the limb not having been kept steady, the bandages confining the limb having been loosely applied.

The fracture was in the middle of the bone, and the lower fragment was retracted about an inch and a half on the inner side, and behind the upper. The limb was quite flexible at the seat of fracture, and was almost entirely useless as a means of support.

The ends of the bone were drilled with a small gouge worked by a carpenter's brace, and a peg, two and a half inches in length, was driven home with a hammer. The presence of the foreign body gave rise to such extensive inflammation and constitutional disturbance, that it was found necessary to remove it at the end of eight days. Profuse suppuration took place, undermining the whole deep textures of the thigh, and it seemed doubtful for some time whether the patient was not to sink from the effects of the operation. He rallied, however, and the suppuration gradually diminished. Firm osseous union took place, and at the end of some months the patient began to make use of the limb in walking. A sinus, however, continued to discharge matter at the seat of operation; and, after he had been walking for some weeks, he suffered from an attack of erythema of the limb; suppuration of the knee-joint followed, accompanied by hectic fever, and amputation was performed immediately above the seat of fracture.

The preparation showed the fractured ends firmly united by a large osseous deposit.

Mr Mackenzie believed this was the only case in which Dieffenbach's method had been practised in Edinburgh. The result was not encouraging as regarded its further trial, at least in the thigh. A considerable amount of success had followed the practice, in ununited fractures of the leg and forearm, in the hands of the London hospital surgeons; but the only other case in which, as far as he could find, it had been attempted in the thigh, had proved equally unfortunate; he alluded to a case by Mr Square of Plymouth, in which, as in the present case, it had been subsequently found necessary to perform amputation.

Abnormal Obturator Artery in Femoral Hernia.—Mr Mackenzie showed a dissection of a case of femoral hernia, in which the abnormal obturator artery had encircled the neck of the hernia, and in which he had had the good fortune to operate without wounding the vessel.

The patient was a female; the hernia, of the size of a hen's egg, had been strangulated for two days before she was admitted into the hospital. The sac was opened in the operation, and the stricture, in Gimbernat's ligament, divided as usual. A slight flow of venous blood took place on the stricture being divided. The bowel was very dark, but not gangrenous, and was returned into the abdomen. The symptoms were quite relieved, and the wound healed rapidly. On the eleventh day after the operation, however, a little fecal matter began to ooze through a small part of the wound, which remained unclosed; a preternatural anus was established. After a time the feculent discharge began to diminish in quantity, and for some time there seemed to be every prospect of recovery, when pulmonary symptoms appeared, and she died from phthisis six months after the performance of the operation.

The parts were removed for dissection. The peritoneal sac of the hernia formed the walls of the preternatural anus, and the obturator artery, with an accompanying vein, was found closely united to the edge of Gimbernat's ligament on the *inner* side of the neck of the sac. The obturator artery arose in a trunk, about half an inch in length, common to it and the epigastric, from the external iliac artery, and had been pushed, by the descent of the hernia, to the pubic side of the neck of the tumour.

It had been a disputed point among anatomists how far the common variety of the origin of the obturator artery endangered the vessel in the operation for femoral hernia; but it was generally admitted that the descent of the hernia between the femoral vein and the abnormal artery (as in this instance) was very rare.

Cases had been recorded where fatal hemorrhage had occurred in the operation, apparently from this variety of the artery, but Mr M. was not aware of any case having been recorded in which this variety had existed, and in which the vessel (as in the present case) had escaped being wounded in the operation.

The fortunate operation was of course mainly attributable to accident; but he had used the precaution, which he had always adopted in operating for femoral hernia, of introducing no more than the point of the knife within the stricture, and of using a slightly blunted cutting edge for the division of the stricture—a precaution which had been long ago recommended with a view of avoiding the accident in question.

Scirrhus of the Testicle.—Mr Mackenzie exhibited a specimen of scirrhus or hard cancer of the testicle, which he had removed a few days previously from a young man, who had been suffering from the disease for about twelve months.

The stony hardness of the gland had given rise, previously to the performance of the operation, to the belief that the tumour contained bony matter. While soft cancer of the testicle was of common occurrence, scirrhus was so rare, that some surgical writers had asserted their belief that the disease never affected the testis. The present instance was quite unequivocal. The section of the tumour presented a precisely similar appearance to a section of the common scirrhus tumour of the mammæ. Microscopic examination had confirmed the nature of the tumour as being cancerous.

Enlarged Mamillæ in the Male.—Mr Mackenzie showed two growths, which he had on the previous day removed from a young man of thirty years of age, and which precisely resembled in external appearance, and after section, well-developed female mammæ. Microscopic examination, however, had failed to establish any resemblance between the structure of the growths and that of the female breast. The enlargement of the mamillæ had been going on for two or three years, and could not be attributed to any special cause. The patient was a well-developed muscular man, and presented no peculiarity of conformation in any other respect.

Parasites in the Heart of a Dog.—Dr Paterson of Leith exhibited the heart of a dog, which had been transmitted to him from Shang-Hae in China. The cavities of it were filled with bunches of a species of *Ascaris*, bearing the characters of the *Ascaris Marginata*, which infests the intestines of that animal. It appeared from the account sent home with the specimen, that numbers of the hunting dogs kept there have died from the same cause, appearing quite lively and well, until when running, they suddenly made a bound in the air, and fell down dead. It moreover appeared, that it seemed entirely confined to the dogs which are fed in kennels on uncooked, or partially cooked, food; while those fed from the table, and who lived in the house, were exempted from it. Dr P. recommended that the dogs in the kennel should be fed alone on food well and carefully boiled, and he has since learned that the mortality has diminished.

Portion of Intestine discharged after Intussusception.—Dr P. also exhibited a portion of intestine, still invaginated, about ten inches long, which had been passed by a patient of his twelve days after the occurrence of the symptoms of *ilius*.

The patient, a girl, nine years old, made a perfect recovery. The disease began after an attack of dysentery. Much vomiting and great prostration of strength occurred at first; on the fifth day, a large quantity of bloody serum was passed by stool, and without any feculent matter; on the seventh day, a small quantity of feculent matter was passed, and occasionally till the twelfth, when a

perfect relief occurred to all the symptoms on the passage of the large portion of intestine shown to the Society. The treatment which was pursued, was the application of morphia to the blistered surface of the abdomen, and opium by enemata, as recommended by Dr Evans of Carlisle. (See Monthly Journal for January 1854.)

Dr Simpson said, that he had seen the case along with Dr Paterson, in which there was immense distension of the belly ; indeed, from the great enlargement of the cutaneous veins, the vena cava appeared to be obliterated by the pressure. Dr W. Thomson, in his collection of cases (see Edinburgh Medical and Surgical Journal, vol. xlv., p. 102), had only given two instances of the disease in patients so young, and none in which a double invagination had taken place, as, on examination of the preparation, appeared to have been the case in the present instance.

Peculiarities in Physical Diagnosis of Tubercle in the Lung.—Dr W. T. Gairdner exhibited a section of the lung in the patient alluded to by Mr Mackenzie, as dying, after the operation for hernia, from pulmonary disease. She had been seen once by Dr G., along with Mr Mackenzie, and most of the signs of a cavity, viz., intensely tubular respiration, loud bronchophony, and cracked-pot sound on percussion, were ascertained below the left clavicle. As the remainder of the side was perfectly dull, and gave no increased but rather suppressed resonance of voice, it was supposed that a large tubercular excavation existed in the left apex, and that the rest of the lung was atrophied or compressed by pleuritic effusion. The fact was different. The tubercular condensation was complete in the lower lobe, and in the lower part of the upper lobe, where the deficit had the form of *infiltrated* tubercle ; one or two small irregular excavations existed in the lower part of the upper lobe in front ; and the apex was comparatively free from tubercle, though compressed and presenting traces of much older and obsolete deposit at one point. No part of the lung could have admitted much air, but the cavities, and the comparatively pervious apex, must have given rise to the peculiar tubular respiration alluded to.

Degenerations of Liver and Spleen.—Dr W. T. Gairdner exhibited the following preparations and drawings, illustrative of various degenerations of the liver and spleen :—

1. Spleen, affected with the *waxy degeneration*, having the Malpighian sacculi enlarged, their contents having undergone the change described by Dr Sanders, in the Physiological Society's Transactions.

2. Spleen, similarly affected with *waxy degeneration*, the Malpighian bodies small and invisible. Both these preparations were from cases of phthisis.

3. Liver affected with the pure *waxy degeneration*, as described by Dr Gairdner in the Physiological Society's Transactions. (See Monthly Journal for last Month.) Three drawings were shown, illustrating different stages of this lesion, and showing the appearance of the organ under low microscopic powers.

4. Similar drawings of livers affected with pure fatty degeneration.

5. Three preparations of cirrhosis or "hobnail" liver, showing that this lesion occurs under a variety of forms, being in some cases uncomplicated ; in others, connected with the waxy and fatty degenerations respectively.

6. Two specimens from a case of cancer of the liver. The organ presented an exaggerated form of waxy degeneration, and the cancerous deposit had its cells and protein-elements similarly affected. One preparation showed a distinct cancerous nodule and a neighbouring lymphatic gland involved in the disease. In the other specimen from the same subject, the cancerous nodule was completely atrophied, surrounded by puckering, and transformed into calcareous matter.

Dr Sanders briefly corroborated the statements of Dr Gairdner with reference to the occurrence of the waxy lesion in the spleen, and for a fuller statement of his views referred to the Physiological Society's Transactions.

Dr Alison had listened with pleasure to the communication of Dr Gairdner. He would like to know whether he had traced any connection between this waxy

degeneration in the liver and other organs, and the atheromatous degeneration of arteries. The waxy liver he had chiefly met with in children.

Dr W. T. Gairdner, in reply, said, that atheroma seldom went along with waxy liver. There was no strict age to which the waxy lesion could be confined; he had chiefly met with it in children, and in persons below fifty years of age. As far as his observations went, the fatty liver was at least as common in later life, though it might be found at all ages.

Fatty Degeneration of the Uterus.—*Dr Simpson* exhibited the uterus of a woman who had died some days after delivery, to demonstrate its fatty degeneration under the microscope, and remarked that in the uterus after delivery this peculiar change occurred as a normal condition; the decrease of the organ in size being consequent on the fatty degeneration of the muscular tissues, and their subsequent absorption in the form of fat. This involution of the uterus commenced, he thought, at the mucous surface, and radiated outwards, being at first most marked internally.

Hydatids in Fluid removed by Tapping.—*Dr Simpson* showed a fluid containing a number of hydatids which he had procured on tapping a patient, who had previously undergone that operation without the escape of any such fluid. On admission into hospital she presented fearful distension of the abdomen, more so, indeed, than *Dr Simpson* had ever before observed. Fluctuation was present, more particularly in the centre of the swelling. On tapping, a large quantity of fluid containing hydatids, was, with difficulty, evacuated. The patient subsequently died, when the hydatids were found to have been contained in the cavity of the peritoneum, and external to a large ovarian cyst. Their origin was traceable to the peritoneal basement membrane, from which they sprung; and in their process of growth they probably projected into the cavity of the peritoneum, and subsequently became detached. They appeared to belong to the genus *astoma* of Goodsir, and many of them presented in their interior curious thread-like bodies resembling spermatozoa.

Enlargement of Superficial Lymphatics.—*Dr Simpson* exhibited a drawing illustrative of very great enlargement of the superficial lymphatic vessels over the pubis and the lower part of the abdomen, coming on after delivery, and resulting, he had no doubt, from obliteration of the deeper vessels.

Nigrities of the Skin in Females.—*Dr Simpson* submitted sketches of two females affected with deep darkening of the tint of the skin of the face, etc. In one the skin was as black as a negro's. In both instances the alteration had set in with severe headache. The disease had been described as nigrities, and though a similar normal change was ordinarily recognised in a slight form in the areolæ, and in the darkening of the centre line of the abdomen in puerperal patients, in the cases shown the depth of tint was distressing.

Case of Contracted Pelvis.—*Dr Simpson* had lately received from *Dr Balfour*, from India, the skeleton of a patient who had been affected with rickets, and who, after being ten days in labour, had undergone the Cæsarean section. Death ensued. The pelvis was very much contracted; indeed more so than in any specimen contained in his museum. In illustration, he showed the pelvis of the last case of Cæsarean section in Scotland. (He corrected himself, however, as he learned that the operation had been performed by *Dr Laurie*, of Glasgow, within the last few days). In the case of *Dr Nimmo*, the rounded sacrum projected so far forwards that it was at first mistaken for the foetal head. He also showed a cast of the pelvis of the case in which *Professor Dubois* had performed the last operation of the kind in Paris, and which presented an instance of that very rare deformity, the double oblique pelvis of *Nægelè*.

Cancer of Body of Uterus.—*Dr Simpson* showed two cases of organic disease of the uterus in which cancer had attacked the body and fundus of the organ,

leaving the cervix entire. Rupture had in one case ensued, and death took place from peritonitis. This variety was not described in obstetric works; but he believed it was common, as he had seen a number of instances of this form of cancer of the uterus.

Extensive Atheromatous Disease.—Dr Haldane, pathologist to the Royal Infirmary, exhibited the morbid appearances of a case in which the whole arterial system was affected with atheromatous and calcareous degeneration. Calcareous matter was deposited in the aortic valves; the coronary arteries were ossified, and nearly the whole of the inner surface of the aorta was covered with cretaceous scales. Some of these scales were as large as sixpences, and were but loosely attached to the vessel. The disease was most advanced in the lower part of the abdominal aorta. All the vessels given off from it were in a similar state. Both of the common iliac arteries were similarly diseased, but not to quite so great an extent. The right external iliac was not obstructed, but the internal iliac was dilated and occupied by a solid clot, so that it was completely impervious. At the bifurcation of the left common iliac, there was a clot which extended down and completely occluded the internal iliac, and partially obstructed the external. A director could be passed down the latter.

In this case there was ulceration and partial sloughing of the coats of the bladder, depending no doubt on the obstruction to the circulation through the internal iliacs, the external iliacs being pervious, there was no affection of the lower extremities. At the lower end of the clot, in the left internal iliac, was a calcareous scale. This had perhaps been washed off the wall of the aorta or common iliac, and, not being able to pass further down, was the cause of the obstruction.

In the same case a large calcareous mass, the size of a walnut, was found in the liver, and some small cretaceous concretions in the substance of the kidneys.

Case of Contracted Aorta and Additional Pulmonary Valve, by Dr H.—Aorta very small, barely admitted point of forefinger; valves healthy; pulmonary arteries fully three times as large; mitral valve somewhat contracted; tricuspid rather dilated. No direct communication between the right and left sides of the heart; ductus arteriosus impervious. There were four sigmoid valves in the pulmonary artery, one much smaller than the others, but having a distinct corpus aurantii.

The subject from whom the preparation was taken was a woman aged 31. She had suffered from bronchitis and great difficulty of breathing. There must have been a permanently distended state of the venous system as compared with the arterial.

In general when the aorta is so much contracted, there is a communication between the two sides of the heart, either by a pervious foramen ovale or ductus arteriosus, or by an opening (from arrested development) in the septum of the ventricles. In this case no such communication existed.

CORRESPONDENCE.

ON THE REGISTRATION OF FACTS IN PATHOLOGY. BY DR T. K. CHAMBERS.

To the Editor of the Edinburgh Monthly Medical Journal.

Glasgow Royal Infirmary, Jan. 25, 1854.

SIR,—As the following correspondence may be of scientific interest to those engaged in extensive pathological investigations, I beg you will give it a place in your valuable journal, and at the same time allow me an opportunity, through the same medium, of acknowledging the obligation I am under to the high authority whence I have elicited the information.

The following letter is one which could only be replied to at length, and as Dr Chambers has so fully entered into the details of the machinery from which his valuable "*Decennium Pathologicum*" has been compiled, I am sure it will serve

as a valuable source of information to those engaged in similar pursuits.—
Yours very truly. Wm. AITKEN, M.D.

From Dr Aitken to Dr Chambers.

Glasgow Royal Infirmary,
January 13, 1854.

SIR,—The perusal of your valuable papers entitled “Decennium Pathologicum,” has led me to take the liberty of writing for the purpose of consulting you regarding the registration of facts in pathology.

The results you arrive at in these contributions, as well as the ends you merely indicate as requiring to be yet made out, appear to be of so much importance to mankind and to the science of medicine as it ought to exist, that I trust you will overlook the unusual liberty I have (as a stranger) thus taken.

The appointment of a museum curator to this hospital having been made about eighteen months ago, a wide field is here open for recording facts in pathology, and I, as the individual then appointed, have the pleasure of recording such facts. The following is a statement of my present mode of registration. I merely make a record of the date of each post mortem examination made by myself, with the name, age, and reference to the journal of the ward where the history of the case may be found, then a short heading which states the organs that are most obviously the seat of disease, arranged in the probable order of invasion as gathered from the history of the case; and lastly, a statement follows in detail of all the morbid appearances. Thus far the method is in accordance with that taught me by Dr J. H. Bennett.

But, in addition to such a record, it appears to me desirable to have a system in which the results obtained as above indicated may be at once tabulated in some permanent form.

I have not been able to make up my mind as to what form such a table should take; and as you have given so much attention to this subject, I hope you will give me the result of your experience, so that in the end, by a similarity of machinery in operation, I may obtain results more or less similar to those you have recorded.

If, therefore, it is not drawing too much on your valuable time, I shall esteem it a great favour if you can furnish me with any form of a daily tabular register which might be adopted, so as to include the more important facts obtained in the course of a post mortem examination; or of any other form of table you may be in the habit of using at St George’s Hospital for pathological purposes.
—I am, Sir, yours truly,

WILLIAM AITKEN, M.D.

From Dr Chambers to Dr Aitken.

1, Hill Street, Berkeley Square,
January 20, 1854.

SIR,—I shall be quite repaid for the trouble of answering your letter, if, by so doing, I can encourage you to carry on the work of Hospital Registration which you have begun. On this instrument must our chief reliance be placed in the present day for curbing the rampant invasions of guess-work (falsely called theory) on the realms of science, and for rooting out traditional error.

In reply to your inquiries, I will first tell you the nature of the materials whence are derived the papers I have published partly in the “Medical Times” and partly in the “Medico-Chirurgical Review,” under the title of “Decennium Pathologicum.” The record of autopsies at St George’s Hospital is kept in a mode precisely similar to that which you have adopted at Glasgow. In a printed heading, are placed the date, name, age, and sex, the number of hours after death when the corpse is opened, the name of the medical officer who attended the patient, and the most obvious anatomical causes of decease, with a reference to the general register, and to the case (or ward) book. The details of each examination occupy, as a general rule, one side of the folio leaf, and are written by the curator of the museum. On the opposite page is a sketch of the *præ mortem* history drawn up by the registrar from the case-books kept by the clinical clerks.

I did not begin to examine these records with a view of making a statistical use of them till they had been kept nearly ten years, by the end of which time the number of examinations had exceeded two thousand. The observations were not conducted on any plan designed for the elucidation of special points, but were simply noted down as a matter of routine duty. Scientific impartiality was thus secured, but the benefits of enthusiasm were missed.

Now, I think it may be considered almost as an axiom, that no *discovery* of organic laws can be made by accumulation of instances, unless they have been collected in anticipation of the said law or its converse. The theory must be formed in embryo before induction can claim to supply it with the food necessary to growth. "The framing of hypotheses," says Dr Whewell, "must be for the enquirer after truth, not the end, but the beginning of his work."

It was not, therefore, to be expected that the discovery of any new law would come out of the arrangement of the records at my disposal. But still I might with justice hope to elucidate matters which are always more or less before the mind of a scientific medical man in all cases, such as the mutual connection of various conditions of the viscera, the modifications arising from age, sex, station in life, and (when similar series of statistics are compared together) from different localities and circumstances. To these points, then, I directed my attention.

I will now tell you the plan I adopted for the methodical arrangement of the observations. I first made a complete alphabetical index of all the morbid appearances without exception, referring to the volume and page in which they are found. The appearances are arranged *anatomically* under the name of the organ affected, with occasional cross references to pathological states.

I give a few instances of headings taken at random:—

PERICARDIUM,

- Accidental injuries to,*
- Laceration*; v. 233—vi. 148, etc.
- Air in cavity*; i. 79.
- *in membrane* (emphysema); iv. 264.
- Blood in cavity*; ii. 90—iv. 86, etc.
- *in membrane* (ecchymosis); ii. 2-60, etc.
- Bloody serum in cavity*; i. 35-49, etc.
- Clear serum in cavity*; i. 11-14-15-20-21-26-27-40-130-
¹180-190, etc.
- Clear serum and adhesions combined*; i. 130-180.
- Clear serum in subserous cellular tissue* (œdema); iii.
158—iv. 102, etc.
- Old adhesions*:—
 - (A) *external*; i. 60—ii. 41, etc.
 - external, containing bone*; iii. 105—iv. 39, etc.
 - external, containing tubercle*; x. 76.
 - (B) *internal*; i. 52, ²63-85-86, etc.
 - internal adhesions broken loose*; ii. 71-103, etc.
- Pus or recent fibrine*:—
 - (A) *externally*; ii. 41—viii. 253, etc.
 - (B) *internally*; i. 9-36, etc.
 - etc. etc. etc.

FAT (NORMAL).

In excess or defect, see ADIPOSE TISSUE.

FATTY DEGENERATION.

Of heart; see HEART, fatty degeneration of.

Of kidneys; see KIDNEYS, *fat in substance*; *fatty deposits visible to naked eye*.

Of liver; see LIVER, fatty degeneration of.

CARIES.

See ATLAS,—AXIS,—CRANIUM,—EAR,—FEMUR, etc. etc.

Caries of.

¹ In the underlined cases the quantity was large, distending the membrane, or is stated to exceed four fluid ounces.

² In the underlined cases the adhesions were universal.

I subjoin instances of these tables —

BRAIN.														(Year) 1841. (Page) 2.													
Num-ber of Case.	Age.	Sex.	State of Brain.										Spinal Cord.		Chest.		Abdomen.			Other lesions	Causes of Death, and Observations.						
			Membranes.					Substance.																			
			Arte-ries.	Ver-tices.	Cavity of Ara-chnoid.	Pla Mater.	Congestion and Thick-ening.	Soft or wat-Tubercles, Generally or Locally.		Clot in Rt. thalamus.		Dil. and thick-ened.	Rt. lower lobe he-patis.	Spleen.	Liver.	Kid-neys.											
20	34	M.	H.	+ Se-rum.	Dry.	Con-gested		H.	Clot in Rt. thalamus.	Not exam.										Apoplexy, paralysis of left side, sloughs on sacrum. (Drunkard.)							
100	4	F.	H.	+ Se-rum.	+ Se-rum.	Tub.	Congest.	Locally soft.	Tub.	Not exam.		H.		H.	H.					Acute hydrocephalus (hereditary tubercu-losis).							

TUBERCULOSIS.													
(Year) 1843. (Page) 6.													
Num-ber of Case.	Age.	Sex.	Head.	Lungs.		Bron-chial Glands.	Adhesions of Pleura.		Acute Pleurisy.		Hepat. of Lung.		Causes of Death, etc.
				Right.	Left.		Right.	Left.	Right.	Left.	Right.	Left.	
				Scrof. disease of bones.	Vom.	Vom.	×	×	×	×	×	×	
234	34	M.				?							Phthisis Pulm.
247	42	F.				?							Dropsy. No Emaciation.

+ denotes "large quantity of;" — "small quantity of;" × the presence of lesion varied.

Besides this I made a series of tables of the cases of disease of the principal organs, such as the heart, kidneys, brain, liver, and of certain pathological states such as tuberculosis, cancer, pyæmia, fevers. These tables were designed to exhibit the connection of the morbid states of the several organs and their relation to one another, and also as a means of checking off and correcting the alphabetical index.

The paper on which they are drawn out is of course larger, so as to allow more room for description if necessary; and various little modifications and conveniences will suggest themselves to any one who pays attention to their construction for his own use.

It is easy to see how, from these tables, the statistics which I have drawn up naturally flow.

I have been explicit, and perhaps tedious, in describing the machinery I employed, because it seems to me capable of fulfilling the want you express of some form of tables for *daily use*. It would be just as easy, nay easier, to fill up the tables and index after each *post-mortem* examination, as it was after the accumulation of ten years; and I think, if I was in your place, I should adopt a somewhat similar plan. You will find, as you proceed, subjects for special observation develop themselves to your mind, and there will be but little labour in going over your previous work, so as to bring it up as far as possible to your new requirements; or if that does not give sufficient information, it will be easy to commence a new series of tables for such special subject.

Many, indeed most, cases will of course, under this plan, be repeated in several tables, but in spite of additional toil involved, I should advise you to repeat each time the sex, age, and chief cause of death, and also the state of the several viscera; as such a course will save you trouble in the end, when you come to condense your table into statistics.

If the index, tables, and detailed records proceed at the same pace as you apparently intend they should, they will become much more valuable for scientific statistics than the materials on which I had to work. For you will have the great advantage of sketching out beforehand the laws or facts which you design your statistics to prove or disprove, and will be enabled to collect them with a clearly defined object in view; you will thus not be liable to the constant disappointment of finding that the very points on which you require most exact information are slurred over or omitted, while the page is filled up with details valueless to your purpose.

It has been suggested to me that statistical ends would be better answered by having the pages of the *post mortem* book ruled, so as to have a proper place for the description of each organ; but I think that the space which a *perfect* sheet of such a sort would occupy, invalidates any advantage it might possess. A moderate sized sheet would sadly cramp your delineations, and you would be constantly tempted to invade the blank spaces left by healthy organs, to the utter destruction of good book-keeping. For this reason, at St Mary's Hospital (where I believe, during the three years of its existence, registration has been more thought of than at any kindred institution in London), we have adopted the simple form of fatal-case-book, which is used at St George's and by you. The account of the autopsy is written by the curator of the museum, and the case on the opposite page by the medical and surgical registrars. A reference is made to the general register of all patients, and to the ward-book of the physician or surgeon in attendance—which ward-book, by the bye, is, according to our laws, the property of the hospital.

As a proof of the advantages to be gained by carrying on mere *post mortem* records, even unaccompanied by accurate clinical observation, with a certain design before you, take any one subject, and think what a variety of valuable facts might be derived from observation of the simplest kind requiring scarce any scientific acumen or authority to form opinions. Take, for instance, tuberculosis of the lungs—a most practically useful series of statistics might be made by weighing the tubercular tissue separate from the healthy, and finding with how little of the latter life is supported.

- by observing in *every case* the larynx and trachea, and ascertaining their state, where symptoms of their being diseased were absent during life ;
- by making a microscopic examination of a transverse section of the mucous membrane of the stomach, and finding whether or not the tubular structure is healthy in phthisical persons ;
- by observing the quantity of intestine involved, when death takes place by diarrhœa, and ascertaining how far the violence of the diarrhœa is dependent on the quantity or on the part involved ;
- by continuing Dr Boyd's observations on the weights of organs, not omitting in each case to weigh and measure the whole body as a medium of comparison ;
- by ascertaining whether the amount of emaciation has any relation to the death ;
- by measuring with a pyramidal graduated ruler the areas of the pulmonary arteries and veins, and finding if they are altered by diseases which chronically obstruct the circulation through the lungs ;
- a similar measurement with a smaller ruler of the arteries and veins of healthy and hypertrophied parts, for example of the coronaries of the heart, would be valuable and unique, as also of the arteries and veins of inflamed parts.

But the monstrous length to which this letter has trailed, warns me to stop my dreamy operation of making suggestions for work. I could wish that my student days were to come over again, that I might do something more than suggest. In default of the granting of this wish, let me conclude with one which I am more sure to see accomplished, viz., that your conscientious devotion to science will receive its just reward.—Believe me, faithfully yours,

THOS. K. CHAMBERS.

ON THE ORIGIN OF CHOLERA IN DUMFRIES.

To the Editor of the Monthly Journal of Medical Science.

SIR,—Having observed in your Journal of the present month that some misunderstanding exists as to the localities in which the first cases of cholera appeared here in the autumn of 1832, I have carefully examined the records of our local Board of Health, which was constituted in the spring of that year by the Lords of the Privy Council—of which Board I was myself a member—and find that the first case was that of Mary Paterson, who kept a small shop, in which she lived and slept, in English Street ; and that she had been suffering from a severe bowel complaint for a week previous to the 15th September, when she received medical aid, and was reported to the Board as ill of cholera ; and that she died about two o'clock on the following morning.¹ I also find that the second case occurred on the 16th, and the third one on the 17th Sept., in a house directly opposite to Mary Paterson's shop. The names of the individuals were William Bell and John Paton, who, being advanced in years, both rapidly sunk, and died. They were attended by Mr M'Lauchlan² and myself. The fourth case is also recorded as having occurred in English Street, and was attended and reported to the Board by Dr Grieve. The patient's name was Robert Hume ; but the result is not stated.³ In fact, the disease was confined

¹ There were some miserable lodging-houses, for the reception of vagrants from all parts of the kingdom, adjoining this poor woman's shop ; and so great was the anxiety of her neighbours to witness her sufferings, that two gentlemen and a town officer stood at her door till within an hour of her death, to prevent them annoying both her and her medical attendants, Drs Wood and M'Cracken ; the latter of whom shortly afterwards fell a victim to the disease.

² This gentleman, whose zeal and exertions in the mitigation of human misery could not be excelled, died here of Asiatic cholera, December 10, 1848.

³ About this time a waterman, of the name of Brannaghan, was also attacked, and died in a few hours, in what is called Paddy's Close, the entrance to which is only five or six yards from Mary Paterson's shop, where the first case terminated fatally. This man's case, however, by some mistake, is not recorded, although I am aware that several medical practitioners visited him. It is stated, however, in the Board's minute of September 19, that "The medical gentlemen recommend that the close named Paddy's Close be thoroughly cleaned, and the houses washed with hot lime."

to English Street until the 20th September, when a man was taken ill in the Globe Inn Close, High Street, which is not more than a minute's walk from English Street, and soon after removed to the Cholera Hospital—which was opened for his reception, where he died early the following morning. Three cases are next recorded in St Michael's Street, all of which are reported as having occurred on the 22d September. One of these was a poor man from Kilmarnock, who had taken up his abode in a small lodging-house at the south end of that street, a day or two before. He had been ailing on the previous day, but his symptoms were not urgent or alarming, until the morning of the 22d, and he died that night. The other cases alluded to were those of a Mrs Caig and her daughter, Mrs Nielson, who lived together in a house at the opposite end of the street. Mrs Caig being the first seized, Mrs Nielson, was her principal, if not her only, nurse; and the mother had only been dead a few hours when the daughter was taken ill, and about midnight of the 22d she also was a corpse. I saw all these cases by request of the Board; but they were not under my care. Dr Thorburn and Dr Wood were the medical attendants of those in St Michael's Street, and Dr Charles Bell, now of Edinburgh, visited the man in the Globe Inn Close, and afterwards attended him in the hospital. The disease now became pretty general in other parts of the town, and on the 24th reached Maxwelltown, which stands on the opposite bank of the Nith, where, in one short month, it hurried to the grave 103 of the inhabitants.¹

In 1848, the first case of cholera appeared in a confined close in High Street on the 15th November, and the second case in the same house on the 18th. They were both fatal—the first in twenty-one hours, and the second on the third day. Cases soon afterwards occurred in various parts of the town, and also in Maxwelltown; and before the end of December the deaths in Dumfries alone amounted to 311.

Although I have long been of opinion that, when cholera prevails, it is the locality, and not the sick, we have to fear, I must candidly admit, with the preceding facts before me, to which I beg to call your attention—more especially those relating to the first visitation in this quarter, that the progress of the disease during the first eight days was, to use the favourite term of Sir Gilbert Blane, more like one communicable by emanations from the human body, than one entirely owing to atmospheric influence, or any other influence with which we are acquainted. It is only, however, by carefully recording, and properly arranging, well ascertained facts, that we can ever expect to come to a correct and satisfactory decision on the contagious or non-contagious nature of Asiatic cholera as it appears in this country.—I am, etc., etc.,

ARCHD. BLACKLOCK,

Late Surgeon, R.N.

Dumfries, Feb. 13, 1854.

MEDICAL REFORM.

It was stated by Mr Brady, M.P., at a meeting of the South London Medical Association, that he had been informed on good authority, it was impossible from the present distracted state of her Majesty's counsellors, to bring them to think of any measure of large and comprehensive reform. But he impressed upon those present the certainty of a simple Registration Bill being passed in three months, if the profession would agree to it. This bill he has caused to be prepared, and it is now published in the weekly journals. The object is to register every physician, surgeon, and apothecary, in England, Scotland, and Ireland, by a registrar in each of those parts of the kingdom. Lists of the qualified practitioners are to be published, and penalties enjoined on those who falsely represent themselves as medical men. This is the substance of Mr Brady's bill; but we are at a loss to understand of what advantage it can be to the profession, for the quacks have only to deny that they represent themselves as medical men in

¹ The total number of cases reported to the Boards on both sides of the river, between 15th September and 27th November, was 1094, of which 524 died.

order to escape its provisions ; while a mere list, of tolerable correctness, is now available to the public by means of the directories. A botanist, herbalist, galvanist, frictionist, homœopathist, or mesmerist, is not necessarily a qualified practitioner. If not registered, the law does not touch him ; if he be, it is equally ineffectual. How, then, does this bill operate in putting down quackery ? But, whilst it is likely to accomplish no good, it is certain to do an enormous amount of mischief. The bill positively obliges those who are now practising with honour and reputation to undergo fresh examinations and pay for new diplomas, in order to meet that great grievance of varied privileges in different parts of the kingdom, which it does not venture to attack. Let medical men ponder on the following clause :—

“XVI. And whereas there are many Physicians, Graduates of British and Irish Universities, now practising in England, who have not a legal Qualification to practise in England and Wales : Be it enacted, That they shall be allowed to continue to practise as Physicians for *One Year* from the Date of the passing of this Act, in order to afford them an Opportunity to obtain a legal Qualification, and until such Qualification be obtained they are hereby authorised to be provisionally registered according to the Provisions of this Act for the said period of *One Year*, and no longer.”

Can it be imagined by any man attempting to legislate for the profession that the numerous graduates of the Scotch and Irish Universities, now practising in England, are to be made, by this useless bill, to present themselves to the London boards, and pour gold into their coffers for nothing ? for the mere honour of being registered but not protected ? The idea is too absurd to be entertained for a moment.

If we are to have a reform bill at all let us have no quarter measures. Let us have one, at least, that shall meet the three great evils which oppress us. Equal privileges, equal education, and protection against quackery, must be the indivisible and only basis for the interference of the legislature.

On going to press we have heard, from an authentic source, that the above clause is to be modified, but in what manner is unknown.

HIPPOCRATES AND BACCHUS.

A Society has been formed for the purpose of procuring the legislative prohibition of the sale of fermented liquors, except for medicinal purposes. Should a law to that effect be enacted, the only question will be, What purposes are, or are not, medicinal ? But that question will be rather what is termed a poser. For example, melancholy is a disease, and if brandy and water is permitted to be taken in cases of mental depression, there will be no end to keeping spirits up by pouring spirits down. A dram is a popular and efficacious remedy for a pain in the interior, but if that is to be allowed as an excuse for taking it, the majority of the population will be constantly having stomach-aches. Some people may think to settle the difficulty by referring it to medical men, who however, naturally, would be too happy to prescribe liquor to healthy applicants in the hope of getting them for real patients. An English Liquor Law treating strong drinks as drugs, would at any rate be a boon to the medical profession. For one draught that a physician now prescribes, he would have to prescribe thousands, if in him were vested the privilege of prescribing draughts of ale. One wonders in what terms a doctor would frame his recipe for the complaints that deans and chapters, for instance, are apt to be troubled with after dinner. Perhaps in some, such as the following :—*R. Vin : Alb : Hispan. seu Rubri quant. suff : cujus sumatur cyathus subindè, urgente siti :* which would be a technical order for unlimited port or sherry. Or the generous medicine might be ordered thus : *R. Vin : Cujusvis Oij. gradatim post prandium, hauriend : et rep' si opus fuerit.* Another prescription might be given for a bowl of spiced wine, concluding with the direction, *Fiat Episcopus, statim sumendus.* Small glasses of brandy might be prescribed *pro re natâ*, and an alcoholic mixture might be ordered by the for-

mula *Misce pro pileo dormitorio, horâ somni sumend.* In surgery windows we should see, in great gilt letters, such inscriptions as *Cerevisia Londinensis, XXX Opt :* or *BARCLAY PERKINORUM et Soc. Integra.* A new feature would have to be introduced into medical education, and the examiners at Apothecaries' Hall would require candidates for their diploma to understand the Beer Engine.—*Punch.*

Mr Punch's prescription of Bishop will remind the German reader of the examination of that distinguished theological student Hieronimus Ioba, who, when a candidate for orders, answered the first question, "Quid sit Episcopus?" as follows:—

Ein Bischof ist, wie ich denke
Ein sehr angenehmes Getränke
Aus rothem Wein, Zucker und Pomeranzen saft
Und wärmet und stärket mit grosser kraft.

which may be thus rendered :

A Bishop is, as I think
A very pleasant sort of drink
Of red wine, sugar and lemon-juice sour,
And warms and strengthens with mighty power.

VARIETIES.

ROYAL DISPENSARY OF EDINBURGH.—The annual general meeting of this Incorporation was held in the Council Chambers on Saturday, the 28th Jan., the Right Hon. the Lord Provost in the chair. The applications for relief during the year had declined by 335 as compared with 1852, when there were 9671 relieved, which is attributed to the advance in the price of labour and the abundance of employment. The numbers prescribed for, and attended, by the medical officers and their pupils, either at the Dispensary or at their residences during the past year, was 7732. The Directors are happy to report a decrease in fever, of 52 cases as compared with the preceding three years, Small-pox, scarlet fever, and measles, however, had been prevalent, particularly the latter, 127 cases having been attended to last year. In erysipelas and whooping-cough there has been a decline, but in cases of catarrh and bronchitis there was a slight increase. Affections of the throat and chest have also been on the decline, but diarrhoea and dysentery cases show a considerable increase as compared with 1852. The cholera, says the Report, has had little, if anything at all, to do in swelling the Dispensary registers, as, owing to the precautions taken by the various Parochial Boards, the poor of every class, when attacked by this dreadful disorder, were immediately attended to by the officers of those Boards. The vaccinations during the year reached 491 in number, thus exceeding the past year by 41 vaccinations, and being by much the largest number known at the Dispensary for many years previous. The statement of accounts showed a revenue, during the year, of L.271 : 3 : 4, and an expenditure of L.289 : 9 : 1,—excess over income, L.18 : 5 : 9.—*Scotsman.*

NUMBER OF CHEMISTS AND DRUGGISTS IN ENGLAND AND SCOTLAND.—The number of patent medicine venders for 1852 was 8379 in England, and 604 in Scotland; of whom the proportion of chemists and druggists is about 7000. The number of members of the Pharmaceutical Society is about 3500.—*Pharmaceutical Journal.*

THE PUFF BIOGRAPHICAL.—"Owing to the absence of the artist on the Continent, we have been unable to obtain a portrait of Dr W. Stokes completed in sufficient time for our present number. It is to be engraved on steel, and we expect to be able to publish it in our May number, in continuation of the Gallery of the living eminent Physicians and Surgeons of Ireland which we commenced last year."—*Dublin Quarterly Journal.*—[We trust Dr Stokes does not lend himself to this proceeding.]

ASSISTANT-SURGEONS IN THE NAVY.—We regret that Colonel Boldero's motion in the House of Commons (23d ult.), to afford additional accommodation to the above class of officers, was rejected by a majority of 216 to 104. It is, however, to be feared, that the assistant-surgeons have not been true to themselves, as Sir James Graham stated that for the 35 vacancies of the present year, he had received 109 applications. Let the profession clearly show that naval appointments are at a discount, and the Admiralty will soon concede the desired accommodation.

THE BEARD AND MOUSTACHE MOVEMENT.—Medical men have come forward to prove, that to many artizans the moustache was invaluable as a supporter of health; and that, in conjunction with a beard and whiskers, it prevented the attacks of many maladies that afflicted those whose avocations were carried on amid dirt, dust, grit, filings, and smoke, besides being of great service in neuralgic and pulmonary affections.—*Illustrated London News*. In Edinburgh an Association has been formed, which as soon as it numbers 500 citizens, is to resolve on a certain day to abandon the razor, and to meet together three months afterwards, to enjoy themselves, with beard and moustaches. It is suggested that the razors should be sent to the British Museum as relics of a *barbarous* age.—*Scottish Press*.

THE ARITHMETICAL SCHOOL OF CRITICISM.—The astute editor of the Dublin Quarterly has hit upon an easy way of reviewing books. He counts the pages and lines, maintaining that this plan "sufficiently indicates the minuteness with which subjects are investigated, as well as the care which has been bestowed on their discussion." This supposed brilliant discovery, however, is only a clumsy plagiarism from *Tristram Shandy*, as will be at once recognised by the following quotation:—"And what of this new book the whole world makes such a noise about? Oh! 'tis out of all plumb, my Lord,—quite an irregular thing!—not one of the angles at the four corners was a right angle. I had my rule and compasses, etc., my Lord, in my pocket. And for the epic poem your Lordship bid me look at—upon taking the length, breadth, height, and depth of it, and trying them at home upon an exact scale of *Bossu's*—'tis out, my Lord, in every one of its dimensions. Excellent critic!"

ENGLISH LAW AND MEDICAL PRACTICE.—A respectable London surgeon, Mr Housley, is called in to set the fractured thigh of an infant—gangrene ensues, and the limb is amputated. The friends bring an action against the surgeon, and the jury give a verdict of L.250 damages with costs, which involves loss of practice and destruction of professional character. On the other hand, a bone setter, named Thomas, bandaged the leg of a man called Crowley, so tight, that gangrene also came on and the leg was amputated. In like manner an action was raised, but because "he did not hold himself out as a person of skill and knowledge, nor wished to be considered as such," he was triumphantly acquitted, to the increase of his reputation among the ignorant community. Again, a fellow named William Hobson Palmer, described as a *botanist*, is distinctly proved to have poisoned a woman with enormous doses of lobelia and cayenne pepper. But as it was also proved that he was wholly ignorant of medicine, *therefore* he was acquitted under the direction of the presiding judge. Such is English law! Need we be surprised that quackery should flourish?

SINGULAR DINING-ROOM.—Professor Owen was entertained on the 30th of Jan., at dinner, in the model of the *Iguanodon* (the largest extinct British quadruped) at the Crystal Palace. Twenty-one gentlemen were accommodated in the interior of the gigantic animal. Its length is 35 feet, and the girth round the body about 25 feet.—*Edinb. Courant*, Jan. 3, 1854.

NEW HOTEL DIEU IN PARIS.—The present gigantic alterations going on in Paris, embrace the formation of a new Hotel Dieu on the north side of Notre Dame, which, like the present hospital of that name, is to contain 800 beds, and will cost altogether about fifteen million francs.—*Journal de Méd.*

THE ZULUH CAFFRES AT THE JARDIN DES PLANTES.—The Zuluh Caffres, lately exhibited in London, are now exciting much curiosity in Paris. On visiting the animals in the Garden of Plants, it was observed that some of them exhibited marked terror, whilst others were evidently enraged. The Caffres, on the other hand, remained impassive, with the exception of one, who, on seeing the lion, grasped his bow and arrows, as if about to use them. This reminded the keepers of an incident which happened two years previously, when a young savage from North America attempted to lodge an arrow in a panther, and was with some difficulty prevented from doing so.—*Gaz. Médicale*.

NEW CHILDREN'S HOSPITAL IN PARIS.—Her Majesty the Empress of the French, in her solicitude for the necessitous classes, has induced the government to realize a project having for its object the benefit of unfortunate mothers whose children are attacked with disease. The prefect of the Seine, therefore, is busied with the construction of an hospital, to contain 400 beds, to be situated in the Faubourg St Antoine, where children will be received on the same condition as in the French Children's Hospital at the other extremity of Paris.—*Journal de Méd.*

PARISIAN MEDICAL SOCIETY.—This Society continues to flourish. The following are the present office-bearers:—President, Robert Bowman, M.D.; Vice-President, Charles Taylor, M.R.C.S., Esq.; Secretary, John H. Burton, M.B., Dub.

PUBLICATIONS RECEIVED.

A Treatise on the Diseases, Injuries, and Malformations of the Rectum and Anus. By T. J. Ashton, Surgeon to the Blenheim Dispensary, etc. London. 8vo. 1854. Pp. 356.

On Indigestion, and certain Bilious Disorders often Conjoined with it. By George Chaplin Child, M.D., Consulting Surgeon to the Westminster Dispensary. Second Edition. London. 8vo. 1854. Pp. 210.

The Modern Treatment of Syphilitic Diseases, both Primary and Secondary. By Langston Parker, Surgeon to the Queen's Hospital, Birmingham. The Third Edition, entirely re-written. London, 1854. 8vo. Pp. 345.

The Medical Directory for Scotland. London, 1854. 12mo. Pp. 207.

Comparative Anatomy. By C. Th. v. Siebold and H. Stannius. Translated from the German and edited, with notes and

additions recording the recent progress of the Science, by Waldo T. Burnett, M.D. Volume I. London, 1854. 8vo. Pp. .

On the Nature of Inflammation. By James Cappie, M.D. Edinburgh. Reprint. 8vo. 1854. Pp. 23.

Questions regarding Epidemic Cholera. Issued by the Epidemiological Society. 12mo. 1854. Pp. 13.

The Elements of Natural Philosophy, or an Introduction to the Study of the Physical Sciences. By Golding Bird, M.A., M.D., F.R.S., etc., and Charles Brooke, M.A., M.B., F.R.S., etc. Fourth Edition. London. Small 8vo. 1854. Pp. 626.

Reports of Epidemic Cholera, drawn up at the desire of the Cholera Committee of the Royal College of Physicians. By William Baly, M.D., and William W. Gall, M.D., Members of the Committee. London. 8vo. 1854. Pp. 220.

NOTICES.

The reprints of papers from this Journal are entirely managed by the printers or publishers, to whom all communications on this subject must be addressed. We would remind our contributors that separate issues of their papers invariably cause much trouble and consumption of time, the cost of which, with paper, is regulated by a printed scale, which may be obtained from the printers on application.

Contributors of original articles are requested to forward communications to the Editor as early as possible. As a general rule it will be very difficult to insert a paper in the first part of the Journal unless it be received before the 12th of the month.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Operation for Split Palate.* By JAMES SYME, Esq., Professor of Clinical Surgery.

(*Read to the Medico-Chirurgical Society of Edinburgh, March 1, 1854.*)

IN the review of modern improvements in the practice of surgery, which I some time ago offered to the Society, it has been alleged that all new operations were not treated with the attention they deserved. Upon inquiry, it appeared that the only ground for this charge of omission was limited to a modification of the procedure for remedying the congenital defect of split palate, which lies in dividing the muscles, with the view of rendering the parts concerned more favourably situated for the accomplishment of union between the edges of the fissure,—the credit of this proposal being claimed for Mr Fergusson of King's College. As I distinctly stated in commencing the paper to which I have just referred, that my object was not to mention novelties of an objectionable kind or doubtful expediency, I expected that silence in regard to this operation would have been attributed to a difference of opinion as to its merits, and not to any want of disposition to do it justice. But as the latter explanation has been suggested, I think it may be proper to express my sentiments on the subject so distinctly that there shall be no room for misapprehension or misrepresentation.

I now, therefore, beg to state, as my decided and deliberate opinion, that the alleged advantage of dividing the muscle of the palate to promote union is an entire delusion of those who believe in it, since a partial division could, of course, produce no good effect; while a complete one is impracticable, and, if accomplished, would be useless. The muscles proposed to be divided are the levator-palati and the palato-pharyngeus. These muscles are not slender threads, but fleshy masses of considerable thickness and breadth, occupying a very deep, inaccessible, and irritable part of the fauces. But in the performance of myotomy or even tenotomy, tension of the texture to be cut is well known to be essential for its complete division; and, as in the case of split palate, the attachments of the muscles having no fixed point of resistance, this state of tension cannot be induced, the operation proposed would be nearly, if not quite impracticable, even under circumstances of the most favourable kind, which place the part to be divided within reach of sight and touch.

The knife which Mr Fergusson has recommended for the purpose should alone be sufficient to excite serious doubts as to the practicability of accomplishing the object in question ; and if any one believes that he could divide completely the lax and yielding bellies of the palatal muscles by such means, his ideas of operative procedure must be very different from those which most anatomists would be led to entertain from their acquaintance with the animal tissues and the power of instruments upon them. That the muscles in question may be wounded or partially divided, I readily admit ; but that they can be completely cut across, and still more, that they can be so with certainty, I no less positively deny. And if the division is only partial, it must be equally incompetent to produce the effect desired as an imperfect section of the adductor muscle of the eye-ball, is found to be for the cure of squinting.

The next point for consideration is the amount of advantage to be expected from division of the palatal muscles, supposing it were practicable. If they remain undivided, it is said there will be a constant tendency to separate the raw edges, and prevent union ; while if they are divided, the palate being rendered lax and flaccid, will be in the most favourable condition. But it may here be asked, does the presence of muscular contractility in other situations impede union ? and does a flaccid state of the textures concerned tend to promote it ? In harelip, when the patient cries, the edges of the fissure are drawn so completely aside as nearly to efface the appearance of a lip ; yet, if the operation be properly performed, no inconvenience is experienced from this source, and a sound union is accomplished with almost absolute certainty. In wounds of the eye-lids or cheek, even when attended with a considerable loss of substance, and when the raw edges are widely separated by the muscular contraction, no difficulty is found in keeping or uniting them together. It seems, indeed, as if, through some intuitive influence of the vis medicatrix, the muscles of the part concerned cease to contract with violence, and merely give that degree of tension or firmness which is well known to favour the adhesive process. On the other hand, there can be no doubt that if the palate admitted of being rendered perfectly lax and flaccid, it would then become very unfavourably situated, either for the performance of the operation, or for the accomplishment of a satisfactory result, since, as every practical surgeon knows, there is nothing more opposed to sound union between the surfaces of a wound than the soft and flabby state which results from redundancy or relaxation of the textures. From what has been said, it will, I trust, appear that complete division of the palatal muscles is impracticable—that it is not requisite for a successful issue—and that, so far from being so, it would really be adverse to the object in view.

I may here remark, that very serious doubts have been entertained as to the expediency of performing this operation at all, since it cannot be attempted with any prospect of success until the patient is

old enough to abstain from voluntary resistance; and then the organs of voice have become so practised in overcoming the difficulties of their imperfect condition, that closure of the fissure, so far from being always beneficial, sometimes appears injurious to the distinctness of articulation. But as the operation, if undertaken, should be conducted so far as possible to assure a satisfactory result in regard to the accomplishment of its immediate object, I shall now offer some remarks upon the different steps which are required on the occasion; and as it may be supposed that, although the principle of Mr Fergusson's operation has seemed to me so questionable, there is perhaps something in the mode of its performance deserving of attention, it will be proper to inquire particularly into the method which he recommends.

Mr Fergusson advises that, in the first place, the muscles should be divided by cutting on each side deeply into the pterygoid fossa with his triangular knife, and then using scissors to complete the process. Now, granting that the object in view may be thus accomplished, which I entirely disbelieve, it is at all events evident that the patient's throat must be in a painful, irritable, and bleeding state when the next step is undertaken. But this step is of the most essential importance, since it is nothing less than placing the edges of the fissure in a condition admitting of their sound union, by paring them so smoothly and accurately that, without any undue loss of substance, the two raw surfaces may allow of perfect adaptation. How a process so nice and delicate is to be executed when the parts are obscured by blood, and the patient's power of self-command has been impaired or expended in enduring the much more painful preliminary proceeding, I am at a loss to understand any more than the possibility of paring the edges of the fissure, as Mr Fergusson advises, by seizing the uvula and cutting towards the commissure, especially with such a knife as the one he recommends for the purpose, which is thick and narrow instead of being thin and broad as it ought to be. How the stitches are to be got in does not very clearly appear from Mr Fergusson's description; but his advice to take them out on the second or third day seems in the highest degree objectionable, since the union, however perfect in the first instance, can then have little power of resisting pressure, either from food or the tongue, independently of the disturbing influence of the pharyngeal muscles. The threads should penetrate the whole thickness of the palate, and be tied with no more force than is sufficient to retain the edges in contact, so that, in the event of union taking place, they may neither cause sloughing of the portion included nor cut their way out by ulcerative absorption. In the case of a young lady, on whom I lately operated with such success that the adhesion was complete to the very tip of the uvula, one stitch was removed on the eighth and the other on the tenth day.

In performing the operation, the best way of proceeding is to place the patient on a chair in a good light, then to seize one edge

of the fissure at its middle by sharply pointed forceps, and introduce the knife, which should be thin and lancet-shaped like the one of this form used for the extraction of cataract, a little above the commencement of the cleft, and cut evenly down from this point to the extremity of the uvula so as to detach a slice of sufficient thickness to expose the submucous textures. The same process being repeated on the other side of the fissure, nothing remains but to introduce the stitches, which is best done by means of a slightly curved needle with fixed handle, which should be directed from without inwards, first on one side of the fissure, and then on the other. The two inner ends of the threads being then tied together, one of the other ends is to be pulled until the knot is drawn through the edge of the palate, and sufficiently far out of the mouth for the purpose in view. Two stitches are sufficient, one being placed at the root of the uvula, and the other midway between this point and the angle of the edges of the fissure. The threads should be tied with the "reef knot" and in doing so, resiliency of the textures may be counteracted by keeping the threads in a state of tension. For at least two days the patient should subsist entirely upon fluids, and of these even have a very sparing allowance. He should also, of course, avoid talking, coughing, sneezing, and all other actions calculated to disturb the uniting process.

Entertaining the views which have now been explained, I am at a loss to see why the operation for split palate should be regarded as one of the improvements in modern surgery, since the old plan appears to be still the best, and merely requires careful execution to afford success. But as next to the suggestion of improvements the most important service that can be rendered, in a practical sense, is the exposure of error, I think this subject deserves the attention of the Society on another ground; and although it would be unreasonable to expect that all the members of my profession should at once relieve their minds from the delusion under which they have for some time past been labouring, I nevertheless venture to hope that the truth, as it seems to me, having been thus publicly and uncompromisingly stated, will gradually exert a salutary influence, by taking root, and in good time producing the fruit of a sound practical principle.

ARTICLE II.—*Notes of Hospital Practice.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh.

THE ECTROTIC OR ABORTIVE TREATMENT OF SMALL-POX BY
ZINC PLASTER.

ON two former occasions I have called attention in this Journal¹ to

¹ Vol. x. p. 60, and vol. xv. p. 407. ..

the remarkable results obtained by a mercurial plaster thickened with starch applied to the face in cases of small-pox. Several cases were also detailed, the number of which might easily have been augmented, showing not only that pitting was prevented, in severe confluent natural cases of the disease, but that the pain, swelling and suppuration of the face, the general fever and restlessness, and the violence of the disease, were all greatly diminished by the local treatment. Dr George Paterson of Tiverton, however, published a case¹ where excessive and dangerous salivation was in this way occasioned, the risk of which must seriously compromise the otherwise great advantages of the ectrotic treatment. But it may be asked whether, after all, the mercury is in any way necessary to the success of this treatment. Its original propounders in Paris may indeed have supposed that the absorbent powers of the drug was the true cause of its success, but it seems to me that another explanation may be offered. There is, for instance, a close analogy between the mode of healing of wounds and ulcers, so well described by Dr Macartney of Dublin—that is the so-called “modeling process”—and what takes place in the ectrotic treatment of small-pox. In the former cicatrices are far less liable to be produced than after healing by the first or second intention, and in the latter the pitting or cicatrization is prevented. The artificial plaster therefore takes the place of the natural scab or clot of blood, protects the parts below, and enables them to heal slowly but more perfectly than if exposed to the air uncovered and uncompressed by superjacent crusts. If this be the correct theory of the ectrotic treatment, the mercurial might be discarded, and any kind of plaster which would concrete on the face might be expected to produce the same beneficial result. This session I determined to try the effects of such a plaster, and after two or three failures have succeeded in procuring one that answers perfectly. The first case I treated with simple lard, thickened with starch and powdered charcoal, but it was so little coherent, that the patient, during the night, rubbed it off on her pillow or with her hands, and on her recovery she was pitted all over. In another case I tried carbonate of magnesia saturated with oil. But this also failed. In a third case, however, common calamine (*zinci carbonas*), saturated with olive oil (proposed by Mr Bird, one of the clinical clerks), formed a coherent, tough crust, which remained on the face, and was found to answer well. Three cases of natural small-pox have been since treated in this manner with the result, not only of preventing the pitting, but of diminishing the local and general symptoms, exactly in the same manner as I have formerly detailed, as being the effect of the mercurial plaster. The following is one of these cases :—

Alexander Ross, æt. 13, never been vaccinated, was seized with shivering on

¹ Vol. xv. p. 536.

the 7th January, followed by the usual symptoms of fever. Entered the Infirmary on the 9th, when a few papules were observed on the face and arms. On the 12th the face was thickly covered with vesicles, which from their closeness would certainly have become confluent. The mask of calamine and oil was now applied. The disease ran its usual course, the eruption being confluent on the arms and trunk. Throughout the progress of the case the application of calamine saturated with oil preserved a firm and coherent crust, and was renewed from time to time. The patient experienced no smarting of the face, there was no swelling of the eyelids, no purulent discharge, or local unpleasant symptoms of any kind. The secondary fever was tolerably smart, delirium being present two days. On the 22d the mask came off, leaving a clean smooth surface, free from all trace of pitting. Dismissed quite well on the 26th.

The following formula, after numerous trials, has been found to constitute the most efficient plaster:—Carbonate of zinc, 3 parts; oxide of zinc 1 part, rubbed in a mortar with olive oil to a proper consistence.

TREATMENT OF FAVUS BY A SULPHUROUS ACID LOTION.

The treatment I have found most useful in favus is, after removing the crusts with poultices and shaving the scalp, to keep the surface moist with cod-liver oil. Certain it is that so long as the oil is applied the disease never returns, and in the majority of cases in young persons, especially if they be well fed, a permanent cure is effected in about six weeks. Having observed, however, that Dr Jenner of London had treated the disease by means of a sulphurous acid wash, composed of one part of sulphurous acid mixed with three parts of water, and knowing the powerful effect of this acid in checking parasitic vegetable growths, I tried this treatment in the Clinical Ward. Dr Alexander Struthers informs me that, in January 1853, when resident clinical clerk, he had treated a case of favus with success in this manner. Five cases, in all of which the parasitic character of the favus crusts was demonstrated by means of the microscope, were selected for trial. In all, the crusts were first removed by poulticing and the head was kept shaved. Lint saturated with the lotion was ordered to be applied over the entire scalp morning and night, and the whole kept constantly moist by wearing an oil silk cap. In two cases, about the third week, the favus crust had already come back, which I traced to the oil silk cap having become displaced during the night, and not covering the scalp with sufficient care. In the other three cases the surface was kept moist and clean for six weeks. Then the treatment was suspended, and in twelve days the favus had returned in two cases. In the third case a cure was effected, and what is singular is, that it was the worst case of the five.

Helen Goodall, æt. 16, has been affected with favus of the scalp for four years; has frequently been in the Infirmary, and subjected to various kinds of treatment, under different physicians, without any permanent benefit. On admission, November 3d, great portion of the scalp was bald, from destruction of the hair bulbs, but the other portions were covered with a prominent yellow friable crust, of mousey odour, crowded with pediculi. The treatment with the sulphurous

acid lotion was commenced on November 7th, and suspended December 23d, 1853, leaving the scalp partly bald, but quite clean. On the 15th of January 1854, the disease had not returned. The scalp was then rubbed over with the oil of cade, to remove the squamous eruption, and she was dismissed, apparently quite cured, February 5th.

This result, although only one case in three, warrants a further trial of the remedy, and renders it worth while to contrast it with the effect of continued moisture alone.

TRANSIENT GRINDING PAIN SHOWN TO BE COINCIDENT WITH THE PASSAGE OF BILIARY CALCULI.

Nothing, occasionally, is more vague than the accounts given by hospital patients of local pain in the region of the liver. Of this there was a good instance during the present session in the case of a man, John Macdonald, æt. 50, a tailor, affected with jaundice, who, on admission, November 29th, 1853, informed us again and again that he had never experienced any kind of hepatic pain whatever. Notwithstanding, we received positive information from a clinical student who saw him at one of the dispensaries, and by whom he was sent into the Infirmary, that he was then suffering excruciating agony, was doubled up, and could scarcely stand, from apparently the symptoms of a gall-stone passing. On admission into the house, no local pain or weakness existed, but the jaundice continued. On December 10th he was again seized with acute, grinding pain in the right hypochondrium, which lasted three days, and left him very weak. This decided attack, in conjunction with the history of a previous one, indicated the presence of gall-stones passing through the biliary ducts. But still the jaundice continued, as if some permanent obstruction to the common duct existed, of the nature of which, however, we had no evidence, as there was no local swelling or pain, and the size of the liver, as determined by percussion, was quite normal. He died comatose December 19th.

On dissection, a scirrhus mass, the size of a walnut, was situated at the head of the pancreas, surrounding and compressing the common duct at its entrance into the duodenum, and bulging into that gut. The portion of duct which passed through the tumour was an inch long, and hardly admitted a common size probe. Behind the constriction the common, cystic and hepatic ducts were found greatly enlarged, admitting the passage of the little finger. The gall-bladder was much distended with dark-coloured bile, and contained two gall-stones of black colour, rough surface, and the size of coffee beans or very large peas, formed apparently of inspissated bile. The liver was of a dark green colour, and was studded over with several masses of whitish substance, varying from the size of a pea to that of a shilling. These masses were of a rounded form, not elevated above the surface of the organ, and presented no depression on their surface. On cutting into the liver, numerous similar masses were found. The substance of the liver was throughout of an olive-green colour, and

the gall ducts were everywhere enlarged. The organ weighed 3 lb. 9 oz. sp. gr. 1064. The pancreas contained several small cancerous nodules, several of which were encysted, containing colloid matter.

Now, from the fact of there having been two attacks of grinding pain, and of two biliary calculi only having been found in the gall-bladder after death, it is probable that the former were occasioned by the passage of the latter. When further it is considered that these calculi presented all the characters of such as are formed in the liver, and that they could not have passed through the constricted common duct into the intestines, then it becomes certain that they must have passed from the liver, through the dilated hepatic and cystic ducts into the gall-bladder. It is seldom that a case occurs where the coincidence of pain, with the passage of particular gall-stones, is so evident as in the one just noticed.

ON THE DIAGNOSTIC VALUE OF THE ABSENCE OF CHLORIDES FROM THE URINE.

Simon and Redtenbacher first stated that chloride of sodium, a salt always present in healthy urine, was absent from that fluid during the onward progress of pneumonia, and returned to it when absorption of the exudation was about to commence. This statement was confirmed by Dr Beale of London, who, in the 35th vol. of the Transactions of the Medico-chirurgical Society of London, furthered our knowledge regarding it by additional valuable researches. My attention was directed to this remarkable fact during the present session by Dr Robert Cartwright, a gentleman attending the Clinical Wards of the Infirmary, who informed me that he had seen it occasionally of great service in a diagnostic point of view, in the clinical wards of Professor Oppolzer at Vienna. It so happened that a man, John M'Donald, æt. 25, had just been admitted labouring under well marked simple pneumonia at the apex of the right lung. He was a labourer, who had enjoyed perfect health until two days before admission, when, on being exposed to wet and cold working at drains, he was seized with shivering followed by fever, and the usual symptoms and signs of pneumonia. On adding a drop of nitric acid to some of his urine in a test tube, and then dropping into it a little solution of the nitrate of silver, the fluid remained clear, although so great is the delicacy of this test, that a white cloudy precipitate is at once formed, if a very minute quantity of the chloride of sodium be present. It was on the fourth day of the disease that the observation was first made, and the chlorides remained absent during the fifth and sixth days, during which period the disease extended from above downwards, until it occupied the upper two-thirds of the right lung. On the seventh day a slight haze was observed in the urine, indicating that the salt was returning to that fluid, and the man expressed himself as being much better. On this day there was great dulness on percussion, all crepitation had ceased, the breathing was tubular with bronchophony. On the

eight day slight returning crepitation was audible, the dulness had diminished, but the urine, owing to some accident before the visit, had been thrown away. On the ninth day, however, the chlorides were abundant in that fluid, together with lithates, loud crepitation was now universal throughout the lung, and the dulness had nearly disappeared. From this time the man made a rapid recovery, never having been bled, and was discharged quite well on the sixteenth day.

I now requested Mr Seymour, one of the clinical clerks, to test the urine of all the patients in the ward, and others who might subsequently be admitted, which he did, and thus collected a large number of observations, the results of which I shall allude to immediately. In the mean time another case entered, which seemed to point out the value of this test in a diagnostic point of view. It was that of a man, Donaldson, æt. 26, labouring under typhus fever, in whom the disease ran its usual course to the tenth day, when chlorides were demonstrated in it. On the eleventh day, however, pulmonary symptoms came on, and the chlorides were entirely absent from the urine. This led me to make, with the clinical class, a careful examination of the chest, when all the signs of pneumonia were detected in the lower half of the right lung. On the fourteenth day the chlorides reappeared, the pneumonic signs diminished (?) and the fever ceased with a critical sweat.

The third case was even more satisfactory in proving the moment of commencing and departing pneumonia by testing the urine for chloride of sodium. A man called David Murray, æt. 43, entered with pneumonia of the lower two-thirds of the right lung. No consistent account could be obtained from him as to when the disease commenced, and it was impossible, therefore, to determine whether the coarse crepitation which was audible over the inflamed lung was the advancing or returning crepitation. But the chlorides were absent from the urine, which indicated that the disease was advancing. The following day complete consolidation had occurred, with dry tubercular breathing and absence of crepitation, and a minute quantity of the chlorides was found in the urine. The patient, however, instead of getting better showed no improvement, and the next day the chlorides had again disappeared, indicating extension of the pneumonia. On the evening of this day he was seized with acute meningitis of which he died. On dissection, in addition to universal cerebral meningitis, the whole of the right lung presented the usual characters of grey hepatization.

These cases serve to point out a remarkable connection between the absence of chlorides from the urine and the onward progress of pneumonia. I forbear from offering any opinion as to the theories which have or may be advanced on this subject. The fact requires to be more extensively investigated clinically than has yet been done to test its value. Still it seems to me, that where pneumonia

exists, inferences of great importance as to the stage and progress of the disease may in this way be arrived at by the physician, of which he would be wise in future to avail himself, more especially when the test is so easily applied, and its character so readily determined.

Mr Seymour tested with great care, and at repeated times, the urine of upwards of fifty other cases in the wards, embracing a great variety of disease. He found the chlorides absent in one case of phthisis, with intercurrent pneumonia, but in no other. They were also absent in one case of peritonitis, and in all the cases of small-pox. Further investigation will probably discover these salts to be absent in other diseases, which, although it may diminish the importance of the sign as distinctive of pneumonia, leaves unaffected its value as pointing out the onward progress of that disease. The whole subject, however, being so new in a clinical point of view, it is evidently premature to speculate in any way regarding it.

I need only now allude to one other point, viz., that if any phosphates exist in the urine, nitrate of silver throws down a faint sediment, which, although it cannot be mistaken for the precipitate of chlorides in healthy urine, may be confounded with the appearance it presents when small in amount. In such a case the action of ammonia, by dissolving the chlorides, is at once distinctive.

CASE OF OVARIAN DROPSY, WHICH GRADUALLY EMPTIED ITSELF BY SPONTANEOUS OPENINGS THROUGH THE FALLOPIAN TUBE. SINGULAR ATTEMPT AT IMPOSITION OF PIGS' BLADDERS, FOR CYSTIC FORMATIONS IN THE UTERUS.

In June 1852, I received from a highly respectable practitioner, in the north of Scotland, a bottle containing several cysts, with a letter informing me that they had been passed *per vaginam* by a woman labouring under ovario-uterine disease. He wrote that

"The patient, about eighteen years ago, had a mature child. Her labour was followed by an attack of peritonitis, and she dates her present ailments from this period. Before the case came under my care, she had been long in hospital for ovarian dropsy, and was there repeatedly tapped. During the last few years she has passed *per vaginam* from time to time, one of the membranous productions of which I herewith send you specimens. You will perceive that in some instances they were perfect casts of the interior of the uterus; in others they have been broken in the removal. When she first applied to me, the paracentesis had been delayed beyond the usual time. The dropsical accumulation was great, and her general health in a very unsatisfactory state for surgical interference; and before I could make up my mind to operate, nature kindly came to the relief of doctor and patient, and managed the thing so well that I have allowed the good dame to have everything her own way since. She did it thus: a membranous cyst was thrown off, and this was immediately followed by the discharge *per vaginam* of the dropsical fluid, to the amount of several pints. That this also came from the uterus, I satisfied myself by tactile examination whilst the fluid was passing. Since then this process, the discharge first of the membrane (distended with serum), and immediately after of the effusion, has been repeated every few months, the patient in the interval enjoying an astonishing measure of health."

Without entering into a minute description of these cysts, it will

suffice to say, that after carefully examining them, I came to the conclusion that they were the urinary bladders of some animal—and from their size (between two and three inches in their long diameter), perhaps of lambs or small pigs. They had evidently been macerated, and the external and half of the muscular coat removed, and the smooth mucous surface turned inside out. In some of them, however, there could be seen the two openings of the ureters, whilst in others fragments of one or both tubes were still attached. On communicating my opinion as to the nature of these cysts to her medical attendant, and hinting that so far the woman was an impostor, he replied as follows:—

“I removed two or three of the membranes, on as many different occasions, from the vagina, and the state of the os uteri, as ascertained on their removal, was always such as to leave no doubt in my mind as to their having been ejected from the uterine cavity. On each occasion the woman suffered severely, having had regular and painful uterine contractions, till the diseased product was expelled, and profuse hemorrhage afterwards generally inducing syncope. I mentioned in my former note that I have more than once felt the dropsical fluid (which, as I also stated, is generally discharged shortly after the sac) passing from the os uteri. Add to this that the woman's circumstances are such that it would be next to impossible for her to procure the means of perpetrating the trick you suspect her of. The membranes were kept by me for years immersed in spirits. May not this circumstance have rendered your examination of them less satisfactory.”

In this letter my correspondent announced his intention of sending the woman to Edinburgh, if I would take her into the clinical ward of the infirmary. This I promised to do, and having passed another “cyst” in the interval, she was admitted September 30th, 1852. The following is a condensed history of her case, entered in the ward-book, with an account of how I at length succeeded in detecting the imposture, which she had hitherto so successfully practised:—

Elizabeth Allan, æt. 43.—Servant; a tolerably stout healthy looking woman. States that she enjoyed good health up to the year 1829, when she experienced a lingering *post partum* recovery. In 1830 her abdomen commenced to swell, for which she was tapped, and 190 oz. of fluid removed. For six years she was under medical treatment; but then (1836) she had so far recovered that she was once more able to follow her usual occupation. In 1841 the abdominal swelling returned, which continued till 1848. At the beginning of that year, the abdomen being greatly swollen, she felt something give way, followed by intense burning abdominal pain, and a copious discharge of an opaque fluid from the vulva. The pain subsided in a few days, and some weeks afterwards she was much better and was enabled to get out of bed. She says the same thing has happened upwards of twenty times since, at various intervals—the last occurred on the 27th of August 1852. She also states that about seventeen cysts or polypi have at different times been removed from the uterine passages—their removal being preceded by bearing down pains. During all this time the catamenia have appeared regularly, and lasted on an average from eight to ten days. She further says, that for the last twelve years she has had repeated attacks of hematemesis. In 1852, her attention was drawn by her medical attendant to a bulging beneath the left clavicle, where she has frequently experienced a deep-seated pain of a burning character, with a sense of a trembling or vibratory movement.

“On admission, the patient complains of uneasiness and pain in the lower part of the abdomen, and of frequent tympanitic distension. On examination,

the abdomen presents the appearance of a woman in the sixth or seventh month of pregnancy—is everywhere tympanitic on percussion, but on palpation there is an undefined fulness and induration in the left iliac and hypochondriac regions—no pain on pressure when the patient's attention is directed from the part. The left side of the chest appears fuller than the right side, but expands well on inspiration. No dulness on percussion; respiratory sounds normal; no cough or pulmonary symptoms. Below the acromial extremity of the left clavicle, a loud blowing murmur can be heard over the subclavian artery, which is inaudible on the right side. Circulatory, digestive, urinary, and integumentary systems otherwise normal.

The patient complained of tympanitis and pain, for which she was treated by occasional purgatives, enemata, carminatives, and antispasmodics. On the 6th of October, I requested Dr Simpson to examine her per vaginam. He found the cervix uteri about three quarters of an inch in length, increased in thickness and density. The os uteri was patulous, and admitted easily the first phalanx of the index finger. The edges were rough. The body of the uterus was mobile, but its volume was increased. The left ovary was enlarged, and the rectum distended posteriorly. At this time the patient expressed her opinion that a discharge of fluid was about to take place. Nothing occurred, however, until the 5th of November, when, a little before five o'clock P.M., rising to micturate, she felt something give way in the lower part of the abdomen, and about 50 oz. of fluid escaped on the floor. A small quantity was carefully collected, and was found to consist of slightly opalescent serum, of sp. gr. 1005, slightly coagulable by means of heat and nitric acid. Dr Simpson, on examining per vaginam, discovered nothing new, except that the fluid undoubtedly was still trickling from the os uteri, and that the distension of the rectum, which was then also present, was owing to scybalæ. On introducing the uterine probe, the cavity of the uterus measured three inches and a half in length, and contained nothing abnormal. Nothing unusual followed the discharge of fluid. On the 28th of December Dr Simpson having previously introduced a sponge tent to open up the os uteri, examined the interior of its cavity, but could discover no tumour, unnatural hardness, or roughness of its walls. There was then also a considerable watery discharge.

During all this time she continued to complain of vague abdominal pains, which were evidently feigned. She was carefully watched, and no cysts had come away. On the 4th of January she expressed herself so well that she was desirous of going out. Feeling satisfied that this request on her part was to procure the means of imposition, permission to go out was granted, and Mr William Calder, one of the clinical clerks, agreed to follow her. She went straight to the market, and was seen after making inquiries of one or two butchers to purchase a pig's bladder. Three days afterwards, January 7th, I was shown at the visit a macerated piece of this bladder, which she affirmed had been passed during the night, and was a portion of a ruptured cyst. According to her own account there had been violent bearing down pains for three nights previously. I proceeded to inspect the substance, and on informing her that it was a piece of pig's bladder, her astonishment and alarm may easily be conceived. She subsequently confessed this imposture, but nothing could induce her to communicate anything with regard to her former ones.

On reviewing the facts of this case, it appears probable that cystic disease of the left ovary had burst at successive times into some portion of the left Fallopian tube, and so been gradually emptied; and that with a view of exciting further sympathy, she had introduced the macerated urinary bladders of lambs, sheep, or pigs into the vagina, and pretended that they had been formed in the uterus.

ARTICLE III.—*On the Prevalence of Chromato-Pseudopsis or Colour-Blindness ; its Evils, and the Means of Diminishing its Frequency.*
By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.
—Continued from p. 44.

Confusion of Red with Green.

ON looking over the cases recorded by others, I observe that the better perception of red and green by artificial than by daylight has not escaped the notice of previous writers. Dalton found that to some twenty colour-blind persons, including himself, "*red and scarlet* have a more vivid and flaming appearance by candlelight than by daylight;" and that "in all points where we differ from other persons, the difference is much less by candlelight than by daylight."¹ Mr Milne of Edinburgh, also, whose case is described by Mr Combe, "calls crimson blue by day, and bright red by candlelight."

It thus appears that a large number of the colour-blind have discovered that red, still more crimson, and to a less degree than either, scarlet, are distinguishable from green by ordinary artificial light, where they are not distinguishable by daylight. That *both* colours appear different, seems certain from the letters I have printed, and I refer to these because I have had comparatively few opportunities of testing the colour-blind by artificial light. In Dr Y.'s case, however, I know certainly that red or green seen *alone* is distinguished by gaslight where it is mistaken by day; and if each colour is thus altered and rendered more visible by artificial light, the two, seen together, must contrast much more strongly when illuminated by it.

I do not here enter into the explanation of the greater visibility of red and its complementary colour by candle or gas light, which, however, it may be remarked, is chiefly to be sought for in the different quality of this from sunlight; but three points demand notice.

1. The conclusion already insisted on, that red and green are both occasionally visible to the colour-blind who habitually confound them, is greatly strengthened if vision by artificial light be taken into consideration as well as vision by daylight.

2. Those who confuse red and green by daylight continue to do so, though to a less degree, by artificial light.

3. From this it follows that, contrary to general belief, our present railway signals are safer, so far as liability to mistake by the colour-blind is concerned, by night than by day. But, as the cases of Mr Hughes and Dr Y. specially illustrate, though safer, they are not safe.

To complete the discussion of red and green in their relation to colour-blindness, two points call for discussion from their interest in

¹ Mem. Lit. and Phil. Soc., Manchester. 1798. Pp. 40, 41.

connection with the physiology of this affection of vision, and their importance in reference to the observation of colours by the colour-blind.

In describing Dr Bryce's case (*ante* vol. xlvii. p. 391), I have referred to the effect of distance in rendering red invisible to him. His case is not peculiar. Mr D., Mr T., Dr K., Dr E., Admiral —, all refer to their ceasing to distinguish red from green when removed to a short distance from those colours, although they discriminate them close at hand. My friend Mr David Stevenson, the accomplished engineer of the Northern Lights, has suggested to me that the *distant* colour-blindness referred to may be only the result of ordinary myopia or short-sightedness. That ordinary myopia affects the vision of colours as well as of form or outline is unquestionable, but that there is what may be termed "chromic myopia," or a short-sightedness to colour, not accompanied by a corresponding short-sightedness to form or outline, admits, I believe, of distinct proof. Thus, Dr Bryce described the cruciferous arrangement of the petals of a ver-bena, but mistook its colour, of which he was reluctant to speak at all. Prof. Y., who is long-sighted, and a practised observer with trigonometrical instruments, states, in reference to the difference between red and green as seen by daylight, that "though I can see them well enough if near, at a little distance the contrast makes no impression on me" (*Ibid*, p. 500). Mr T. regards himself as better off than his neighbours, "so far as strength of vision and clear definition both of distant and near objects are concerned," but he adds, that "at a distance of about sixty yards it would puzzle me to distinguish the colour of a soldier's coat, although *the general outline of the figure* would enable me to pronounce upon its being that of a soldier" (*Ibid*, p. 498). Admiral — makes a similar reference to a red jacket and to a tiled barn (*Ibid*, p. 499); and Drs K. and E. are equally foiled by red and green objects when removed to a comparatively short distance from them.

From these volunteered statements of sufferers from colour-blindness, I drew the inference that the perception of form and outline remains with their eyes after that of red and green, as accompanying shape, has been lost in consequence of the distance between the spectator and the coloured object being increased; and, conversely, that on walking up to a far off red body, its shape is discerned before its colour. With ordinary myopic eyes, the reverse I believe to be the case. I have appealed to several friends, otherwise normal in vision, who, like myself, are short-sighted, and we agree in the conclusion, that a letter, figure, or device, is visible to us as a shapeless red blot at a distance which precludes us from giving any opinion on its form. At the same time, I have laid Mr Stevenson's suggestion before the more accessible of my colour-blind acquaintances, with the following result:—

Dr K. writes thus:—

"I have your letter relative to the suggestion that the nonper-

ception of red was occasioned by ordinary myopia. I hold this theory to be quite untenable, as many myopic eyes are quite normal as to colour. I am neither annoyed by myopia nor presbyopia. As to outlines of objects, or even minute details of form, whether near or at a distance, I believe my eyesight is at least as acute as that of others; indeed, I should say in this respect, rather more so, from the circumstance that my eye has been trained to look for *difference of form*, seeing I receive little or no aid from that of colour. In the case of red fruits and flowers, I can see the *form* long before I can detect the colour, and this for the simple reason, that the colour of the fruit and leaves being to my eye the same, there is nothing to direct my eye to the point where the fruit is. I have often remarked, that persons with normal eyes remarked to me the existence of fruit on a tree at surprising distances, from the contrast in colour alone; but when I have walked with them towards the tree, and directed them to tell me when they distinctly saw the *form* of each fruit (be it cherry, gean, rowan, or other), I always found that I could see the *form* fully as soon if not sooner than they could, though in my case I was not aided by colour, because fruit and leaves were all one hue to my eye. They saw a blot of red standing out on a mass of green from a great distance, but my eye detected *form* before theirs. I have no doubt on this point relative to my own case."

Dr E. replies, that to the best of his belief he "can see the form of an object long after its colour has been lost to the eye, and to simplify the matter into the shape in which you properly put it, I could tell you whether a spot was round or square, long after I had ceased to recognise colour at all."

To test the matter further, he took a long-sighted, normal-eyed companion with him into a garden, on one of the walls of which a number of crysanthemums were growing in full blossom, visible when first seen from the garden-gate, at a distance of some thirty yards. The flowers were yellow, pink, and crimson. Those which were yellow he saw sooner than his companion, but the red blossoms were imperceptible to him long after they seemed to his friend the most conspicuous objects on the wall.

Further, a fellow-physician made a series of experiments on Dr E.'s perception of colour, by placing wafers, and coloured papers of various shapes at different distances from the eye, with the result which he thus reports:—"The appreciation of *form* was perfect (no matter what colour was employed), at any point within the range of *vision*;" whilst so greatly did the appreciation of colour vary with the distance, that a red-purple disc, which was recognised to be such close at hand, was pronounced to be green when removed to a distance; and scarlet and green wafers, which were regarded as showing shades of green or brown close to the eye, were declared to be green at the distance of four feet, and to be pale red at the distance of eight.

Mr Hughes, also, has at my request made observations on the relative visibility at a distance, of form and colour, as displayed in the Royal Scottish Academy's Gallery of Pictures, and reports that he perceives the outlines of painted objects, after he has ceased to be certain regarding their colour. In addition, a medical man, Dr S., whose case I have not recorded, volunteered the statement that he could, though at a considerable distance from them, recognise poppies among green corn, so long as they were unblown, by the marked shape of the drooping unexpanded flower; but as soon as the green calyx was shed, and the scarlet petals were exposed, he lost sight of them.

These cases might suffice; but whilst I write, a party unknown to me, spontaneously sends from Statham, Norwich, a letter on his colour-blindness, in which regarding what he is about to describe, as a peculiarity of his own case, he says:—"I can discern red colours in general, at hand or within a few yards, but fifteen or twenty yards off, I cannot distinguish ripe cherries from leaves, although when half-ripe they were quite evident to me."

I feel assured, therefore, that a colour-blind eye becomes insensitive to red, or doubtful regarding it, at a much shorter distance from a red object than a normal eye does, and ceases to perceive a difference between red and green long before these have become invisible to the perfect eye. In truth, it does not consist, so far as I know, with the familiar experience of those free from colour-blindness, to find that any distance, however great, leads to an identification of red with green; and although what the great anatomist Dr Barclay loved to call *experientia vulgaris* is to be taken with due allowance, yet when we find no normal-eyed person ever hinting at a temptation to mistake a distant red object for a green one, whilst a number of colour-blind persons, who have had no communication with each other, are found acknowledging a prevailing temptation to do this, we cannot hesitate to regard them as blind to red and green at distances which only slightly diminish the contrast between these colours to those possessed of more favoured organs of vision. In support of this conclusion, I would only further refer to two circumstances.

1st, A very large number of persons have discovered their colour-blindness by failing to distinguish red flowers or fruits from their leaves unless very near them.

2d, Without any reference to the special question under consideration, I have inquired at nearly all the subjects of chromato-pseudopsis known to me what colours they saw in the distant rainbow, and nearly all have replied that they did not see red or green, but saw the other prismatic bands, or at least the yellow and blue. Mr B. (in whom, however, colour-blindness was induced by cerebral injury) sees no colours at all in the rainbow, and does not see red and green close to the eye. But many of those whose cases I have reported distinguish red from green close at hand, yet when they gaze on a rainbow, at a distance from it which does not render them insensible

to two of the three primary colours which it displays, cease to perceive the third primary, red, and also green.

There appears thus to be a "chromic myopia" or short-sightedness to colour, which, so far as I am aware, has not hitherto been generally recognised.

The last point I have to notice in connection with the identification of red with green concerns the dark shades of both colours. The addition of black to red converts it into brown, and a similar addition to green changes it into olive, although that title is more properly applied to a mixture of dark purple and green. The dark-reds and dark-greens thus generated are great puzzles to the colour-blind. Those who confound full bright-red with full bright-green are, I believe, always equally at fault with dark-red and dark-green; and many who do not mistake the pure fully illuminated colours are stumbled by their darker shades. The particular proof of this will be given in the record of the proportion of persons in the community who err in discriminating colours; but here it is important to notice that not only red-brown (red *plus* black), but orange-brown (orange *plus* black), yellow-brown (yellow *plus* black), and russet, or ruddy-brown (purple *plus* orange), appear to many eyes identical with the darker shades of green. Dr Bryce, Mr Hughes, and Dr C. illustrate this peculiarity of vision. The tailor's foreman (*ante* vol. xlvii. p. 502) has sent among his matches a ruddy-brown and a deep-green. Dr K. and Admiral — both refer to mistakes in purchasing dresses, which betray the difficulty they encounter in distinguishing brown from green. Mr S. reports his six friends to be very faulty in this respect, and tells of one who purchased a bottle-green coat whilst intending to provide himself with a brown one. A survey, moreover, of all the cases which I have recorded will show that olive, which may be taken to represent all shades of dark-green, and brown in all its shades (but especially russet or red-brown) are for the majority of colour-blind eyes the same or very similar colours.

I dwell upon this point, because an inability to distinguish olive from brown appears to be the slightest manifestation of that well-marked colour-blindness which, when fully developed, ignores the difference between full red and green. This slight colour-blindness is of importance practically, inasmuch as those affected by it not only confound dark shades of the colours in question, but are liable to mistake tints originally bright when these are tarnished by time and exposure, or when they are seen in twilight or dimly illuminated. I have referred, under Dr Bryce's case, to the importance of this slighter colour-blindness in reference to railway signals, and to this I shall recur.

It is important, also, in reference to the physiology of colour-blindness, especially when taken in connection with the chromic myopia already described. Shortened distance or increased darkening, *i. e.*, in both cases, diminished illumination, lessens the sensi-

tiveness of a colour-blind eye to red and green long before it annihilates the perception of these colours by a normal eye. In this we only see another illustration of that limited range of sensitiveness to colour which has already been referred to as an unenviable prerogative of the colour-blind organ of vision. A slight excess of light is sufficient to paralyse or exhaust its sensitiveness to colour; an equally slight defect of light leaves that sensitiveness unaroused: in both cases colours become invisible.

Confusion of Crimson with Blue.

The confusion of crimson with blue is one of the most characteristic marks of colour-blindness. A reference to the great majority of cases recorded in this paper will show that the subjects of chromatopsia have independently of each other discovered the peculiarity of their vision in reference to mixtures of red and blue. It will suffice to refer to Dr Y., Prof. Y., Prof. N., and the Countess of D. They have also recognised that the deeper shades of purple, as well as crimson, were liable to appear blue to them, and have separately reached the conclusion that their eyes perceived in mixtures of red and blue only the blue. Prof. Y., for instance, states that, "when blue and red are mixed, the blue is apt to *kill* the red" to his eyes.

Dalton reports of himself, and of the persons, nearly twenty in number, whose colour-vision resembled his own, that crimson appeared to them blue by daylight, and of himself that blue, purple, pink, and crimson seemed to him "all referable to blue."¹

Apart, therefore, from any hypothesis as to the cause of this limitation of colour-vision, it appears to be one peculiarity of the colour-blind eye to be unable to discern red by daylight, if it be mixed with blue; and a very slight addition of the latter is sufficient to render the former quite invisible, so that a crimson which by many so-called normal eyes is identified with *red*, by colour-blind eyes is identified with *blue*.

By candlelight, as already stated, the tendency to confound red-purple (of which crimson is the reddest shade) with blue disappears, or is greatly lessened, and the experience of a large section of the colour-blind, including Dalton and the cases which he has described, is embodied in the statement of Prof. Y.:—"With purples in daylight the blue overcomes the red; in candlelight, the red either overcomes the blue, or appears in its just proportion, I cannot say which."—(*Ante*) p. 500.

Confusion of Red with Black: total invisibility of Red.

By far the most remarkable variety of colour-blindness, in a scientific point of view, is that which shows itself in the identification of red with black. This singular affection of the organ of vision

¹ Manchester Memoirs. 1798. Pp. 28-36.

appears to have escaped the attention of former writers, or to have been at best incidentally noticed in one or two cases as a fact of no particular importance. Yet, the analogy which Sir David Brewster pointed out between the colour-blind eye and the organs of hearing of those to whom Dr Wollaston drew attention as unable, though otherwise free from deafness, to perceive high or shrill notes,¹ a phenomenon which has been made the subject of experiment by later observers, especially Chladni and Savart, might have led to the expectation and detection of an utter blindness to red, which is the obverse (though not the analogue) of deafness to shrill sounds, and to these alone.²

Attention was perhaps turned aside from this inquiry by an observation of Dalton's. He accounted for his own colour-blindness by supposing that the *vitreous humour* of his eye was *blue*, and explained his perception of pink, crimson, and red-purple as blue, by the consistent inference, that the red rays which such colours sent to his eyes were stopped or absorbed in their passage through the blue medium, which only transmitted rays of its own hue. But there was a difficulty in the way of this theory, which did not escape its author. "What seemed," observes he, "to make against this opinion, however, was, that I thought red bodies, such as vermilion, should appear black to me, *which was contrary to fact.*"³ He then proceeds to explain how he disposed of this difficulty, in a passage which need not be discussed, since it was ascertained after Dalton's death that the vitreous humour of his eye was not blue; and, further, singular as it may appear, he did confound (as will presently be shown) certain reds (though not vermilion) with black. Had he been aware of the facts which I am about to record, he would have regarded them as strongly corroborating the truth of his theory. As it was, however, his explicit declaration that he did not see red as black, and that he saw as long a prismatic spectrum as others, has probably led later observers on chromato-pseudopsis to overlook or pay little attention to this form of the affection, although it presents a blindness to colour which may be called absolute.

Scarlet, but still more crimson, and also pure red, are invisible to certain eyes. The following are the principal facts in connection with this invisibility, so far as I have yet ascertained them:—

1. My attention was first strongly directed to the blindness of certain eyes to red, by Mr N. of Torquay's report of his case (*ante* vol. xlvii. p. 496). In his letter he mentions that "a deficiency of daylight makes scarlet resemble black;" that, in evening twilight, "the flowers of a scarlet geranium appear of a

¹ On Sounds inaudible by certain Ears. Phil. Trans. 1820. P. 306.

² According to the undulatory theory, the wave of red light is slower in its vibrations than the other coloured waves. It thus corresponds to the slower undulations of sound, which produce low tones. Blindness to red is, therefore, analogous to deafness to *grave*, not acute sounds.

³ Mem. Lit. and Phil. Soc., Manch., 1798. P. 42.

velvety dark gloss, much darker than the leaves;" and that, in the same circumstances, he "could not distinguish between a scarlet and a black coat, even when quite close to them." Further, in artificial light, unless it is very strong, "scarlet appears nearly black."

2. Mr T. (*Ibid*, p. 498) describes similar peculiarities in his case. A very brilliant scarlet anemone was with difficulty discovered in a bed of flowers, although it was one of the most conspicuous of them, and its position was known; and when at last the eye caught it, it appeared as an ill-defined red blot. Scarlet poppies also, growing amidst corn, constantly eluded Mr T.'s notice. In these cases, and especially the first, the flowers do not seem to have been seen, but it is quite possible that they were not discerned, because they appeared green, and were confounded with the leaves, not because they were altogether invisible. Mr T.'s own impression, however, appears to be that he did not see them at all; and he adds the decisive fact in reference to one object,—“unless close to ripe hawthorn berries I perceived no red, and even when near enough to see the individual berries, they appear to me for the first few seconds rather *black* than *red*, and only gradually assume their red hue.” It would thus seem that Mr T.'s eye very tardily responds to the stimulus of red light, so that a red object is first imaged on the retina in black, making for a time no luminous or colorific impression upon it, and only slowly clothing itself with colour.

3. A third example of blindness to red occurred in Mr Hughes (*Ibid*, p. 391), who frequently copied red lines in engineering drawings in black. I have since learned from this gentleman that the red which he is thus liable to confound with black is a rather dark shade of crimson.

Greatly struck by those cases, for which I was unprepared by what I had read of colour-blindness, I now made inquiry among the subjects of that peculiarity of vision within my reach as to the relation which appeared to them to subsist between red and black. It soon appeared that a liability to see red as black is by no means uncommon among the colour-blind.

4. Dr E., on being applied to in reference to his experience in the matter, recalled the circumstance of his having written a letter, partly with red, partly with black ink, without being aware of the difference. (*Ibid*, p. 393).

5. Dr Y. could not at first recall any occasion on which red had seemed black to him, but he afterwards observed that by gaslight he could distinguish two lettered cards to be respectively red and black, which appeared to him by daylight of the same colour.¹

¹ In describing Dr Y.'s case (*ante* vol. xlvii. p. 493), I referred to this confusion as having occurred in connection with “the leather binding of church books,” but I find that I had misunderstood my informant, who tells me that the coloured objects mistaken, were cards elevated before the congregation in one of the churches of Edinburgh, with numbers referring to tunes upon them, and purposely painted in different colours to indicate which of two music

In addition to those cases, I have discovered several others still more curious, which I now relate.

6. Mr James Inglis, in the establishment of Messrs W. and R. Chambers, informed me of a gentleman in Edinburgh who, on one occasion when walking with him, mistook flakes of dry red paint which had fallen on the pavement for soot. He startled his companion by looking up to the house-tops, remarking that a chimney must have been on fire.

I have had several interviews with the party in question, whom I shall call T. R. He is an educated, intelligent young man, a cautious and conscientious observer, and gave me every facility in examining the peculiarities of his vision.

I found him a strongly marked case of colour-blindness. He again and again arranged, at my request, bundles of coloured wool, as well as coloured papers and glasses, and the detail of one of his arrangements of wools will best show how strongly defined his colour-blindness is.

The table below represents the third assortment of worsteds made at intervals by him. The wools were new, and were left with him for more than a week. He revised the arrangement more than once, and finally sent it as the best he could make. The title of each bundle is the name given to it by the arranger.

BERLIN WOOLS AS ARRANGED BY T. R.

Red Bundle.

3 Scarlets.
1 Crimson (rather dark).
3 Reds (deep).
1 Orange (chrome).
1 Brown (ochre).
2 Greens (rather dark).
1 Citrine (with excess of green).
1 Green (dirty).

Blue Bundle.

2 Blues (bright).
2 Blues (dark).
1 Blue (pale).
1 Purple (dark).
2 Lilacs (1 dark and 1 light).
1 Pink (rose or pale crimson).

Brown Bundle.

1 Brown (light ochre).
2 Greens (light).

Green Bundle.

1 Green (full and bright).
1 Green (full, but bluish).
1 Scarlet (full and bright).
1 Buff (light).
2 Drabs (light).
1 Drab (dark).
1 Flesh-colour (dirty or dull).

Pink Bundle.

1 Salmon colour.
1 Peach blossom.
2 Blues (light).

Orange Bundle.

1 Orange (full and bright).
1 Yellow (dull).
1 Green (pale).
1 Citrine.

books, both in use, was intended. Dr Y. received no assistance by daylight as to which book should be taken, from the colours of the figures, although each, as I have ascertained, is about three inches long by two broad. The red figures are crimson; the others a blue-black.

Purple Bundle.

2 Crimsons (1 bluer than the other).

Unarranged.

1 Black.

1 Dark Green.

Fawn Bundle.

1 Pale Green.

Yellow Bundle.

1 Pale Yellow.

In the arrangement above, no indications appear of a tendency to confound red with black. The whole of the reds are arranged among positive colours, and the majority are rightly arranged; whilst the solitary black wool is left unplaced, and remains apart, along with a very dark green. In keeping with this exclusion of black from the coloured wools (which, however, was the result of much comparison of them with each other), T. R. could with difficulty recall the fact of his having mistaken red paint for soot, and he had not realized the truth that he was liable to confound red with black. Yet he spontaneously added, on being reminded by his companion of the circumstances attending the mistake in question, that on one occasion seeing a lady in church well known to him, wearing what seemed to him "a *black* bonnet," he asked her for whom she was in mourning, and surprised her greatly by the question, for her bonnet was of *crimson* velvet. He tells me that he has mentally made the same mistake more than once since I first saw him, but warned of his tendency to confound colours, he looked at the apparently black bonnets in different lights, and satisfied himself that they were, or at least might be, red.

Still more recently, he has been discovered by Mr Inglis to be liable to confound red ink with black ink, and I have in my possession a card on which one word is written in crimson, and the other in ordinary ink, both of which appeared to T. R. equally black. The characters are large, and the colours strongly contrast to a normal eye, as they were intended to do by their writer. The card in question, which contained two memoranda for the writer's use, fell accidentally in T. R.'s way; but to test the matter further, Mr Inglis wrote an entire letter in red ink, which he requested his friend to copy for him. Its colour led to no spontaneous remark on T. R.'s part, and when asked how the ink pleased him, his only comment was, that he thought it rather blue; he had been unaware that the ink was red.

7. This difficulty in distinguishing inks, I believe, is by no means rare. I have made the acquaintance of a clerk in the Edinburgh Post-Office, who has surprised his superiors by signing his name to official papers in red ink. He did this unwittingly, and his customary method of distinguishing red from black ink is by the difference in their respective odours.

I know of two other clerks in town who cannot tell red ink from black ink till it dries, and who also rely upon the difference in odour as a means of distinction between them.

Dr Rowe, of the Royal Asylum, Morningside, informs me

that a friend of his, a banker near London, finding himself constantly confounding red and black ink with each other, had a bottle of a different shape provided for each, as a safeguard against further mistakes. These facts may seem trivial, but they show how comparatively frequent blindness to red is.

8. A case probably as well marked as T. R.'s, occurs in the person of a young clerical student in Glasgow, whom I shall call R. M. T. I have been disappointed in seeing him, but Mr Peter Stevenson, philosophical instrument maker, Edinburgh, who knows him intimately, ascertained the following fact, which is sufficiently decisive in reference to his vision.

He himself and his friends had long been aware that he was to some extent colour-blind, and by way of testing this, he was asked by Mr Stevenson, when recently in Edinburgh, to tell him the colour of a web of children's handkerchiefs hanging at a shop-door, distant in a straight line, about forty feet from the window where he was sitting.

The handkerchiefs in question were white, with rude designs and letters printed on them from copperplates in crimson ink, so that at a little distance they appeared pretty uniformly red. R. M. T. at once pronounced them to be "black like the print of a book;" and as the same designs are as frequently printed in black as red, an eye disposed to confound these colours, and more familiar with the former as occurring in engravings, would be put off its guard by the character of the lettering and designs presented to it. I have seen one of the handkerchiefs, which is of a bright, full crimson colour, but I do not know the peculiarities of R. M. T.'s colour-blindness. He is a student at Glasgow College, and Professor William Thomson has kindly engaged to test his colour-vision by the prismatic solar spectrum, so that further information may be given in the sequel concerning his case.

9. My friend, Mr John Crombie Brown, is acquainted with a party S. N. (known also to Alexander Christie, Esq., A.R.S.A.), who has a fine eye for form and outline, but is markedly colour-blind. In early life he was apprenticed to an upholsterer, and on one occasion being sent for black cloth to cover a coffin, he brought scarlet.

Other cases less fully examined have been reported to me by trustworthy parties.

10. Dr David Skae informs me of a young medical man, formerly an assistant in the Morningside Asylum, who was frequently compelled to appeal to others to decide for him, before sealing a letter, which was red and which black sealing-wax. Mr Walker, the eye-surgeon, also knew this gentleman, and recalls the fact of his mistaking red and black chalks for each other, when engaged in crayon-drawing. He is at present in China, but has been applied to for the particulars of his case.

11. Mr R. S. Grieve, the large carpet-maker, George Street,

Edinburgh, tells me that he has often heard his father speak of a weaver in his employment, who, besides matching red with green, frequently matched scarlet with black.

12. To these cases I may add that of the Countess of D., who enumerates together as colours she cannot confidently pronounce upon, "*red, green, black, brown, and lilac.*" (*Ante* vol. xlvii. p. 504.)

After encountering, without specially seeking for them, so many examples of the confusion of red with black, I thought it exceedingly unlikely that previous investigators of colour-blindness should not have met with cases of it; and on searching, it appeared that they had.

Thus Mr Harvey has recorded the case of a tailor in Plymouth who was otherwise colour-blind, and to whom black appeared "generally green, in particular cases crimson." A purplish red flower, the great snap-dragon (*antirrhinum majus*) he pronounced to be *black*, and "a very good match for my [Mr Harvey's] black coat." His master stated, that "being desired to repair an article of dress that required black silk, he employed crimson; and a similar mistake occurred on two other occasions."

"On another occasion, when a young gentleman's dark blue coat was brought to him for immediate repair, the mother was surprised to find the elbow of the coat repaired with crimson."¹ In connection with this last mistake, Mr Harvey quotes a case recorded by Dr Nicholls in the Medico-Chirurgical Transactions, where an officer in the navy "purchased a blue uniform coat and waistcoat, with *red* breeches to match." Here probably, as was the case with Mr Harvey's example, dark blue was undistinguishable from black, and the latter would have been confounded with red also.

Dr Colquhoun of Glasgow has described the case of a gardener in Clydesdale, who was originally a weaver, but gave up that trade, "because he confounded the red, black, green, and purple threads."² It is further stated in reference to this person, that "he confounds red with lilac, rose, brown, black, white, although he perceives the difference of the light tints and dark tints;" and that "in bright candlelight he cannot specify the shades of violet or brown, nor those of black, which he takes for brown, red, green, or black." In other respects his case was an extreme one.

I finally turned to Dalton's account of his own case, and that of his fellows, and at first it appeared that he had distinctly ascertained that he did not confound red with black, as I have already illustrated, by a quotation from his paper. Sir John Herschel and Sir David Brewster also, who both paid much attention to Dalton's case, have expressed their conviction that he saw as long a spectrum as others

peared black to him; and no one can avoid concurring in the conclusion of the great opticians named above, that Dalton did not *habitually* fail to perceive the less refrangible end of the spectrum, although it did not appear to him red. Herschel, accordingly, in addressing Dalton, says:—"It is clear to me that you, and all others so affected, perceive *as light* every ray which others do. The retina *is excited* by every ray which reaches it." And again,— "It seems to me that we [the normal-eyed] have three primary sensations where you have only two. We refer or can refer in imagination all colours to three,—yellow, red, and blue. All other colours we think we perceive to be mixtures of these, and can produce them by actual mixture of powders of these hues, whereas we cannot produce these by any mixtures of others. . . . Now, to eyes of your kind, it seems to me that all your tints are referable to two."¹ A similar conviction is stated by Herschel, in his treatise on light in reference to the colour-blind as a class:—"All the prismatic rays have the power of exciting and affecting them with the sensation of *light*, and producing distinct vision, so that the defect arises from no insensibility of the retina to rays of any particular *refrangibility*."²

Sir David Brewster thus writes:—"In all those cases [of colour blindness] which have been carefully studied, at least in three of them in which I have had the advantage of making personal observations, namely, those of Mr Troughton, Mr Dalton, and Mr Liston, the eye is capable of seeing the whole of the prismatic spectrum, the red space appearing to be yellow. If the red space consisted of homogeneous or simple red rays, we should be led to infer that the eyes in question were not insensible to red light, but were merely incapable of discriminating between the impressions of red and yellow light. I have lately shown, however, that the prismatic spectrum consists of three equal and coincident spectra of *red*, *yellow*, and *blue* light, and consequently, that much yellow and a small portion of blue light exist in the red space;—and hence it follows that those eyes which see only two colours, viz., *yellow* and *blue*, in the spectrum, are really insensible to the red light of the spectrum, and see only the yellow with the small portion of blue with which the red is mixed. The faintness of the yellow light which is thus seen in the red space, confirms the opinion that the retina has not appreciated the influence of the simple red rays."³

A reference to Dalton's own account of the appearance which the solar spectrum presented to his eyes, will show the compatibility of those conclusions with what I have now to urge. Dalton writes

¹ The quotations are from a letter as yet unpublished, but which will appear in the forthcoming life of Dalton, by Dr W. C. Henry, by whom I have been favoured with a perusal of it.

² Encyc. Metrop. Article Light, p. 434.

³ Letters on Natural Magic. 1832. P. 31.

thus:—"I found that persons in general distinguish six kinds of colour in the solar image, namely, *red, orange, yellow, green, blue, and purple*. Newton, indeed, divides the purple into *indigo and violet*; but the difference between him and others is merely nominal. To me it is quite otherwise. I see only *two*, or at most *three*, distinctions. These I should call *yellow and blue, or yellow, blue, and purple*. My yellow comprehends the *red, orange, yellow, and green* of others; and my *blue and purple* coincide with theirs."¹ Assuredly, this passage appears unhesitatingly to assert, that to its writer the red end of the spectrum appeared yellow; and the fact that Dalton permitted Herschel and Brewster to refer during his lifetime to him as having this impression of the solar image, seems decisive of the matter, especially when it is taken in connection with the fact that the great chemist had anticipated the possibility of red appearing black to him, and had decided by looking at vermilion that it did not so appear. It must be remembered, however, that there is no common language between the colour-blind and the colour-seeing; that Dalton gave in only a silent and negative adhesion to the opinions which I have quoted; and that vermilion, besides that it is not a pure red, is a colour so intense, that although it did not appear black, other reds might. And that other kinds of red than vermilion did appear black to Dalton, is shown by his own account. After the passage which I have quoted, as descriptive of his impression of the spectrum, he continues as follows:—"That part of the image which others call red, appears to me little more than *a shade or defect of light*; after that the orange, yellow, and green seem *one* colour, which descends pretty uniformly from an intense to a rare yellow, making what I should call different shades of yellow."² The language here is very precise. The red is not spoken of as appearing yellow, or *any other positive colour*; but as appearing defectively illuminated or dark, *i. e.*, more or less black; whilst the orange, green, and yellow are referred to as different gradations of one positive colour, namely, yellow.

It further appears that, in looking at coloured objects, Dalton was not liable to confound red with yellow, as he certainly should have done if these colours had appeared to his eye identical with each other. Thus, under the head of orange and yellow, as seen both by daylight and candlelight, he writes:—"I do not find that I differ materially from other persons in reference to these colours. I have sometimes seen persons hesitate whether a thing was white or yellow by candlelight, when to me there was no doubt at all."³ And under red as seen by daylight, he states, as already mentioned, that crimson and pink generally appeared blue, and scarlet was confounded with green.⁴

¹ Mem. Lit. and Phil. Soc. Manchester, 1798. P. 31.

² Op. et loc. lit.

³ Ibid. p. 34.

⁴ By artificial light it was otherwise; then crimson and pink became

Moreover, he tells us,—“All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud* which were very nearly alike. Crimson has a *grave* appearance, being the reverse of every showy and splendid colour. Woollen yarn, dyed crimson, or dark blue is the same to me.” Again:—“The colour of a florid complexion appears to me that of a dull, opaque, blackish blue upon a white ground. A solution of sulphate of iron in the tincture of galls (that is, dilute black ink) upon white paper, gives a colour much resembling that of a florid complexion. It has no resemblance of the colour of blood.” Again:—“Stockings spotted with blood or with dirt would scarcely be distinguishable.” Lastly:—“By day some reds are the least showy imaginable; I should call them dark drabs.”

It thus appears, that as Dalton saw the red end of the spectrum dark or darkish, so certain red objects showed to his eye as dark blue, dark brown, dark drab, mud-coloured, dirt-coloured, or even like ink. The most famous example of colour-blindness, therefore, should seem, although he did not fully realize the fact himself, to have been in certain circumstances blind to red.

(*To be continued.*)

ARTICLE IV.—*Enormous Growth from the Posterior Lip of the Os Uteri removed by Operation.* By E. R. BICKERSTETH, Surgeon, Liverpool.

Margaret Helpin, æt. 39, a poor woman, in a very weak and exhausted condition, and of an exceedingly exsanguine appearance, applied to me in the middle of June last for a constant flooding, which had continued with hardly any interruption for eight years. She was the mother of several children—the last, eight years ago, was delivered by craniotomy in the Rotunda Hospital, Dublin, on account of a “tumour of the womb” which prevented the natural birth of the child. After this period she had a constant bloody discharge, and at times profuse hemorrhage, from the womb. She suffered from all the severe symptoms of long continued loss of blood, extreme weakness, frequent loss of sight, palpitation, dyspnoea, and occasional syncope. She had slight pain at the lower part of the back, and complained of weight and fulness in the pelvis; also of a tumour which protruded between the vulvæ after standing for a short time. Her bowels were habitually confined and defecation very difficult.

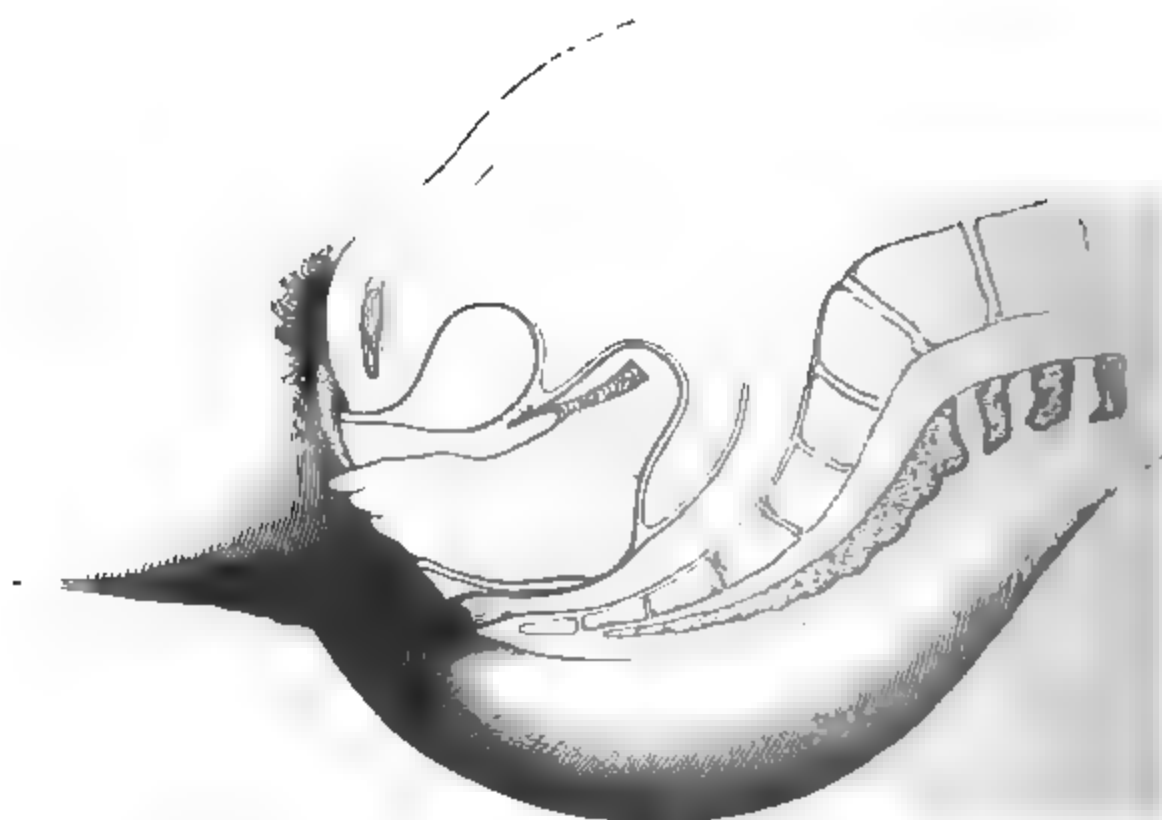
On examination I found the vagina filled by a firm growth of fibrous consistence. It was of a conical form, the base being uppermost, and apparently continuous with the posterior lip of the cervix uteri, while the apex protruded slightly between the labia. The anterior lip very thin and almost membranous was felt on the front surface of the tumour as high as the finger could reach.

yellowish red or reddish yellow, and red and scarlet appeared much more vivid and superb than by day. In keeping with this, Dalton states, that in the prismatic image of a candle-flame, “the red extremity of the image appears more vivid than that of the solar image.”

The os was very large, and its lateral dimensions so greatly increased as to be capable of admitting two fingers. On examination by the vagina it was impossible to ascertain the superior extent of the growth, but by introducing the finger into the rectum (which was considerably compressed) it could be felt to turn forwards rather abruptly about four inches from the orifice of the intestine. No enlargement or fulness could be discovered in the supra pubic or iliac regions, or indeed any other indication that could lead to the supposition of a tumour or hypertrophy of the body of the uterus.

The tumour felt of equal consistence throughout. It bled when disturbed, and the blood, as far as was ascertained, came from the region of the os. The diameter of its base, i.e., between the os and the posterior reflection of the vagina was not less than three inches; the measurement from side to side, at the same level, was still greater.

Fig. 1.



The diagram Fig. 1 represents an imaginary view of the pelvic viscera in section, showing the enormous development of the posterior lip of the uterus; the supposed condition of the body of the same organ, and also the rectum considerably compressed. It was drawn simply from the information ascertained by digital examination before any surgical interference was resorted to, and although I have since had an opportunity of inspecting all the parts, I am unable to make any alteration in the outline that could resemble the real condition more absolutely. The dotted lines mark the seats of the first and second operation.

After mature deliberation and repeated examinations, assisted by my friend Mr Grimesdale, we determined to attempt the removal of the tumour. We proposed, by continued traction, to draw down the tumour as far as to enable us to pass ligatures through its base, and then to tie or cut it off as appeared best. Accordingly, on the 2d of July, she was placed under the influence of chloroform, and strong hooks and forceps passed into the substance of the tumour at different parts. With these forcible traction was exerted for a considerable time, but with only slight effect on the tumour, for it was impossible to pull it outwards to any extent. Under these circumstances all that was done was to pass a strong needle armed with a double ligature of whip cord, from the posterior reflection of the vagina to a little below the orifice of the os. Each half

was then secured with the ordinary polypus tubes, and by tightening the ligatures occasionally a large slough was separated at the end of a week.

Very little constitutional disturbance followed this operation, and although the tumour was not much reduced in size, the poor woman felt better and more comfortable than for long before it. The hemorrhagic discharge, from which she had so long suffered, ceased and did not again return.

The tumour rapidly regained its former dimensions, and three weeks after the operation was even larger than before. Her distress returned, and she suffered in addition from occasional partial prolapse of the growth attended with retention of urine. The introduction of the catheter, and observance of the horizontal position for a few hours, sufficed to relieve her until the recurrence of a similar attack.

I did not see her for a week previous to the 25th of September. On that morning Mr Lewis was called up at an early hour to relieve her from retention. She had not passed water for several hours, and on examination he found an enormous mass, as large as a child's head, protruding from the vulvæ, and laying on the bed between her thighs. After emptying the bladder by means of a catheter he attempted for some time to return the prolapse, but without success. Shortly afterwards I saw the patient with him. It was evident that the entire tumour and part of the uterus itself were prolapsed, and that to return it, even if desirable, would be impossible. It was deeply congested, of a dark colour, hot, and dry. She suffered terribly from pain in the back and across the loins, and called loudly for relief. Her pulse was about 100 and of tolerable strength. She had no sickness or hiccough. In other respects she did not present any immediately alarming symptoms. By supporting the tumour on a pillow to prevent its dragging, and enveloping it in a poultice, she was rendered somewhat more comfortable.

On inquiry I found that for several days previously the tumour had protruded more than usually, but that as she did not suffer from retention she had neglected to attend to it. On the evening before when trying to pass water it had come down *en masse* suddenly. At the same time she suffered violent pain.

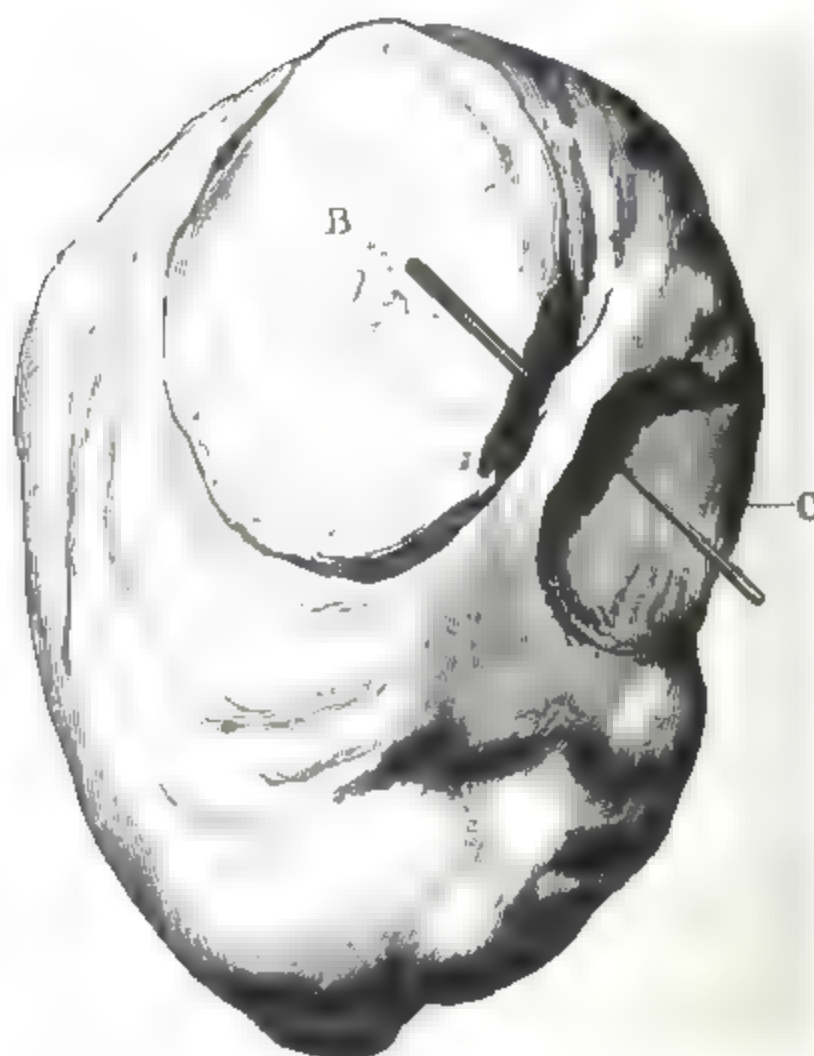
In the afternoon I was again assisted by Mr Grimsdale to a more complete examination of the prolapsed mass, and our preconceived ideas of its form and attachments were thereby completely verified. Not only the tumour, but a portion of the uterus itself, were protruded, so that we could now see the os laying on the surface of the tumour, and by introducing a sound we were able to ascertain that the cavity of the uterus was not more than two and a half inches in extent. The tumour had also a tolerably distinct neck, it being at this part not more than three inches in diameter, while the body of the growth was nearly twice as wide in every direction. She was suffering most severely from symptoms of strangulation; had occasional hiccough and feeling of sickness; the pulse small and frequent; the countenance anxious. Mortification of the protruded portion was evidently commencing. It was black and turgid with blood. There appeared very little prospect of her surviving a spontaneous separation of so large a mass, and we determined, on the following morning, should her condition be favourable, to remove the entire tumour.

September 26.—She passed a sleepless night in great pain, but did not appear in any respect worse than on the preceding day. Superficial gangrene had fairly commenced, and an indistinct line of demarcation formed round the neck of the tumour.

A strong ligature of whip cord was first tied as tightly as possible round the neck of the tumour, about an inch above the anterior lip of the uterus, and then, as an additional security, and also to ensure the patency of the cervical canal, it was transfixed from behind forwards at the same level with a double ligature, the needle being carried directly through the centre of the os, and each half firmly tied. Having done this the bistoury was carried round the tumour, and it was cut off close below the ligature. There was no bleeding. The uterus

immediately returned to its normal position. After two hours she expressed great relief and thankfulness that the operation had been performed. The same

Figs 2 and 3



day she was able to pass water without assistance ; the symptoms of strangulation disappeared, and she was in every respect comfortable within a few hours.

It had been intended to remove the ligatures surrounding the neck of the

uterus at the end of twenty-four hours, but as she continued perfectly easy and free from pain, with good pulse and returning appetite, this proceeding was put off from day to day until, on the sixth day, we cut the knots and took them away. On the eighth day she was able to sit up, and on the tenth she went down stairs, feeling well but weaker than before the operation. She was down stairs each afternoon for some hours for several days, and appeared to be slowly regaining her strength.

About a fortnight after the operation she complained of pain down the left thigh. There was nothing to be seen, but on vaginal examination I found a small soft tumour in the neighbourhood of the os. On pressing it firmly it broke down, and about a couple of ounces of clear pus passed by the vagina; the next day she was easier, but shortly the pains returned and became more severe, extending to the knee and calf of the leg. Nothing could be felt per vaginam beyond an irregular soft mass such as might be expected after an operation of the kind. Her health at the same time began to fail, and her pulse became quicker, being seldom under 100 and frequently 120 per minute. She had occasional slight rigors, and these shortly became more severe and more frequent. The tongue got dry and she complained of constant thirst. Pain attacked the right knee also, and was so severe in the left leg and thigh as to prevent sleep. Anasarca of the left leg then commenced and extended up the thigh, which became enormously swelled. It was evident that the poor woman was suffering from pyemia in a chronic form, and that a short time must terminate her existence. She became much emaciated, and died on the 31st of October, having survived the operation five weeks.

On examination of the body on the following day, all the abdominal viscera were found in a perfectly healthy condition. There were no abscesses, and no traces of inflammation of the peritoneum. The uterus and its appendages were of the normal size, weighing together not more than four ounces (see Fig. 2), while the tumour (Fig. 3) was seventeen ounces in weight. The accompanying wood-cuts are from drawings made by myself; they represent the uterus and tumour very accurately, and both are drawn in exact relative proportion. The posterior wall of the uterus was not thicker than is natural. Its structure on section was healthy. Its sinuses were large, but did not contain any deposits. Around the cervix uteri, which was not larger or thicker than in the healthy condition, was a deep furrow, evidently formed by the ligatures, and below this, attached by a slender neck to the inferior surface of the posterior lip, was a hardened and irregular slough, the size of a walnut, containing purulent depositions. (A Fig. 2). It was manifest the ligatures had failed to strangulate the entire thickness of the hypertrophied posterior lip; that a small portion in its centre had retained a certain degree of vitality, and that in it the purulent depositions had taken place. The circumstance of my having felt, a fortnight after the operation, a soft tumour which broke down under my finger and discharged pus was at once explained. All the veins about the rectum, vagina, and bladder were gorged with dark blood, and many of them contained partly decolorized clots. I traced several directly to the slough described, and one in particular, opening into the inferior iliac vein, was completely choked up for some inches. Both the external and internal iliac veins contained large firm decolourised clots, and at the junction of the former with the vena cava was a solid mass nearly as large as a dried date. On section it presented a small cavity in its interior filled with thick brownish pus.

The bladder was slightly thickened, and its mucous membrane injected. The thorax was not examined. The tumour weighed, after removal, 17 oz. It was of an irregular oval form, somewhat flattened from before backwards. (See Fig. 3). The large circular space upon its anterior and upper part (B) represents the flat surface, as it was cut off through the neck of the uterus. It measures from three and a half to three inches in diameter. The smaller rounded patch below, and to the right (C), is the uterine os, while, between the two is seen the narrow band which constituted the attenuated margin of the

anterior lip of the os. A pen-holder is placed under this to display more distinctly the portion of the cervical canal removed along with the tumour. The greatest transverse diameter of the tumour is obliquely across, from the left to the right side, and from before backwards. In this direction it measures six and a half inches; crossing it in the opposite direction, on the same level, it is four and a half inches. Its extreme length, from base to apex, is eight inches. Its surface is nearly regular, except at the lower and anterior portions, where it presents some slight nodulated projections. Previous to the commencement of the gangrene its exterior was smooth and shining, but now it is ash-coloured, and disorganized to the depth of one or two lines. The substance of the tumour is uniformly dense at every part. It is composed throughout of firm fibro-nucleated tissue. On section it has a pale rose-coloured appearance, and thick white bands of firm fibrous structure are seen to traverse it in every direction. The vessels running through its substance are small and round, and do not bear any analogy to uterine sinuses.¹

The extreme rarity of tumours occupying this portion of the uterus is well known to all pathologists; but what is specially remarkable in the case is, the extraordinary size it had attained without destroying life by hemorrhage, or irrecoverably involving the essential structure of the uterus itself. Although the posterior wall of the uterus was three inches in thickness at the time of the operation, it had diminished to its normal size in five weeks; a new os, not larger than is usual, had formed, and every part had so completely regained its natural condition, that it was with difficulty conceived that a tumour of the magnitude described, and of so extensive an attachment, had been recently removed. The subsequent examination has shown, that the operation was not only practicable, but that it was physiologically and pathologically correct, and that but for the unfortunate occurrence of phlebitis, the poor woman would have recovered, and would probably not have been liable to a return of the disease.

On reviewing the case, I cannot help thinking it would have been better to have cut the ligatures within a few hours after the operation, and to have trusted to check bleeding by plugging the vagina. Or if this plan had not been adopted, the polypus tubes might have been introduced, and the ligatures tightened day by day, so as at last to bring away the *entire* slough. No half-strangulated and inflamed substance would then remain to serve as a nidus for purulent depositions, and the risk of phlebitis would be proportionably less.

ARTICLE V.—*Case of Excision of the Knee Joint.* By WILLIAM KEITH, M.D., M.R.C.S. Eng., Senior Surgeon to the Royal Infirmary, Aberdeen, and Lecturer on Clinical Surgery.

IN the present state of the question, How far excision of a diseased knee joint is a useful or a warrantable operation? no apology is deemed necessary for giving publicity to the annexed case.

¹ This remarkable tumour is preserved, together with the uterus, in my private museum, where I shall be happy to show it to those interested in the subject.

The patient having been this day discharged cured, the duty next felt to be incumbent is to make known the whole particulars of the case to the profession, with a view to advance the progress of such practice if really an improvement in surgery—or share in the blame if in the end the attempt should turn out to be a failure—to plant this operation high in the list of those which constitute conservative surgery.

John Hay, aged 9, from Old Aberdeen, of small stature, and delicate look, but of a happy and cheerful disposition, was admitted, on the 7th November 1853, into the Royal Infirmary, Aberdeen, with scrofulous disease of the right knee joint, which has existed, better and worse, for twelve months. The leg is fixedly bent on the thigh at an acute angle, the heel almost touching the nates. The knee joint is much enlarged generally; the head of the tibia and condyles of the femur evidently expanded; the capsule of the joint is distended and feels pulpy. There is tenderness on pressure over every part of the joint, and actual pain on any attempt being made at movement or extension. The diseased limb, owing to wasting, looks, as a whole, diminutive beside its fellow. His general health is good, though he is reported to have had occasional attacks of diarrhoea within the past three months. His spirit is buoyant, and he earnestly desires to save his limb.

November 22.—It was agreed on consultation that excision of the joint should be practised in his case.

On Saturday, 26th November, at 10.30 A.M.—Under chloroform. An incision was made from the inner to the outer condyle of the femur, in a semicircular line, the point of the flap reaching to the head of the tibia, the ligamentum patellæ being there cut through. The flap, including the patella, was dissected from all its connections, to a line fairly above the condyles; the lateral and crucial ligaments were cut, when the utmost facility presented for sawing off the condyles of the femur. The articulating surface of the tibia was then sawn off from behind forwards, the line of section not reaching so low as the fibula. Two inches in whole being the exact measure of the two portions of bone removed at the operation. The face of the patella was implicated so far as to have become partially adherent to the trochlea of the femur, and was to some extent denuded of cartilage, it was therefore smoothed by the removal of a thin slice by the aid of a farrier's paring knife; the remaining portion of the patella being evidently healthy was allowed to rest in situ. Two small arteries only required ligature. Indeed the operation might be designated as bloodless, so little hemorrhage having occurred. The wound was closed by six stitches, dressed with ointment, compresses, and bandage, and the limb extended to the straight without any difficulty—laid and secured in a well-fitting Macintyre's metal fracture frame. The whole proceeding may have occupied five minutes. He was put to bed still under the influence of chloroform. To have eight drops of laudanum so soon as he awakes.

7 o'clock P.M.—Has slept quietly and with little interruption since the operation; says the limb is quite easy; not a drop of blood has oozed from the wound. Has taken soup in small quantities, but feels squeamish, probably from the effects of the chloroform.

Sunday, November 27 (first day), 7 o'clock A.M.—Became uneasy and fretful at midnight, and had a second anodyne (eight drops) administered, since which has passed a quiet night, and is still sleeping soundly. To have an egg and tea to breakfast. 10 o'clock A.M.—Feels easy; seems well; took his breakfast with relish. To have soup and chicken to dinner. 2 o'clock P.M.—Is cheerful and easy; relished his dinner. 7 o'clock P.M.—As at last report. Having sunk into the mattress he lies twisted, and is therefore relaid and squared up. Bowels have not moved since Friday; give three grains of rhubarb pill to-night.

Monday, November 28 (second day), 7 o'clock A.M.—Slept the first half of the night; towards morning felt the limb uneasy, and the bandage tight; removed the whole dressings; found the flap adherent all round, the knee in no degree tumid. Dressed it lightly with ointment, compress, and many-tailed bandage, the latter surrounding both metal case and limb. 8 o'clock P.M.—Ate his dinner with relish; seems well; complains only of an occasional starting of the limb. Bowels have not moved; give 5 gr. rhubarb pill.

Tuesday, 29th November (third day), 10 o'clock A.M.—He required ten drops of laudanum at 11.30 P.M., after which had an excellent night's rest; feels well, and is cheerful. The joint looks natural, and as shapely as if never touched. The wound seems quite united; removed two of the stitches. Bowels not moved; give a cup of senna with Rochelle salts 3i. Continue soup and roast chicken. 8 o'clock P.M.—Bowels have moved freely three times. To have ten drops of laudanum if required for rest, meantime he is easy and disposed to sleep.

Wednesday, 30th November (fourth day), 11 o'clock A.M.—Slept soundly; feels refreshed. Wound looking firm; one ligature has come away.

Thursday, 1st December (5th day), 10 o'clock A.M.—Slept soundly after getting six drops of laudanum; relished his breakfast; removed other two stitches. The wound united throughout. 9 o'clock P.M.—Cheerful and easy; bowels slow; to have a small cup of senna infusion in the morning.

Friday, 2d December (6th day), 10 o'clock A.M.—Passed a good night; bowels well emptied two hours after having got the senna. Dressed the wound, and removed the fifth stitch. 8 o'clock P.M.—Has occasional starting when asleep, which pains the limb at the time. The dressings renewed in consequence of being moistened by a serous exudation, which escapes at a small channel formed by the presence of the one remaining arterial ligature, at the outer angle of the wound. The knee looks well and quiet; bowels rather loose. To have ten drops of laudanum.

Saturday, 3d December (7th day).—Slept well; feels comfortable. Dressed the wound; very little discharge; removed the remaining stitch; union complete. 8 P.M.—Complains of his heel; supported it by a small pad of wadding, placed under the Tendo-Achilles. 8 drops of laudanum to be given.

Sunday, 4th December (8th day), 10 o'clock A.M.—Slept well; feels well. The one remaining ligature came away. Meat diet, and two oz. of port wine daily. 7 o'clock P.M.—Bowels have moved naturally; relished his wine. Lifted the limb out of the metal frame; renewed the whole paddings and dressings, and replaced the limb as formerly, with little complaint on the part of the patient.

Monday, 5th December (9th day), 10 o'clock A.M.—Slept well; feels easy. The discharge from the orifice, formerly mentioned, is very trifling, but indicates by its odour the separation of a portion of bone.

Tuesday, 6th December (10th day).—Had a good night. Can feel with the probe a portion of bare bone at the outer angle of the wound, as yet firmly adherent to the shaft of the femur. Increase the wine to three oz. daily.

Wednesday 7th December (11th day).—As at last report; bowels opened naturally.

Thursday, 8th December (12th day).—The femur projects somewhat to the right; its bare point presents itself at the small orifice at the external angle of the wound, occasioning by its pressure tension of the skin and pain. Enlarged the opening, and clipped off a full half inch from the shaft of that bone, which was the extent detected by the finger, to be denuded of periosteum. The patient being under chloroform at the time, the opportunity was taken to remove MacIntyre's frame; and after having fairly straightened and fully re-adjusted the limb and dressings, it was secured in that position by Desault's long straight splint.

Friday, 9th December (13th day).—Required an opiate at midnight. Appetite is impaired to-day; bowels open.

Saturday, 10th December (14th day), 10 o'clock A.M.—Appetite returned; feels well, and is cheerful. At his urgent request, the limb was replaced on Mac-

Intyre's frame. The object in view, when applying the long splint, has been attained, viz., straightening both thighs on the pelvis; and now he promises to keep himself straight. 8 o'clock P.M.—Experiences much comfort from the change of splint. Bowels have moved naturally.

Sunday, 11th December (15th day), 10 o'clock A.M.—The best night he has passed since the operation. The knee looks well; the pus, small in quantity, is thick and creamy. The wound recently made is filled to overflowing by four drops of turpentine liniment, which is now the sole dressing. Three oz. of wine daily, and good diet.

Monday, 12th December (16th day).—Going on well. He is sitting up in bed. Bowels regular.

Thursday, 15th December (19th day).—Well, as at last report.

Tuesday, 20th December (24th day).—Can move his limb with little assistance, and now without any pain. He moves about in bed freely.

1854. *Saturday, 14th January* (49th day).—Has gone on improving. A mere drop of matter oozes from a point at each end of the cicatrix. A superficial abscess offers to form on the upper and outer aspect of the flap, from no very apparent cause. He moves the limb about with great confidence, as if the bony union were firm and complete.

Wednesday, 18th January (53d day).—Fluctuation felt at the point above indicated; an opening was made, and nearly half an ounce of ripe pus evacuated.

Friday, 27th January (62d day).—The wound has healed up. The boy stands firmly on both feet; the right limb, straight as an arrow, wants only one inch under the heel to make the bearing on both limbs equal. Allowed to rise and dress daily.

Friday, 10th February (76th day).—The wound firmly cicatrized. He is up daily, and going about on crutches. The joint has much the shape of its fellow; is solid to the feel, from the mass of callus present, and is entirely stiffened by ankylosis. The thigh on the right side is more plump than on the left, no doubt, in consequence of the shortening of the shaft, causing the muscles to belly out from relaxation. The joint bears handling freely.

Thursday, 16th February (82d day).—He is daily going about, up and down stairs. By bending his other knee a little, he can walk with both heels on the ground. Is requiring no more surgical treatment, and might be dismissed cured.

Friday, 10th March (104th day).—He has been kept under observation, and permitted to run about the hospital until to-day, when he is allowed to return home. The right limb is just one inch shorter than its fellow, and is now the thicker of the two, both leg and thigh.

Remarks.—The mode of operating was that recommended and practised by Mr Richard J. Mackenzie of Edinburgh. The simplest and most efficient that can well be conceived. A semilunar incision extending from condyle to condyle and bisecting the ligamentum patellæ, gives, on dissecting the flap upwards, complete access to the whole joint, and admits of the easiest possible disarticulation, and removal of the diseased portions of bone. No portion of integument was dispensed with, and it is clear never should be. The cicatrix now shows a mere line. The ligament of the patella is united where it was cut as firmly as if never divided. The patella was spared in its seat, and adds by its presence not only to the symmetry of the limb, but also to the solidity of the joint, and to the firmness of those attachments between the thigh and the leg so necessary in progression, and at the same time so needful to preserve the limb straight under the weight which it will, in walking, have to carry. The patella is seldom if ever primarily or extensively diseased, so that all

that can usually be required is to pare a thin slice from its articulating surface, and this is very easily accomplished by the curved paring knife.

In the operation, as a whole, the cut surface exposed was less by one-third than in a flap amputation of the thigh. Bleeding there was none, and shock there was none. Less of either than was ever witnessed by the operator at any amputation he had ever seen. The period on the table is brief as compared with what is occupied in taking up the numerous arteries after amputation for chronic disease of a knee joint. Two minutes might suffice for completing excision of the knee joint and closing the wound permanently, but it were hopeless to make any such attempt with amputation; and it is important, on the patient's account, to keep him as short a time as possible on an operation table.

The after condition of John Hay proved the entire absence of shock to the system; no fever followed; his appetite was unimpaired, and any suppuration that occurred was trifling in the extreme.

As to the nature and period of his confinement after the operation, he was able on the sixteenth day to sit up in bed; on the sixty-second day he was allowed to dress and go about on crutches, bearing part of his weight on the right foot; on the eighty-second day he was so firm on his feet, and so sound about the joint, as to be considered no longer under surgical care, a period which any surgeon would gladly accept as a favourable average, if obtainable after amputation of the thigh in a scrofulous subject.

The important question has been asked,—Should the bones be united by ankylosis after excision of the knee joint, or should any attempt be made to form a moveable or artificial joint?

In confirmed scrofulous disease of the knee joint, ankylosis, if accomplished with the limb nearly straight, is very justly considered a favourable termination to the case—the very fixing of the joint affording a security against the recurrence of like disease in the same joint. Considering, then, the kind of constitution that is so likely to exist in the subject requiring excision of the knee, the risk would be great indeed that diseased action would immediately commence were the denuded head of the tibia required to form a socket in which the end of the shaft of the femur was to roll and rotate, and that also while it had to support the superincumbent weight of the whole body in locomotion.

There cannot be a doubt, therefore, that ankylosis must be secured in every instance to ensure safety to the limb, and *solid* comfort to the patient. The femur and tibia being both sawn across, callus readily unites them, and the patella, intentionally denuded of cartilage, occupies a most important, as well as favourable position, for forming a strong connecting link or band between them, whether the union of the three be by perfect osseous intermixture, or partially by fibrous tissue, the limb straight and strong from being stiff, will safely encounter the fatigues of travel, not more, but now less, sub-

ject to disease than the opposite limb. In the case just recorded, the bony union is complete, proving that such is practicable; it is comfortable to the patient and useful. It needs no prophet to foretell, that as sure as it is the rule of practice to ensure mobility in the elbow after excision of that joint, so sure will the rule be to secure immobility after excision of the knee joint.

ARTICLE VI.—*On a Speculum adapted for employment during Operations on the Mouth, under Chloroform.* By J. SMITH, M.D., Dentist, Edinburgh.

IN the performance of surgical operations on those parts situated within the cavity of the mouth, considerable inconvenience is occasionally experienced by the operator in preventing the jaws and lips from being so much closed as to render it difficult for him to obtain the requisite amount of space, or a sufficiently clear view, for enabling him to proceed with the necessary manipulations. In those cases where the patient is previously subjected to the influence of chloroform, or other anæsthetic agents, the difficulty becomes still greater, not only from the involuntary, or rather unconscious, struggling which occurs, but from the half-opened state in which the mouth generally remains during anæsthesia; except, indeed, in those instances, which are not uncommon, where, upon attempting to operate within the mouth during the influence of chloroform, it not only remains imperfectly open, but is obstinately and forcibly shut. Several contrivances with the view of obviating this difficulty have from time to time been brought forward, but all of them partake, in a greater or less degree, of the disadvantages of the one most commonly employed, namely, placing a cork between the patient's teeth. But no such appliance can be sufficiently well fixed in its position as to prevent the displacement of it by the efforts of the individual in whose mouth it is situated; while, owing to its size and solidity, the introduction of instruments, and the entrance of light, become to a considerable extent interfered with. Even although confided to the care of an assistant, such an apparatus is exceedingly apt to be disturbed during the movements and efforts at resistance which are frequently made by patients under such circumstances; and, in cases where there may be no assistant at hand, or where he is occupied in some other manner than attending to the position of the instrument employed for this purpose, it becomes almost impossible to retain it in its place, and when it does shift, or altogether drop from the mouth, the delay occasioned by its readjustment, especially in such instances as those where the operation has been commenced, renders such an apparatus inefficacious, if not attended with danger.

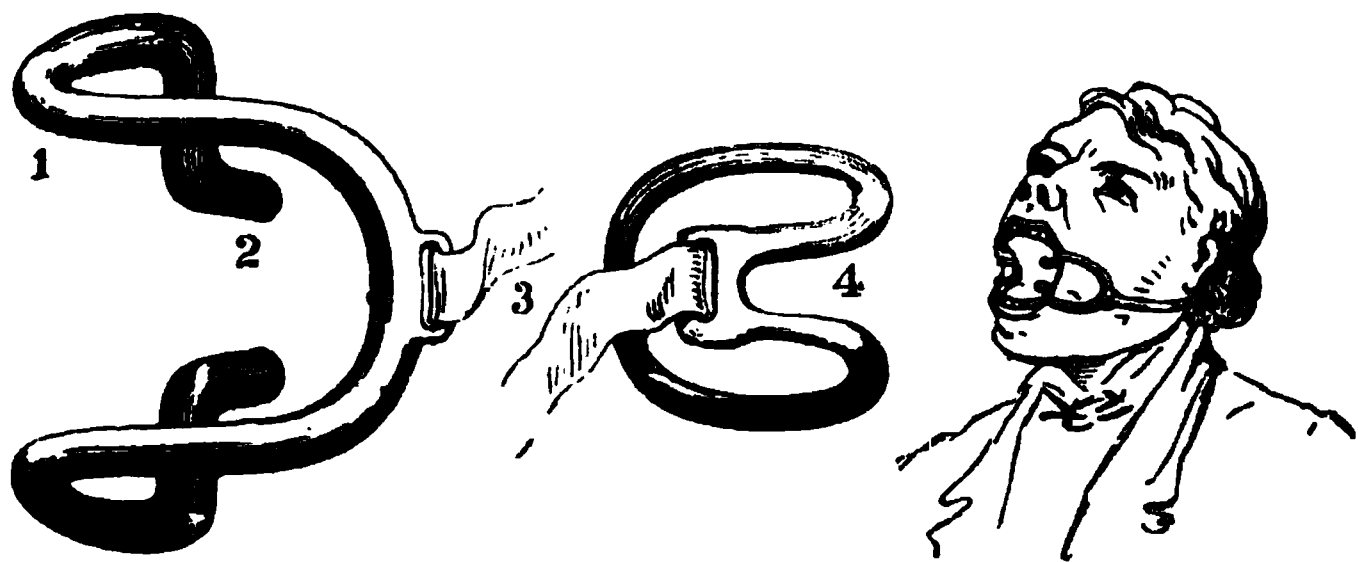
From experience in these difficulties, more particularly as they occur during the extraction of teeth, as well as from that of other

surgeons in more important operations on the structures in this region, I cannot imagine any example in which an instrument, specially adapted for one particular purpose, could be more required; and my attention has been of late more immediately directed to the subject, by observing the extreme degree of difficulty experienced in the removal of the tonsils in a child, arising from no other cause than merely the want of any suitable means for maintaining the mouth in an open state.

No doubt, in many cases, the employment of a cork, or some such body, placed between the upper and lower jaws, serves perfectly well for enabling the operator to proceed with facility; but it is obvious that, upon any further separation of the jaws by the patient than the precise distance at which they are held by the cork, it will at once drop out; and even where an assistant attempts to retain it in its place, the sudden turning of the patient's head, or even the movement of the surgeon's hand, renders its position exceedingly liable to alteration.

Under these circumstances, I have been led to consider whether any simple means might be contrived which would answer the purpose of maintaining the jaws and lips in an open condition during the state of anæsthesia, and which, while it should occupy as little space as might be consistent with sufficient strength, would be in a manner self-adjusting—of easy application and removal—and not apt to shift from the position in which it might be found most convenient for the surgeon to place it.

The accompanying diagram illustrates what, I trust, will be found suitable for all those purposes, and serviceable for the requirements of the ordinary run of cases:—



The apparatus may be divided into three parts, viz.—

1st, The larger steel portion, adapted for at once holding back the edges of the lips, and keeping the jaws widely separate, on one side of the mouth.

2d, The elastic band, attached by one extremity to this, and intended to be passed round behind the patient's neck, and brought to the other side of the mouth, where,

3d, The flat hook is attached to it, and serves at once to keep the lips back at this side, and the whole instrument in its place.

The general configuration of the apparatus is given in the drawing, where the larger portion above-mentioned is seen to consist of a piece of steel, bent into a semicircular shape, the two free extremities of which are again bent back so as to form two smooth hooks (1), intended to be introduced within the lips, and serving to retract them and the cheek. These hooks terminate by having their ends again turned inwards (2), so that from resting flatly on the internal surface of the cheek, they pass between the upper and lower jaws, which rest upon and are kept separate by them. If it were considered necessary, these two ends might be covered with caoutchouc, or some other substance, for the purpose of rendering them more soft, and thereby protecting the teeth or gums impinging upon them, from the chance of injury. At the other extremity of the instrument the hook (4) is attached, and these two metal portions are connected by the India rubber band (3), which passes behind the patient's head, and retains the apparatus steadily *in situ* until the operator thinks proper to remove it. The instrument could, by means of a joint and regulating screw, in the portion intended for separating the jaws, be made capable of adaptation to any size of mouth, or to whatever extent the jaws in different cases might open; but as this would detract from the lightness and simplicity of the apparatus, it would be better to have two of different sizes, since the distance to which the jaws may be separated varies considerably in different individuals, and, indeed, were it for children alone, a smaller one would be indispensably required.

I am not aware of any other instrument of this construction being in use; and while I feel confident that this one will be found suitable for those cases falling under the province of the Dentist, I believe it will be found of some service in many of those operations which more properly devolve upon the care of the Surgeon, since, from its simple construction, it seems capable of application wherever it is at all possible to employ any apparatus of the kind with safety or advantage. From the small amount of room also occupied by it, and its being equally adapted for either side of the mouth, according to what the surgeon may consider most advisable or best suited for his purpose, it can scarcely form any material obstacle during the performance of the greater number of operations required in that locality.

Part Second.

REVIEWS.

The Pathology and Treatment of Stricture of the Urethra. By HENRY THOMPSON, F.R.S.E., M.B., London. 8vo. 1854.

THE subject of stricture of the urethra, at all times one of the greatest interest to the practical surgeon, has of late years attracted special attention in consequence of Mr Syme's proposal, of a new method of treating certain aggravated forms of the disease by external incision, which has led to much controversy on the subject. Those who have paid much attention to the various opinions advanced must have often felt that many of the controversialists who grounded their views upon great experience in treating stricture seemed to forget that such experience was, after all, generally limited to the use of some one special plan, and hence it has long appeared to us desirable that the subject should be thoroughly investigated as a whole by some independent and candid observer. It seems also as if it had been the object of the College of Surgeons of England to obtain such an investigation by making it the subject of their Jacksonian Prize for 1852. In the book before us we have an answer to the appeal; and one which we consider of such importance that we intend to examine it at some length.

The work commences with an inquiry into the anatomy and functions of the urethra and of the various textures with which it is in relation, a thorough knowledge of which, as the author justly observes, is essential to enable the inquirer to arrive at truth in reference to many disputed points in the pathology and treatment of diseases of this organ. Mr Thomson seems to have devoted considerable attention to this important preliminary inquiry, and to have been at great pains, both to make himself acquainted with the observations of others, and by repeating and varying these investigations himself, to render it as complete as possible.

The points to which attention is specially directed are,—1st, the measurements of the urethra, its structure and divisions; 2d, the structures connected with it which determine, to some extent, the form, position, and general direction of the canal, or which, under certain conditions, may affect its calibre. With regard to the measurements of the length of the canal, Mr T. gives the results of the examination of the urethra in sixteen adult male subjects, in which the measurements were carefully conducted by himself. The average measurements in these cases are :—

“ Total length, from anterior border of uvula vesicæ to
meatus urinaris externus, 8½ inches.

Dividing the canal in the usual manner into spongy,
membranous, and prostatic portions, we have—

Length of <i>spongy</i> portion,	6½	”
” <i>membranous</i> ditto,	¼	”
” <i>prostatic</i> ditto,	1½	”
	<hr/>	
	8½	

“ The greatest measurement was 9 inches, the smallest 7½ inches. Of the 16, no less than 10 presented measurements which did not deviate more than ¼ of an inch from the average, and ranging within ½ of an inch only ; that is to say, between 8½ to 8½ inches inclusive.”—P. 3.

He then refers to measurements made by Mr Briggs, of the Lock Hospital, on the living subject, and repeated and verified by himself, in which the average length is stated at from 7½ to 7¾ inches. In noticing the apparent discrepancy between the measurements on the living and the dead subject, he says :—

“ His (Mr Briggs) experiments were made upon the living subject; and, inas-
much as the practical benefit of these researches must be found in relation to
the use of instruments during life, it is confessedly of more importance to
ascertain, if possible, the length of the canal in that condition, than after death.”
—P. 4.

And then he draws attention to the absolute value of both measure-
ments, the one as serving to guide the surgeon in the use of instru-
ments and examination of the passage during life, the other as being
necessary for the purpose of giving exactitude to accurate researches
into the pathological anatomy of stricture after death. The method
employed in the post-mortem measurements of the urethra is one
which has been very generally resorted to, and has advantages ; but
a knowledge of it, we think, will serve to explain the apparent dis-
crepancy of the results in the living and the dead body. We quote
his words :—

“ I have pursued the following course with a considerable number of bodies,
which it has fallen to my lot to examine. The penis and bladder having been
carefully removed from the pelvis, in the usual manner, the entire passage is
laid open along the upper aspect. The parts are then placed, being first mo-
derately extended, upon some smooth polished surface, as on a common earthen-
ware dish, and so permitted to take, by their own elasticity, any form or length,
which their component structures may determine.”—P. 3.

It will be readily perceived that there are two sources of fallacy in
this plan, viz., 1st, the indefinite nature of “ moderate extension,”
for, as our author afterwards shows, a very slight extension will
make “ great difference in this lax state of parts ; but 2dly and
chiefly, the urethra being detached from its fascial connections, its
curvature is effaced, and the membranous portion, formerly extended
between the laminae of the triangular ligament, is relaxed and short-
ened on itself. The measurements on the living serve to correct
these fallacies ; but they in turn are liable to objections, and can

never give the definite certainty attainable by careful post-mortem measurement, even as to the whole length of the canal, far less as to the relative lengths of the different portions into which it is divided. The plan which we think the most useful, and one combining the advantage of anatomical precision, whilst at the same time the relations are maintained exactly as during life, is as follows. The prepuce being well drawn forwards over the glans penis, two hare-lip needles are thrust through it near its orifice, and behind the needles a strong waxed ligature is firmly tied so as completely to constrict the prepuce in front of the glans penis. The bladder and urethra are then to be moderately distended with alcohol, injected slowly by the ureter. After the textures have thus been hardened, a careful section of the bladder and urethra can be made without disturbing the natural relations or curvature of the canal, and thus not only may accurate measurements of the whole length be obtained, but the definite length of the spongy, membranous, and prostatic portions, their relations to each other and to surrounding parts, together with their depth from the surface of the perinæum, accurately determined. We have made many such sections, and have taken casts from them ; in very fresh, firm subjects, the section can be made, as far as regards the urethra, well enough without hardening with alcohol. We may here subjoin the results of our measurements so made.

Measurements of Urethra.

[The measurements were taken by means of a piece of thin waxed cord laid along the canal following its curve.]

I. Measurement of urethra ; parts in natural condition, penis resting on right groin.

Total length of urethra from meatus urinarius externus to anterior border of uvula vesicæ, 7 inches $11\frac{1}{2}$ lines, or almost 8 inches.

Spongy portion.—From meatus externus to commencement of membranous portion at its entrance into the anterior layer of triangular ligament, 6 inches, 4 lines.

Membranous portion.—From its entrance at the anterior to its exit at the posterior layer of the triangular ligament, half an inch, or 0 " 6 "

Prostatic portion, 1 " $1\frac{1}{2}$ "

Total length, 7 $11\frac{1}{2}$

Measurement of the walls, membranous portion between

layers of triangular ligament, { Upper wall, 5 lines.
Lower wall, $\frac{1}{2}$ inch, or $4\frac{1}{2}$ "

II. Measurements of urethra ; penis previously injected with wax, so as to represent the erect condition.

Total length of urethra, from meatus externus to anterior border of uvula vesicæ, $9\frac{1}{2}$ inches.

Spongy portion to orifice at anterior layer of triangular ligament, $7\frac{1}{2}$ inches.

Membranous portion, $\frac{1}{2}$ "

Prostatic portion, $1\frac{1}{2}$ "

Total, $9\frac{1}{2}$

The exact measurements of the width of the urethra are very difficult to obtain ; our results can be but approximative. We agree with our author, that the best plan is the old one, of taking casts by filling the bladder and urethra with wax or fusible metal, and so obtaining the width of the canal at different points when dilated by a moderate and equal fluid pressure. In reference to these measurements of width, Mr T. concludes with the following judicious remarks :—

“ The value of these researches is found in the practical application of the principles which result from them, to the employment of instruments in the urethra during life. Granted that constant relations of size between the different parts of the canal exist, and that the external meatus is known to be, with very few exceptions, the smallest of all ; it follows that an instrument which fills that orifice without over-stretching it, must be able to pass through its whole course, unless some obstruction be present. Thus, to some extent, it may be regarded as a key to the capacity of the rest of the canal. As regards the actual average of measurements met with in practice, it is seldom that No. 12 cannot be fairly introduced into the adult urethra, while Nos. 13 or 15 are often admissible. The diameters of these instruments are, respectively, three-tenths and three and a half tenths of an inch.”—P. 8.

Our author next proceeds to examine the membranous, muscular, and erectile textures immediately in relation with the urethra, which may affect its condition in reference to examination or treatment. The description given of the perineal fascia is correct enough in its main points in relation to the urethra, but some expressions might mislead. Thus, in describing the two layers of the deep perineal fascia (triangular ligament), Mr T. says,—“ The posterior layer becoming thinner at its lower border, subsides into a fascial covering for the levator ani muscle. The anterior is in contact in front with the muscles of the perineum, and its lower border turns forward beneath the transversus perinei muscles to become continuous with the superficial fascia of the scrotum and abdomen, which dips down to join it.” Now, this would lead us to understand that the two layers of the deep fascia were distinct and separate at the point where the superficial fascia joins it, whilst, in reality, both laminæ of the deep fascia converge and become blended together, forming one structure about four lines in front of the point where the superficial fascia is reflected upwards to join it. That junction corresponds to the base of what anatomists have termed the anterior or urethral triangle of the perineum, and from the point of junction, the ischio-rectal layer descends to cover the perineal surface of the levator ani. We think, also, that our author would have done well, even at the risk of a little circumlocution to have incorporated the important anatomical fact mentioned in the foot-note at page 15, into the text, as explaining the real connections of the structures which close the pelvis inferiorly. In concluding his notice of these faciæ he says :—

“ The important connections of the faciæ can only be sufficiently demonstrated by careful dissection. There are no preparations in our museums which

show them well. But those who have the opportunity so commonly enjoyed by students of the present age, of prosecuting their studies in Paris, will do well to examine, at the Museum of the Ecole de Médecine, the finest series of dried preparations of the genital organs in existence; in which the fasciæ especially are most elaborately dissected and beautifully shown."—P. 16.

Mr Thomson must be better acquainted than we are now-a-days as to the state of the London museums; but, as we understand that he pursued his investigations for some time in Edinburgh, we protest against such a sweeping declaration in regard to our schools. We have ourselves made a few preparations to exhibit these fasciæ: one series of these perineal and pelvic dissections, in the possession of Prof. Goodsir, our author might have seen at our university; and our students need not be driven to the necessity of visiting Paris to get a good view of the pelvic and perineal fasciæ.

A careful examination of the anatomy of the muscular tissues surrounding the urethra follows, and the mucous canal is shown to be closely connected by its sub-mucous tissue with involuntary muscular fibre in every part of its course, not existing, however, in equal quantity throughout, and at some points interlacing with portions of the yellow elastic tissue. In the description of the voluntary muscles which act upon the urethra, the compressor urethra receives a full share of attention, and our author coincides in the description of Guthrie and Müller, limiting the terms to transverse fibres placed above and below the membranous portion of the urethra, and to the internal layer of circular fibres described by Müller as immediately surrounding the canal. With reference to the perpendicular fibres, described by Wilson as descending from the pubis, he states that, although he has occasionally met with muscular fibres descending from the symphysis to the prostate and urethra immediately in front of it, "these appear to belong to that anterior part of the levator ani called levator prostatae." Repeated dissections of these muscular fibres have led us to very different conclusions, for whilst it is quite true that the description of these fibres by Mr Wilson is erroneous from his plan of dissection, detaching the lateral connection of the muscle, and allowing it to fall inwards, and so assume a perpendicular sling-like form, yet we consider it is nearer the true description than that given by Mr Guthrie. We think the plan of dissection which Mr Thompson deems essential for the proper examination of this muscle is the one of all others likely to mislead as to its true relations and form. He says, "it must be sought in a fresh body, and from the inside of the pelvis." Now, whilst this plan is admirably adapted for some views of the pelvis, especially for showing the levator ani in its relations to the viscera, it is by no means well suited for exhibiting the compressor urethræ; for to see that muscle from this view, the posterior layer of the triangular ligament must be divided, and thus, the natural septum between the compressor and the anterior fibres of the levator being removed, mistakes occur regarding the connections of these muscles which could never arise

were the natural relations maintained. There are very few plans of dissecting the pelvis which we have not tried; we have repeated Müller's dissections, and modified and planned new views of the pelvis; each plan has its own special advantages; but after much experience, we feel no hesitation in recommending the one subjoined as decidedly the best for exhibiting a connected view of the exact relative anatomy of the structures in the perineum and pelvis.

Description of Dissection.—In dissecting the perineum, after cleaning out the fat from the ischio-rectal fossæ, without removing the ischio-rectal or obturator fasciæ, the superficial perineal fascia should be detached from its lateral connections and reflected backwards. The perineal muscles are to be cleared away so as to expose the crura penis, bulb, the anterior layer of the triangular ligament (deep perineal fascia), and the junction of the superficial with the deep fascia behind the transversus perinei muscles. These parts being left, next proceed to connect the perineal view with the pelvic by a section made as follows:—After division of the soft parts, make the section of the left os innominatum, commencing about $1\frac{1}{2}$ inches external to the symphysis pubis and carried downwards through the obturator foramen, with a slight inclination outwards, dividing the tuber ischii nearly through its centre, so as to leave a considerable part of the attachment of the great sacro-sciatic ligament; the spine of the ischium is next to be detached from the body of that bone by dividing it at its base with Hey's saw or bone-pliers, thus preserving the attachments of the lesser sciatic ligament and the natural connections of the levator ani and pelvic fascia at the posterior part of the pelvis; lastly, the complete removal of the left os innominatum is to be effected either by disarticulating it at the sacro-iliac synchondrosis, or by sawing it through externally to that articulation. Of course, during the section, care must be taken to avoid injury of the pelvic fascia or viscera with the saw. The obturator muscle is to be cautiously cleared away, so as to expose the outer surface of the pelvic fascia, its external or obturator layer, and the pudic vessels. When these have been examined, the obturator fascia is to be divided from its attachment to the great sciatic ligament, and reflected upwards when the fibres of the levator ani will be seen. The anterior and posterior fibres of this muscle should be left undisturbed, and the middle portion divided and reflected downwards, or altogether removed. We have thus a view of the exact relations of the levator ani and the pelvic surface of the posterior layer of the triangular ligament, which is now seen to be reflected upwards on the prostate on the inner side of the levator ani, and to be a portion of the pelvic fascia stretched from the ramus of the pubis across to the side of the apex of the prostate, and lying between the levator ani and the perineum. When the dissection has been carried to this point, before proceeding to examine the internal or visceral layer of the pelvic fascia, and the relative anatomy of the pelvic viscera, we should return to the perineum, and by cautiously detaching the

anterior layer of the triangular ligament from its external connections, where it is continuous with a fibrous texture covering the crus penis, reflecting it towards the mesial line, and then by drawing it and the bulb slightly forwards, so as to render the part tense, we bring into view the muscular fibres of the compressor urethræ in front of, or on the perineal aspect of the posterior layer of the triangular ligament; whilst, looking from the interior of the pelvis, we see the anterior fibres of the levator ani lying on the pelvic surface of the posterior layer of that ligament, and bounded on their inner or mesial surface by the reflexion of that layer upwards on the side of the prostate; so that there can be no possibility of confounding the fibres of the one muscle with those of the other.

Returning to the compressor urethræ, our own dissections have shown us that it consists of fibres arising from the lower part of the pubic symphysis, which descend not quite perpendicularly, but with a very slight degree of obliquity, or rather curved direction from without, inwards and forwards. Other fibres from the pubic ramus pass from without inwards with a greater or less degree of obliquity, the most superior being more oblique, while the lowest are nearly transverse, or have even a slight obliquity from without, upwards and inwards; the whole pass forwards and inwards to join the fibres from the opposite side, beneath the membranous portion of the urethra. If the fibres which arise from the pubic ramus be detached from their lateral connection, they fall away towards the centre, and then we have exactly the sling-like bundle described by Mr Wilson, and we see the source of error in his description to depend on his mode of lateral dissection. We are satisfied of this, also, by examining his dissection which is preserved in the Bell Collection of the Royal College of Surgeons here, where the cut ends of the fibres detached from their lateral connections can be seen distinctly, as they are magnified by the fluid and the circular jar in which they are preserved.

In reviewing the functions of some of the voluntary muscles, our author suggests some very ingenious theories as to the functions of different portions of the levator ani, which are worthy of careful perusal and study, although we cannot comprehend how the anterior portion of the levator can have a sphincteric action on the urethra, placed as it is in the angle of reflexion of the pelvic fascia, and having between it and the urethra the whole thickness of the prostate. The sphincteric action occurs, we believe, at the neck of the bladder itself, and the explanation must be looked for either in the arrangement of the muscular fibres there described by Monro, or in the peculiar ring of contractile tissue described by Sabatier, Bichat, Cloquet, and others. Whatever difference of opinion may exist in reference to the exact arrangement of the muscular fibres, we think there can be little doubt that all anatomists and practical surgeons will admit the effect of muscular action on the calibre of the

urethra, especially at the membranous portion, and also the spasmodic action occasionally met with at the neck of the bladder. In treating of the erectile vascular tissue surrounding the urethra, Mr T. points out a very good plan of obtaining a relative view of the depth and position of the bulb; and in describing the general arrangement of the erectile tissue of the spongy portion, he draws attention to a fact often overlooked—viz., that a thin layer of the vascular erectile tissue is prolonged backwards beneath the mucous lining of the membranous portion. He also specially addresses himself to the settlement of the question as to the existence of a septum of the bulb, about which there has been so much unnecessary controversy of late in reference to hemorrhage. He brings the result of several special dissections to confirm his views, and illustrates the descriptions by drawings of transverse sections of the spongy portion at different points. As we think his researches are conclusive, and that his remarks place the matter in its true light, we quote them. After stating Mr Ellis's description of the fibrous septum passing inwards and dividing the bulb into two lobes, and the opposite statement of Mr Lizars, who denies its existence altogether, he proceeds to state the results of his own investigations on fourteen subjects:—

“On the other hand, its existence has been wholly denied. I have accordingly embraced several opportunities of making transverse sections of the bulb, and can most unhesitatingly confirm the statement that a partition exists, and may sometimes be traced forwards to within two or three inches of the external meatus. It is distinctly seen, in some instances, to be composed of two layers with a faint dark line between them, indicating that the coherence of two bodies in the middle line, to form a single corpus spongiosum, is the typical formation, traces of which were present in all the subjects examined to a greater or less extent. The relation of the partition to the fibrous covering of the bulb is, however, not accurately described above; ‘a portion of the latter does’ *not* ‘project inwards in the middle line,’ etc. The partition never appears to be thus connected with the fibrous covering, but is chiefly developed in the interior of the bulb, immediately beneath and closely attached to the urethra, from which point it becomes less marked as it approaches to the circumference, with which I have never seen it to be entirely continuous. Moreover, the posterior part of the bulb receives many more fibrous partitions or prolongations into its substance than any other part of the spongy body.

“It would appear, then, that the relation of structure to the question of hemorrhage stands nearly thus:—

“That the entry of the arterial branch of supply at about a half or three-quarters of an inch before the posterior extremity of the corpus spongiosum, renders incisions at this point liable to become the cause of considerable hemorrhage. That the existence of several fibrous partitions in the part posterior to the entrance of the artery, and especially one in the middle line, may tend to render incisions into that part of the bulb so defended, less productive of hemorrhage than in parts where these do not exist.

“But when the difficulty, it may be said impossibility, of hitting the exact line of this slight partition, as may be proved on the dead body, is taken into consideration, it cannot be seriously argued, I conceive, that the prevention of hemorrhage depends upon the accomplishment of so delicate an operation. No doubt but the median line in sections of the bulb is the line of safety; and why? Because a short branch of the pudic enters it *on each side*, close to which, if an incision be made, the artery might almost as well itself be opened.

But if the section lie equidistant, or nearly so, from the two vessels, the minute meshes of erectile tissue intervening between the section and the artery, entangle within themselves the coagula which are formed, become choked or blocked up, and so conduce most readily to the checking of hemorrhage, more especially if this be favoured by external cold applications."—Pp. 39, 40.

He might have added, in regard to incisions of this part in stricture, that in the cases of firm resilient stricture, generally the subjects of operations, the texture is so altered by consolidation, that its erectile vascular texture is effaced at the part requiring division, and therefore not likely to give rise to hemorrhage. There is one point in the anatomy of the perineum connected with the vascular texture which our author has omitted specially to speak of—we mean the large venous sinuses lying between the layers of the deep fascia returning the blood from the bulb and other deep textures to the great prostatic plexus of veins. A knowledge of this vascular texture seems to us important, not so much as regards hemorrhage, as in some measure explaining the occurrence of phlebitis and pyemia occasionally following operations or injuries at this part. The anatomical section of the work closes with a very excellent summary of the principal facts noticed, pointing out their practical bearings as applied to the diagnosis and treatment of stricture.

The next section of the work is devoted to the classification and pathology of stricture. After defining stricture to be an abnormal contraction of some portion of the urethral canal, and stating his reasons for preferring this more usually received definition to that of Sir Charles Bell, who, considering the urethra in a quiescent state to be a closed canal, defined stricture to be that condition in which it had lost its power of dilating, our author proceeds to range all strictures under two great divisions, Permanent and Transitory. In the former, he ranges all contractions due to organic deposit in or about the urethra, and leading to alteration of structure and permanent contraction of its calibre. Under the head of transitory are included cases where either local vascular congestion, or inflammation, cause a temporary contraction of the canal, constituting Inflammatory stricture, or where the contraction is caused by some unwonted muscular action, Spasmodic stricture. Mr T. makes some very judicious remarks as to the difficulty of any exact classification, as all these conditions may co-exist, as, for example, in cases of organic stricture, where the calibre of the canal may be temporarily still further contracted from the supervention of vascular excitement and spasm, depending on some temporary source of irritation. Passing on to his description of his different forms of organic stricture, we find him thus speaking of the bridle stricture:—

"It is not uncommon to meet with folds of the mucous membrane obstructing the passage at one of its sides only; this may occur above, below, or on either side, forming a crescentic septum, obstructing a segment of the canal. Similar folds occasionally run obliquely, instead of directly across it, and two or three such sometimes appear to have coalesced, making the membrane irregular and uneven.

"Such as these constitute the 'bridle stricture' of Sir Charles Bell, a term alluded to here because it has become a rather popular one with writers on stricture, by some of whom it has been supposed to refer exclusively to those rare instances in which a free thread or band of lymph runs across the urethra from one side to the other, as if adhesion had occurred to a limited extent in the centre of the passage, which had afterwards gradually suffered extension, a construction which the original description by Sir C. Bell can by no means be made to bear."—P. 56.

There is no doubt that this description corresponds to that given by Sir Charles Bell, and we believe that those who have understood it as a line stretched across the urethra, have probably been misled by looking at Fig. 5 in Plate v. of Sir Charles' "Engravings from Specimens of Morbid Parts," where the stricture is dissected from the wall of the urethra, and a bristle placed in the canal between the stricture and the opposite wall, giving it the appearance in question. We cannot but think, however, that the term bridle stricture would be better limited to the free thread or band of lymph, whilst the lateral or oblique membranous projections would be better described by the term valvular, for, though the projection is often slight, it very generally presents, more or less, the appearance of a valvular fold. As to the rarity of free bands of lymph, although not common, we do not think they are so very rare as our author's expression would lead us to suppose. We have seen several specimens, and we have one preparation in our possession in which there is a very distinct free band crossing the canal near the bulb, and also one of the valvular constrictions placed laterally.

The pathology of the different forms of stricture is treated of in detail with great clearness. This part of the work contains much useful information, and we would direct attention to the investigation of the essential elements of stricture, and the structures in which the constriction is situated, as well as to the remarks on croupal exudation as a cause of obstruction of the urethra, as being especially interesting and instructive. In reference to that *questio vexata*, impermeability, our author, whilst he admits that when the canal is narrowed to a mere pin-hole, it may be completely obstructed and rendered impervious from any slight tumefaction or even a little hardened mucus, is of opinion that the canal never adheres or is completely obliterated by disease, except in cases where fistula in perineo exists, and then very rarely, and states, he has once, and once only, met with such a case. Now, whilst we believe that in practice it is well to dismiss the idea of impermeable strictures, as influencing unfavourably our perseverance, and consequently our chance of success in treatment, yet, on the other hand, the question must be fairly looked at in other points of view. 1st, Do we never see the canal so contracted at some points as not to be capable of admitting a No. 1 or even $\frac{1}{2}$ catheter along the contracted passage? 2dly. Do we never find the natural passage completely occluded

from disease, the urine passing by another route from behind the stricture into the urethra in front of it? The former condition we presume few will deny to exist; of the latter we have in our own possession a specimen where the natural canal is fairly obliterated for about 2 lines a little in front of the bulb, where, from the dilated membranous portion, a small ulcerated opening leads into a new route above the natural canal, and again opens into the urethra in front of the stricture. Abscess in perineo and fistula did occur in this case a few weeks before death (which took place from enormous abscess of the liver), but the obliteration of the urethra evidently had existed for years from its firm consolidated structure; and indeed from our knowledge of the history of the case, we have no doubt that the natural canal must have been impermeable for at least two years. In another preparation also in our possession there is a tight stricture about 3 lines in length in the posterior part of the spongy portion, just wide enough to admit a bristle. On either side of it is a false passage made with a catheter about the size of a No. 2; that on the left side, commencing in front of the stricture, re-enters the natural canal immediately behind it, and leads on to the bladder, and we suspect tight strictures are not unfrequently permeated and the retention of urine relieved in a similar fashion. Such facts show that in one sense the impermeable stricture may be met with, but the cases are avowedly rare; and we repeat that it is well for practical surgeons to dismiss the idea of impermeability as tending to cramp their efforts, and to lead to dangerous operations. The section terminates with an important discussion on the question of the locality of stricture. If any proof were necessary of the value of the preliminary anatomical inquiry, it would be found in contrasting the discrepant statements even of the most eminent and experienced surgeons, who have stated their opinions as to the site of contraction founded on mere general impressions, derived from treating stricture on the living with the clear manner in which the previous exact anatomical description of the urethra enables our author to map it out into regions, and give a simple and definite classification of the parts of the canal most usually affected by stricture. The results of his investigations are as follows:—

“In examining the museums named, I have personally submitted to a close and careful inspection not less than three hundred preparations of stricture of the urethra, of which I possess notes made on the spot of two hundred and seventy, the rest being examples which, from decay or other circumstances, it was impossible correctly to classify.

“These examples may all be comprehended by the three following classes:

“I. STRICTURES OCCURRING AT THE SUB-PUBIC CURVATURE, *i.e.*, at the junction between the spongy and membranous portions and its neighbourhood; the latter term being understood to comprise an inch of the canal before, and three-quarters of an inch behind that point, thus including the whole of the membranous portion.

“The junction itself is the point at which stricture is most frequently situated. Next is the extreme anterior boundary of the division, a spot which is one inch

in front of the preceding, and almost as frequently affected; while, between these two points, six examples of stricture are met with for every one behind the junction, in which latter situation therefore they are very uncommon. Most rarely is a stricture found so far back as the posterior part of the membranous portion.

“II. STRICTURES OCCUPYING THE CENTRE OF THE SPONGY PORTION, *i. e.*, a region extending from the anterior limit of the preceding, to within two inches and a half of the external meatus, and measuring therefore about two and a half to three inches in length.

“III. STRICTURES OCCURRING AT THE EXTERNAL ORIFICE, AND WITHIN A DISTANCE OF TWO INCHES AND A HALF OF IT.

“The following is an analysis of the 270 preparations referred to; they exhibit 320 distinct strictures:—

“Total number of strictures 320.

„	in Region I.	. . .	215	or	67	per cent.	on the entire number.
„	„	II.	. . .	51	„	16	„
„	„	III.	. . .	54	„	17	„

320

“Of these—

“There were 185 examples of *one stricture only*, situated in Region I.

„	17	„	„	„	Region II.
„	24	„	„	„	Region III.

“There were 8 cases in which the urethra was strictured in all three Regions.

„	10	„	„	„	in Region I. & II. only.
„	10	„	„	„	in Region I. & III. only.
„	13	„	„	„	in Region II. & III. only.

“Lastly, I may confidently assert that there is not a single case of stricture in the prostatic portion of the urethra, to be found in any one of the public museums of London, Edinburgh, or Paris.”—P. 87.

The chapters on the causes and pathology of permanent organic stricture, and that on the pathology of transient stricture, contain much valuable and instructive matter; but our limits warn us to be brief, and therefore we content ourselves with directing our readers to that part of the work itself, and we pass on to the diagnosis and treatment of stricture.

(*To be continued.*)

The Diseases of the Heart and the Aorta. By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, etc. Dublin. 8vo. 1854. Pp. 689.

(*Continued from page 269.*)

Dr Stokes next,—and we follow the subject as he has laid it down,—relates the physical signs of pericarditis, and shows how varied is the nature of the friction sounds, and the physical constitution of the inflammatory products which produce them. As a rule, he says, these friction sounds are “singularly localised, and not heard be-

yond the region of the heart ;” but he does not deny that they may be extensively heard over the thorax. He seems, however, to lay more stress upon this fact of the limitation of the sounds, than authors for the most part do ; some estimable writers do not even refer to it. We have certainly seen an erroneous diagnosis made, in consequence of the observer’s faith in this rule ; of its exact value we feel a difficulty in speaking. We remember in a case of pericarditis to have heard a loud double bruit reaching up to the very top of the sternum, which we certainly put down as endocardial, but which proved to be pericardial eventually, and we alone were not deceived.

The friction sounds are remarkably modified by local antiphlogistic remedies, and by pressure : pressure will at any time produce an increase of the sound, and is therefore useful as a differential means of diagnosis between endo- and exo-cardial murmurs. Dr Sibson first pointed out this fact. *As a general rule*, there does not appear to be much difficulty in distinguishing between these classes of murmurs ; but we must certainly admit the proposition laid down by Skoda, that there is no endocardial, excepting the whistling, which *may* not be imitated by an exocardial murmur. In the case above referred to, we believe that the finest ear would have found it impossible to decide, from the nature of the murmur, as to its character ; certainly we who judged it endocardial were in error. Dr Stokes himself admits the difficulty in specific cases. As for the exocardial being a more superficial and extended sound, such distinction is very dangerous, for a distant endocardial bruit may be louder than an immediate exocardial ; and how, as has been asked, by such rule shall we judge of friction sounds arising from the back of the pericardium ? Skoda states that the only aid he knows of to assist in the diagnosis is derived from this fact, viz., that endocardial murmurs correspond to the rhythm and natural sounds of the heart, and that exocardial murmurs seem to *follow* upon its movements.

Some writers have stated that the friction sound disappears when the dull percussion sound indicates much effusion ; but this is certainly far from being invariably the case, perhaps it is not the general case, where friction sound had been heard from the first. The sound may be absent when the dulness is great, in certain cases, solely because the effusion is serous, or purulent, and not plastic. It must also not be forgotten that the force of the heart’s action is concerned in the production of the murmur ; if this be weak, the murmur may be slight, though the effusion be very scanty, and *vice versa*.

Dr Stokes has collected some interesting facts respecting the modification of the friction sound by the presence of air in the pericardium—pneumo-cardium. This condition must be exceedingly rare ; Rokitansky never appears to have seen such a fact. The sounds produced are said to be crackling, gurgling, and metallic. Its rareness renders this condition almost matter of curiosity only. Some of the cases related by authors are of very doubtful character, the

presence of the air being conjectural. We have ourselves, however, witnessed one case; here air escaped from the stomach, through an ulceration in the lower part of the œsophagus, and through the back of the pericardium into its cavity; unfortunately the precordial signs were not observed during life.

Dr Stokes seems to consider that the difference of a friction sound produced by the heart's action within the pericardium and external to it may be diagnosed, but this really seems very doubtful. A most careful observer remarks: "the murmur arising external to the pericardium exactly resembles the murmur arising within it." Indeed, as the time of the friction sound within the pericardium must be co-incident with that of the sound without, it seems impossible to distinguish between them merely by this friction sound.

In summing up, Dr S. tells us "that we must depend for the accuracy of our diagnosis on the actual acoustic character of the signs,—on their diffusion or concentration at a point of greatest intensity,—on their being superficial or deep-seated,—on their amount of extension over the thorax,—their double or single character,—their transmission or non-transmission along the course of the vessels,—on the presence and character of the tactile signs,—on their constancy or variability in character and seat,—and on the effect of treatment in their modification." From what we have already said, we must leave our readers to draw their own conclusions as to the actual value of any of these particular signs. As the rough and general rule, we would venture to say that the double friction sound, in nine out of ten cases, leaves us little difficulty in the diagnosis; the exceptional case will probably not misguide our treatment, though its exact nature may elude our certain grasp.

In speaking of pericardial effusions, and the dull percussion thence resulting, Dr S. does not point out the modifications produced in the sound by the position of the lungs, an important fact in judging of the quantity of fluid. When the lungs are pressed forwards over the heart, a large amount of effusion may produce no alteration in the percussion; and when the contrary holds, a small amount is sufficient to cause a change. Neither does Dr S., in showing how to distinguish this dullness from that caused by pleural effusions, remark the particular form which the distended sac assumes.

We will not follow our author into his account of the vital symptoms and history of pericarditis. There is in it much valuable matter, but perhaps more attention than necessary has been paid to the *rare* and curious symptoms. He shows how varied the symptoms of pericarditis are, and how little value any particular one possesses. Of the pulse he writes down *nine* different conditions occurring in this disease. His statements, however, respecting the connection of rheumatism with pericarditis, we must note: The combination of pericarditis with acute articular rheumatism is common, yet the disease of the heart is more closely related to the rheumatic fever than to the inflammation of the joints. The liability to pericarditis is in

direct proportion to the violence and duration of the fever. In apyrexial cases of acute arthritis, the liability to cardiac inflammation is but slight. Pericarditis may arise at any period of the disease, and even precede the arthritis, and every variety and degree of pericarditis occur in connection with acute rheumatism.

There is nothing to delay us in Dr S.'s account of *endocarditis*. Its idiopathic form is unknown to him; he only knows it as associated with other diseases. *Chronic* diseases of the valves do not belong here,—they are not of necessity associated with it. Its diagnosis is obscure; the only means we have of discovering its presence is from the production of a valvular lesion; but this means is not infallible. He does not believe that endocarditis more frequently exists without pericarditis than pericarditis without endocarditis, as Dr Hope states; its symptoms resemble those of pericarditis, and its association with rheumatic fever is less marked than that of pericarditis.

Chapter 2 is occupied with diseases of the valves of the heart. The key to the pathology of the heart is to be found in the vital and anatomical conditions of its muscular structure. No matter what the affection, its symptoms mainly depend on the strength or weakness, the irritability or paralysis, the health or disease of the cardiac muscles. The nature of a valvular lesion, and its localization, is of comparatively trifling importance; the grand practical points to be determined are: is the murmur heard organic? what is the condition of the muscular fibre of the heart? The cases where it is desirable to fix the exact seat of the disease are few, for our treatment must be guided by the condition of the *muscle*. In prognosis the question may be of more importance, in this respect, that there is a greater probability of sudden death, for instance, in mitral than in aortic valvular disease.

Discussion of the causes of the heart's sounds, our author wisely refrains from. He says, with truth, that observers have taken too limited a view of their possible causes, and shows that no less than twenty-two operations are associated with these sounds—eleven for each side of the heart. Certainly pathology justifies us in saying this, that though some parts of the heart take a leading share in the production of the sounds, that these cannot be ascribed to any single cause. At present we must be content to know, for practical purposes, that the first sound and the systole, the second sound and the diastole, are respectively coincident. Wisely, also, does Dr S. warn him, who thinks to find disease at the bed-side, as he finds it described in his books. Rules of diagnosis are too often founded on the supposition of the isolation of diseases, but its combinations vary infinitely, and *these* must the physician's view embrace. Compared with *these*, the seat and nature of the murmur is as nought. "He will examine into the vital and mechanical state of the heart's cavities. He will ascertain the amount of vigour of the heart," etc.; the duration of the disease, and its origin; and what effects have been produced by the disease upon the brain, lungs, or liver. Dr

S. agrees with M. Forget, that we have only probability to guide us in determining whether a murmur exists on the right or left side of the heart. It is not safe, by referring to the points of the greatest intensity of the murmur, to attempt to fix its seat. This doctrine he has taught many years.

Absence of murmur is no indication of absence of valvular disease, just as the presence of murmur, *per se*, is no sure sign of its existence. Mr O'Ferrall, in well observed cases, has marked the disappearance of the murmur, with progression of the valvular disease. Of this fact, and of its importance in practice, there can be no doubt; in the last stages of heart diseases, when the general symptoms are well marked, and the heart's beats tumultuous, irregular, and feeble, and the malady of long standing, we have often had occasion to verify its truth.

But our author speaks of a *latency* of valvular disease. This is a slow and disorganizing process, which for years may reveal its effects neither to the patient nor to his physician; producing neither signs nor symptoms of heart disease, and yet, as it were suddenly, in the course of a few days, manifest signs of valvular disease will appear. But this is no acute disease, and disastrous often are the consequences to the patient, when it is considered and treated as such, which unhappily it too often is. On this fact he lays great stress. Many interesting cases illustrative of the compatibility of an active existence with a highly advanced condition of valvular disease are also recorded by him. Nobody can dissent from the just remark of our author—that in our practical dealings with valvular murmurs, we should consider rather the effects of the disease than the disease itself; it is certain that a vigorous and active life is not incompatible with the existence of certain valvular murmurs, and that these murmurs may endure for years without producing any general symptoms; but we are not quite sure whether he does give their *real* value to these abnormal sounds. We speak with respect and caution when we speak in the presence of Dr Stokes, but we feel bound to say that our own experience has led us to a feeling of greater certainty in diagnosis both of the seat of the murmur and of its nature, than Dr S.'s appears to have led him, and also, we may add, to more positiveness in diagnosis. Our own belief is, that the cases are very few indeed in which, if sufficient opportunity for examination be given, the physician can err in distinguishing an organic from a functional murmur. We might almost say that Dr S.'s caution in this matter has led him into the opposite extreme. Two cases are related at p. 151, at which, in our opinion, the judgment of the physician as to the organic or functional disease of the heart ought not to have halted. In both the "*physical* signs were unquestionably those of organic disease of the mitral valves;" in both, the general symptoms appear to have been those indicative of cardiac disease. Why then does our author hesitate? Because, it would seem, the general *anæmic condition and the age* of the

patients (female), induced him to ascribe the signs and symptoms to anæmia. Now, we feel tempted to ask this question, has any one ever observed a *persistent* mitral murmur associated merely with an anæmic condition of the body? The thing may be, but we can scarcely hesitate in saying that it must be most rare; hardly in a practical sense, worthy consideration, when all the marked and general symptoms of heart affection co-exist. (There is one auscultatory sign to which we would here allude, as we can scarcely doubt that it would in these difficult cases have been found of every valve.) Our author elsewhere alludes to this sign—increase of the second sound of the pulmonary artery—but it is only for the purpose of rejecting it from consideration. Our experience has given us a very different idea of its value, and we must therefore dwell for a moment upon this point, for it certainly is true that although this increased second sound of the pulmonary artery in certain diseased conditions of the heart, has long been pointed out, and adopted in their text-books as a regularly acknowledged auscultatory sign by Continental writers, it is still but little regarded in this country, and is, indeed, apparently unknown to the majority of observers.

In the second intercostal space, says Hamernjk, about a finger's breadth from the right of the sternum, two sounds, a tic-tac, are audible, and two like sounds at a similar point to the left of the sternum; the first set belong to the ascending aorta; the second, to the pulmonary artery. As a rule, it may be set down, that the sound which corresponds with the heart's diastole, and is heard over its apex, and in the arteries of the neck, is produced by the semilunar valves of the aorta, and that which is heard over the right ventricle, by the pulmonary artery's valves. In healthy persons, these sounds, in respect of strength, pitch, timbre, and duration, are, for the most part, exactly alike, and thus, indeed, they seem to represent but one sound; even in health, however, they present variations in these particulars; thus the pitch, duration, etc., of the pulmonary artery's second sound is sometimes found increased, and often divided into two or three parts. Under the influence of certain diseases, however, a very marked difference is observable. Whatever produces increased tension of the pulmonary artery, produces increase of its second sound, and it may be, its division also into two or three intervals, whether hypertrophy of the right ventricle, obstruction of the mitral orifice, or deficiency of the mitral valves, etc.; at the same time the first sound becomes less clearly defined.

A consideration of these facts at once brings us to this position; that a distinct increase of the second sound of the pulmonary artery, heard in conjunction with a mitral murmur, is almost the certain indication of deficiency or contraction of the mitral valves. One German writer, indeed, of whose name we have heard a good deal lately, goes so far as to say, that we are not to conclude from the

mere fact of a murmur being heard in the left ventricle during the heart's systole, that the mitral valves are defective, unless we at the same time ascertain that there is an increase in the intensity of this second sound of the pulmonary artery.

From our own experience we are not prepared to say that this increased second sound is invariably to be found, corroborative of the existence of disease of the mitral valves, but that it is so in the majority of cases we do not doubt; neither can we doubt but that in the cases mentioned by Dr Stokes, and whose notice has led us into these particulars, the sign would have been found a *pierre de touche*.

Another fact respecting the pulmonary artery, and stated by Hamernjk, is worthy of note. After observing that the vibrations produced by the flapping together of the valves of the pulmonary artery, may be occasionally felt in the second left intercostal space, and close to the sternum, when communicated through lung tissue consolidated by effusions of tubercular, pneumonic, or cavernous matters, he goes on to tell us, "but when the beat of the artery and the flapping of the valves are felt in any other intercostal space, we may be sure that the position and size of the heart, and the condition of the pulmonary artery are abnormal. When the pulmonary artery through any cause, *ex. gr.*, deficiency or contraction of the mitral orifice, etc., has become abnormally increased in length and breadth, it comes in contact with the thorax, and its pulsations are perceptible, but never in the second intercostal space; they are generally to be felt in the third intercostal space, and two or three finger's breadth from the sternum, and even as far as the nipple, and invariably indicate a deep position of the heart.

Our author's practical method of reckoning up the value of cardiac murmurs, as indicators of treatment is excellent, and well worthy attentive perusal. In speaking of diseases of the valve at the right side of the heart, Dr Stokes says, "that clinical medicine does not justify a diagnosis of the disease of the tricuspid or the pulmonary valves." Surely he speaks here too absolutely; a systolic murmur heard loudest over the lower part of the sternum, *i.e.*, over the right ventricle, and associated with swelling and *pulsation* of the cervical veins, would certainly justify us in considering the tricuspid valves defective, especially if no distinct murmur were at the same time audible over the left ventricle and along the aorta. Neither can we admit this dictum:—"If there be a mitral murmur, we lose the sound of the tricuspid valves, and if an aortic, that of the valves of the pulmonary artery." Indeed we must totally dissent from such a proposition, for we believe the cases are very rare where murmurs, either of the mitral or aortic valves, are so loud as to overwhelm the sounds of the tricuspid or pulmonary artery, provided these sounds be sought for at particular spots, *i.e.*, about the lower half of the sternum for the former, and the second left inter-

costal space for the latter. Dr Stokes cannot be doing justice to our present powers of diagnosis when he says, "We have seen how doubtful all diagnostics drawn from the situation and tone of the murmur must be; and hence the element of probability on the one hand, and the association of symptoms and signs on the other, must be our chief guides in determining the seat of valvular diseases."

We consider, also, that he has much exaggerated the difficulty of distinguishing, in certain cases, between the first and second sounds. It is very true that errors are frequently being committed in this matter, but we will venture to say that in the vast majority of instances such errors would be readily avoided, if the observer would *feel* the impulse while he is auscultating the sounds of the heart—an important aid to which Dr S. does not allude—and if he will only remember that the murmur or sound which *precedes* the impulse *must* be diastolic, his chance of error will be but small. It is only when the action of the heart is excessively rapid, and its impulse imperceptible, that any real difficulty can exist.

Mitral valvular disease possesses, our author tells us, no general symptoms by which we may determine its nature; and the difficulties of its physical diagnosis have not been fully stated by writers. "In the ordinary cases of mitral murmur, we cannot say whether the murmur is constrictive or regurgitant," or both together. Cases where the diagnosis may be made with a great degree of certainty, are more often pictured in books than met with at the bedside. We need hardly say, from what has gone before, that these are not our conclusions. We cannot for a moment admit that this subject of diagnosis is in the obscure and hesitating condition to which it has been reduced by our author. It is perhaps true, that in the majority of cases, it matters little what the nature of the murmur may be (provided we have clearly established its origin in actual valvular disease, and its chronic character), as regards the treatment we may be called upon to adopt, for this being of necessity of a palliative character, has more relation to the complications of disorders produced in other organs by these derangements of the heart's structure than to the organic derangements themselves; but whilst so much obscurity hangs over these cardiac diseases, in every point of view under which we regard them, surely it is unphilosophic to reject minute observation merely because it is difficult, and apparently of no immediate avail. Our diagnosis cannot be too particular, so long as it is well-founded, *and has positive facts for its basis*; and we cannot, therefore, believe that the daily experience of observers in general will lead them to agree in this dictum of Dr Stokes: "That the number of cases in which we are warranted in making a special diagnosis of valvular disease is small." Certainly, in the majority of fatal cases which come under our observation, we should say the reverse of this; the diagnosis is made with tolerable security, and its correctness confirmed after death. Again, if, as is admitted, certain valvular diseases are more rapidly destructive of life

than others; special diagnosis must, at all events, be of no little importance in the matter of prognosis. Take the case of a constricted mitral orifice, and of a simple defect of the mitral valves; a diastolic mitral murmur is of more serious import than a systolic mitral, and our opinion of its results must be therefore more unfavourable, for it indicates very serious disease of the valvular structure; a constricted mitral orifice presents a *direct* impediment to the flow of the blood from the lungs, telling forcibly upon the function of these organs, and almost invariably permits regurgitation of the blood in addition. On the other hand, we continually observe instances of systolic mitral murmurs, in which little or no injury seems to be thereby inflicted either on the circulation or the respiration. But Dr Stokes would have us believe that there are no distinctive symptoms of disease of the mitral valves, and that it is almost impossible to decide, in the majority of cases, whether a murmur is associated with the systole or the diastole of the heart.

Our space forbids us following our author into the wide field of observation opened by him concerning the diseases of the valves of the heart. The student may therein gather rich stores of original matter; but he will have to *think* as he passes along, for the work is no text-book, wherein learning is made easy. The study is a difficult one, and our author has not concealed its difficulties. In his recapitulation, he lays down no less than seventy propositions which have a practical importance with reference to valvular disease, and which he believes are the fair deductions to be drawn from the data he has produced. From his particular views respecting the diagnosis of valvular disease, we have expressed our dissent; but his philosophic examination of the consequences and antecedents of these diseases, of their general relations, their wide influence on the economy, their treatment, their value as indicators of treatment, and as producers of secondary lesions, etc., receives our unqualified thanks. The student may herein find a guide, and the physician who has already gained a mastery in these things, will have the means of trying his knowledge, and testing his opinions.

In his *third chapter*, Dr Stokes deals with the diseases of the muscular structure of the heart; and, first, with its dilatation. Simple uncomplicated dilatation he has never observed. Much obscurity still hangs over the history of this disease, and Dr S. states that, in the present state of our knowledge, it is impossible to explain its phenomena. Its leading characteristics are a weak condition of the heart, associated with pulmonary and hepatic affections. The renal secretion is often interrupted, and a gouty diathesis manifest; but in what relation these disorders stand to each other as cause or concomitants, or as effects, or the expressions of some general disease, yet remains to be shown. Organic disease of the valves here is rare; the two heart's sounds become very similar, and are, like the heart's action, difficult of analysis. The attacks of the disease are paroxysmal, and progressive in intensity. Death gene-

rally ensues from congestion of the lungs. We would just remark, that this state of things appears very similar to, if not identical with, that resulting manifestly from bronchitis and emphysema of the lungs; and we cannot but think that, at all events, in the very vast majority of cases, simple dilatation of the heart (and this is generally of its right side) may be traced to pulmonary disease. Congestions of the liver, kidneys, and abdominal organs follow, almost of necessity, upon obstruction to the blood's passage through the right ventricle. Dr S. would have a gouty or cachectic state of body to be the most common agent in producing these conditions.

In speaking of dilatation of the auricles, Dr S. suggests, that when it is extreme it may be attended by a diastolic impulse; and relates a case (p. 274) which seems to back his opinion. Bouillaud relates a similar one. Of the fact of a diastolic impulse being occasionally felt towards the base of the heart there can be no doubt; but its manner of production is by no means clear. There is, however, some obscurity about Dr S.'s case; inasmuch as the diastolic pulsation was synchronous with the first sound of the heart, we must infer that the word diastolic here relates to the auricle, not to the ventricle; and if so, we think an explanation of the phenomenon may be found in the supposition, that at each systole of the right ventricle blood escaped back through the tricuspid valves into the enormously distended right auricle, and so gave rise to the impulse felt at the right of the sternum.

In this chapter Dr S. gives a very interesting notice of an obscure and rare affection, viz., increased action of the heart and arteries of the neck, followed by enlargement of the thyroid gland and eyeballs. We have had no experience of this disease, and will therefore merely call the attention of our readers to the account here given of it. It appears that in one case the disease was mistaken for carotid aneurism, and the ligature of the artery decided upon; but happily the mistake was discovered in time.

On the subject of fatty degeneration of the heart, there does not appear anything which need arrest our attention. Dr Stokes' experience confirms the views of those who have of late brought this affection more particularly before the notice of the profession.

(To be continued.)

Transactions of the Pathological Society of London, including the Report of the Proceedings for the Session 1852-53. London. 8vo. Pp. 286. 1853.

THIS Report of the Transactions of the Pathological Society of London is highly creditable to the zeal and activity of its members. It records their labours during the last session, and embraces about

120 observations, illustrative of a great variety of morbid changes. A detailed review is, of course, out of the question, but we propose to direct the attention of our readers to a few of the most interesting facts contained in these reports.

In this volume, as in its predecessors, the descriptions of specimens are arranged according to the system of organs affected. Cases in which the nervous system was diseased are first detailed. This section embraces fifteen observations, and comprehends cases of apoplexy, softening, cancer, and tubercle of the central nervous masses, as well as cases of diseases of the bones.

A singular case is recorded by Dr Ogle (p. 27), in which a man fifty years of age swallowed a large portion of mutton bone, which became impacted in the œsophagus. Suppuration took place, and the foreign body was speedily discharged. He continued, however, to suffer from dysphagia and pain in the neck; when, about five weeks from the accident, he was suddenly seized with paralysis of the upper extremities. This extended to the lower limbs, and a few days before his death sensation also became impaired.

On examining the body, a portion of the œsophagus, one and a half inch in length, was found greatly constricted, and behind it was a large quantity of purulent matter. The cartilage between the fourth and fifth cervical vertebræ "was perforated by an ulcerated opening, which passed backwards into the spinal canal. The whole intervertebral cartilage was vascular, and so were the cartilages covering the opposed articulating processes of these vertebrae, and the ligamentous structure surrounding them was entirely destroyed. Opposite to the ulceration of the intervertebral cartilage, the dura mater and other membranes were very much thickened and adherent to each other, as well as to the bone and to the spinal cord in this neighbourhood. The dura mater for some distance was covered by recent soft fibrin. The spinal cord was generally somewhat softened, but for a short distance opposite to the fourth and fifth cervical vertebræ it was almost different, and this was the case also in the lower part of the dorsal region."

It is to be regretted that the morbid structure was not examined microscopically in this case, as we are left somewhat in doubt as to the nature of the softening. It is singular that the lower part of the dorsal portion of the cord was in the state above described, while that immediately above it was in a comparatively healthy condition.

Mr Hyde Salter reports the case of a man who had a deficiency of the middle portion and left hemisphere of the cerebellum. It is thought to be unique, but in one case described by Cruveilhier, even greater diminution of the cerebellar mass existed. It is curious that in this case of Mr Salter, a man aged forty-four, it is said that "he was frightfully passionate, and had long been addicted to masturbation." The same tendency was exhibited in the case of Cruveilhier, and has been supposed to be a powerful argument against the theory of Gall, the sexual appetite being well marked, and even excessive,

whilst the organ on which this instinct depends was greatly diminished in size.

The section on diseases of the organs of respiration is rather meagre, embracing only four cases.

The first case is recorded by Dr Bristowe, and is headed, "Co-existence in the Lungs of Cancer and Miliary Tubercle (?)" (p. 35). There was no doubt of the existence of cancer in the right lung, and cancer was also met with in the liver. The only question was, whether numerous "moderately firm, greyish-white, slightly translucent granules, about as large as a small pin's head," which were met with abundantly in the left lung and the upper part of the right, were tubercular or cancerous. Dr Bristowe comes to the conclusion that they were tubercular; and in this opinion we think that the description he has given of them quite bears him out. But it is to be observed that the tubercle was not making progress; for we are told that "each was surrounded by a patch of blackened, solidified lung structure." It is therefore nearly certain that the tubercular diathesis had ceased (only its effects being left) before the cancerous disease made its appearance.

The same subject is illustrated in another case, where there were cancerous tumours growing from the dura mater and from the periosteum of the spine and rib, as well as "several small masses of yellow, cheesy material, and a few grey miliary deposits, which had the appearance of tubercle," in the apex of the right lung (p. 7). There can be little doubt as to the cancerous nature of the deposit in the first-mentioned localities; but we are by no means satisfied that the deposit in the lungs consisted of tubercle. An elaborate microscopical report is appended, but we must say, that instead of clearing up the point, it only leaves it in greater obscurity. An interesting case of hydatids in the left lung is recorded by Dr Black of Chesterfield; but any account of it would occupy too much of our space.

Under the organs of the circulation, twenty-five observations are recorded.

Dr Hare describes (p. 81) a somewhat rare kind of malformation of the heart, in which the orifice of the pulmonary artery was completely closed, and where the only communication between the right and left sides of the heart was by the foramen ovale, which was open to a very limited extent (1-10th by 1-16th of an inch). The ductus arteriosus had a calibre equal to about the size of a crow-quill, and the pulmonary artery divided normally into its two divisions. The point of interest here is the very small size of the patent foramen ovale, as all the blood must have passed through it into the left auricle.

The other observations under this division include examples of diseases of the valves, of hypertrophy, and of fatty degeneration of the heart, of pericarditis, of aneurism of the great vessels, as well as several cases of malformation of the heart and large arteries.

Among the cases illustrative of disease of the digestive organs is a remarkable example of enlarged stomach, recorded by Dr Miller. This viscus occupied nearly the whole of the abdominal cavity, and was capable of holding ten and a half pints of fluid. The difficulty of forming a correct diagnosis in this case appears to have been very great. The only other lesion of the digestive organs to which we would direct attention, is the condition of the liver in two cases. The first is described at p. 6. The subject of it was a boy affected with caries of the dorsal vertebrae, and tubercle of the right lung and mesenteric glands. "The liver," we are told, "was much enlarged, extended across the abdomen, and weighed 5 lbs. 13½ oz.; it was a good specimen of the scrofulous liver." In the same case the spleen weighed 12½ ounces, and the kidneys are stated to have been "mottled." In the first place, we strongly object to the term "scrofulous," as applied to any condition of the liver. For although there is high authority for its employment, it involves a theory which we believe to be inconsistent with fact, as the same condition of the organ is undoubtedly met with in cases where there is no proof of the existence of the scrofulous diathesis. The liver in this case was, we have no doubt, in that condition to which the German morbid anatomists apply the term *speckig*, lardaceous, or bacony, and which many pathologists in this country denominate "waxy;" both of these terms being merely intended to characterise the external appearance. We believe that in the above case the spleen and kidneys were in all probability in the same condition, for the little that is stated regarding them would lead us to this conclusion, while it is well known that when one organ is in this state, the others are very frequently similarly affected. Nothing is said of the blood in this case, though the liver, spleen, and mesenteric glands were all enlarged.

The same condition of the liver existed, we have no doubt, in a case described by Dr Quain (p. 261). "The liver," it is said, "which was found to weigh six pounds, was paler than natural, but felt firm and solid. The increase in size seemed (when investigated by the microscope) to be due to hypertrophy of all the elements of which the organ is composed, rather than to a special increase in one. The gall-bladder contained about three drachms of this thin, dark coloured bile." "The spleen measured thirteen inches in length by eleven in width, and weighed five pounds. It felt exceedingly solid and firm, and was of a dark red colour." On examining the blood in this case, leucocythemia was discovered.

The only other class of cases to which we can allude, are some very interesting dissections of the ear, by Mr Toynbee. This is a branch of morbid anatomy to which but little attention has been paid, owing, no doubt, to the difficulty of conducting the examination. To obviate this as much as possible, Mr Toynbee gives some clear directions as to the manner in which dissections of the ear should be conducted, and by attention to these, a little practice

would, we doubt not, render the examination comparatively easy.

We are happy to state that a marked improvement is exhibited in the description of the morbid appearances and cases recorded in this volume. There is far less vagueness and expression of opinion, with more evidence of investigation and demonstration of fact. Histological research is evidently on the increase among the members of the society, and numerous figures indicate the importance now attached to this mode of inquiry. We confidently predict that, if the members continue to pursue their present method of exact and careful description, they must materially contribute to the progress of pathological science.

Handbook of Chemistry, Theoretical, Practical, and Technical. By F. A. ABEL, Professor of Chemistry at the Royal Academy, Woolwich; and C. L. BLOXHAM, formerly First Assistant at the Royal College of Chemistry; with a Preface by Dr HOFFMANN. 8vo. London. 1854.

MESSRS ABEL and BLOXHAM, two accomplished pupils of Professor Hoffmann's, have in this work sought to combine a text-book for the student in the lecture-room, a guide for the analyst in the laboratory, and a volume of general reference for the metallurgist, and other practical chemical artists.

Such a work was wanted, and we have satisfied ourselves that it has deservedly received the *imprimatur* of Dr Hoffmann, who commends it to the English student.

Although including in its title theoretical, as well as practical and technical chemistry, it is chiefly devoted to the two latter departments of the science, which are discussed fully and clearly; whilst the general principles of chemistry are treated with sufficient fulness for a work mainly practical in its aim.

As a whole, the treatise is an excellent one; but in one respect it may be improved. It contains a much larger number of foot-notes than is desirable in a hand-book, where only the prospect of a great advantage can justify the addition of detached passages to the bottom of a page, which distract the reader's attention, and disturb his continuous perusal of the text. Such a remark may seem hypercritical, and would be so were the notes strictly supplementary matter. But the great majority of Messrs Abel and Bloxham's notes have as much claim to be portions of the text as the sentences actually contained in it, and the text and the foot-notes equally suffer by their separation from each other.

But we do not wish to dwell upon a defect which can easily be remedied in a new edition, and has probably been occasioned by one of the two authors writing the text of the work, and the other annotating it.

The Microscope, in its Special Application to Vegetable Anatomy and Physiology. By Dr HERMANN SCHACHT. Translated by FREDERICK CURREY, Esq., M.A., with numerous Illustrations. London. Small 8vo. 1853. Pp. 131.

THIS is a very useful elementary treatise on vegetable histology. We notice it principally with the view of pointing out the nonsense of the following passage in the preface :—

“It has been thought advisable to omit the greater part of the description of foreign microscopes and auxiliary instruments contained in the original work. These details would, for obvious reasons, be uninteresting, if not useless, to the English reader. There is no doubt of the superiority of English instruments over those described by Dr Schacht; and the elaborate and able treatise of Professor Quekett affords all the necessary information upon the subject of English microscopes, etc.”

Now, this is treating Dr Schacht very scurvily; for surely he is better entitled to think his microscope a good one, after having succeeded with it in so thoroughly working out the delicate anatomy of plants, than Mr Currey is, who, so far as we are aware, has not investigated anything. In our opinion it is very inconsistent to translate the excellent works of foreign histologists, and at the same time condemn the utility of the instruments they use. We consider that the excellence of the work is the strongest proof of the goodness of the means employed. Without in any way wishing to undervalue the advantage of a good microscope, or the admirable improvements of the metropolitan opticians, we cannot forbear stating that their instruments are too large and clumsy, their price preposterously great, and altogether disproportioned to their assumed superiority. Further, it should be understood that good powers of observation, and sound scientific knowledge, are the real essentials for histological investigation. With these, foreign inexpensive microscopes are amply sufficient, and without them the best instruments are useless.

Comparative Anatomy. By C. TH. v. SIEBOLD and F. STANNIUS. Translated from the German and Edited by WALDO I. BURNETT, M.D. Vol. I. *Anatomy of the Invertebrata.* London: 1854. Pp. 470.

IN the English language at the present time we have no good standard treatise on comparative anatomy. It is true we have some excellent “*Outlines*” and “*Lectures*” (which, by the by, are very expensive), but, we repeat, we are greatly in want of a work which shall contain a well arranged and accurate digest of all the leading

facts which the combined labours of a host of observers in this department have of late years brought to light. The appearance of the above translation of Siebold and Stannius' "Lehrbuch der vergleichenden Anatomie" at once fills up the void, and confers a boon upon the English student, of which, we are satisfied, he will readily take advantage.

With the view of rendering the work complete, Dr Burnett has added many useful references, and a supplementary bibliography. We cordially recommend this volume to all interested in the science of comparative anatomy.

Part Third.

PERISCOPE.

PHYSIOLOGY.

PROFESSOR REMAK ON MULTIPOLAR GANGLION-CELLS.

By the publication, in 1846, of Stilling's discovery of the so-called *nerve-nuclei*, in the pons varolii of man and the vertebrated animals, it was first shown that Purkinje and Müller had, like myself, found multipolar ganglionic cells, connected with motor nerve-fibres, in the nervous centres of the vertebrata. Rudolph Wagner also showed, in 1847, that each of the large multipolar ganglion cells of the electric lobe of the torpedo, sends a connecting fibre, by means of a prolongation of the axis cylinder, to the electric roots of the vagus and trigeminus nerves. The other branching processes of these cells, from their granular or striated structure, appeared to Wagner to be intended to unite the cells together.

In some researches I lately made on the *torpedo marmorata*, I could find no such connections. I easily made a satisfactory section of the electric lobe, by previously treating the fresh brain with a solution of corrosive sublimate, or of bichromate of potash. I found the ganglionic cells to be multipolar, surrounded by a soft nucleated sheath, and lying in the interstices of a vascular net-work. The processes sent from them to the electric roots of the vagus and trigeminus were collected at the base of the lobe, forming strong fasciculi, visible to the naked eye. The other prolongations of the cells were invested with thin membranous sheaths, and formed soft nerve-fibres, with dark borders, which entered the medulla oblongata. The connection between the ganglion cells and sensitive fibres has not yet been demonstrated; the sensitive roots of the vagus and trigeminus do not enter the electric lobe, for that of the former enters the medulla oblongata, and that of the latter, the grey appendage of the cerebellum (the *feuillet restiforme* of Serres and Savi), which in its structure, viz., the size and shape of its multipolar cells, is analogous to the cerebellum, not the electric lobe.

I have in my possession transverse and longitudinal sections of the human spinal cord—prepared by the celebrated Stilling, and presented to me by him—which show the passage of the motor nerve-root fibres into the multipolar ganglion-cells of the anterior grey laminæ. (*Saulen*). The transverse section shows narrow lines of broad dark-bordered nerve fibres, which seem to connect the anterior and posterior roots. They extend from the point of entrance of the anterior roots into the anterior grey laminæ, or from the external surface

of the latter back to the posterior surface of the *substantia gelatinosa*, where the posterior roots enter it. Here they go among the ganglionic cells, which, by some of their processes, join the sensitive roots, which, for the most part, extend in broad thick lines through the *substantia gelatinosa* to the posterior grey laminae, and the large multipolar cells. Each of these circular fibre-lines indicates a way whereby we may explain those reflex movements which, in decapitated animals, occur by irritation of the sensitive nerves. And it is worthy of note that the long axis of the largest ganglionic cells corresponds, in its direction, to the long axis of the spinal cord; and that, besides the other processes by means of which they join the nerve-root fibres, they all send out branching prolongations, in the form of two poles, pointed towards the cranial and sacral extremities of the medulla spinalis.

The multipolar ganglionic cells, which I discovered in 1837, do not exist in the spinal ganglia, which consist, as I have this summer ascertained, by observations on fresh *Plagiostomata*, of *bipolar* cells. These, as Leydig demonstrated in the *Chimæra monstrosa*, form nucleated enlargements of the axis cylinder, and are surrounded by an epithelial cellular layer, and also by a firm membranous sheath, which is a prolongation of the neurilema of the nerve tubes. These bipolar cells are found in the spinal ganglia of man, and of mammalia in general. But they often appear to be *unipolar*, when both prolongations leave the cell very close to each other; and we often see, as Kölliker observed, cells with a single process, which, after a short course, divides into two fibrils. I have often observed divisions of the dark-bordered nerve tubes in the spinal ganglia of mammalia, but have not been able to see them in those of the *Plagiostomata*.

Among ganglia there are some which are *exclusively sympathetic*, and which are composed of multipolar cells, ensheathed, like those of the spinal ganglia, by a soft cellular layer and a firm membrane. The number of the cell prolongations varies from three to twelve, but by the subdivision of these, we may see three times as many, and this being regulated by the number of the nerves connected with the ganglion, there are fewer in the circumscribed ganglia (*Grenzganlien*) than in those aggregated, as in the solar plexus. These processes have the same chemical and optical peculiarities as are possessed by the axis cylinders of nerve-fibres. In the solar plexus we find ganglion cells whose processes become divided, like those of the cells found in the electric lobe of the torpedo. Among the multipolar cells of the sympathetic ganglia, we also observe, in mammalia and plagiostomata, bipolar cells. But these are altogether different from the bipolar cells found in the spinal ganglia, inasmuch as both their poles become subdivided, thereby rendering them analogous to the multipolar cells. The same is true of the unipolar cells, which are found among the multipolar in these animals, as also in Batrachians and osseous fishes, and of which, almost exclusively, the sympathetic ganglia are composed in the heads of mammalia. On making a transverse or longitudinal section of one of the sympathetic ganglia, from the thorax or abdomen of one of the mammalia or *Plagiostomata*, we see the single process (which is generally very broad) of such unipolar cells subdividing, after a short course, into many fibrils, which ramify in different directions, all of them extending peripherally in an indeterminate manner.

I have observed in mammalia that the multipolar cells of the circumscribed sympathetic ganglia¹ of the abdomen and thorax, by means of their processes, lying in the axis cylinder of the dark-bordered nerve-fibres, join those which extend from the spinal nerves to the said ganglia. In man and other mammals, each of these ganglia has at least two communications with the spinal nerves.

The lower uniting branch (*Ramus communicans sympathicus, s. revehens*) I

¹ NOTE BY TRANSLATOR.—By this, in the original *die multipolaren Ganglienzellen der Sympathetischen Grenzganglien*, we presume are meant the ganglia of the sympathetic chain, in contradistinction to those of the prevertebral plexuses.

have found to be *grey*; it contains very fine nerve-tubes (Bidder and Volkmann's) and a great many ganglionic fibres; it joins the spinal nerve at its peripheral expansion, and afterwards at its point of entrance, and sometimes close to the spinal ganglion it forms a ganglion composed of multipolar cells. The upper communicating branch (*Ramus communicans spinalis, s. advehens*), is *white*, and contains fibres, which, according to Scarpa, Wutzer, and others, can be traced to both roots of the spinal nerves. I have hitherto only been able to see these fibres entering the anterior roots; the remainder of them—which constituted the minority—became lost in the spinal ganglia. Hence it would appear that the sensitive fibres destined for the sympathetic nerve, must first be united to the spinal ganglion cells before they reach the gangliated chain. The fibres of these spinal connective branches either immediately enter the sympathetic ganglion, or, forming themselves into separate white fasciculi, they join the chain to go into the nearest ganglion. Since then, as shown by a transverse section of a sympathetic ganglion, all the spinal fibres entering it join the multipolar cells; and since the anterior roots of the spinal nerves contain only motor, and the posterior only sensitive filaments, it follows that the multipolar cells of these ganglia are as much connected with sensitive as motor nerve fibres. From each cell there extend, peripherally, broad and dark-bordered, and also fine fibres (Bidder and Volkmann's), as well as some in which no dark contours can be perceived. All these peripheral fibres can be distinguished as sympathetic, and different from the spinal, to which they are connected by means of the multipolar ganglion cells. The alleged existence of some sympathetic fibres, *not* connected with spinal fibres, and consequently not with the great central nervous centres, stands in need of confirmation. Few have hitherto demonstrated, in the nerves proceeding from the sympathetic ganglia to the nervous centres, spinal fibres in whose course no ganglionic cells were found.

From these researches, I consider it almost settled that, in the sympathetic ganglia, the ramification angles (*Verästlungswinkel*) of the sensitive and motor fibres contain ganglion-cells. This does not increase the centralising importance of each ganglion, inasmuch as we make it depend on the confluence of sensitive and motor fibres; nor does it support the idea that among the peripheral fibres issuing from a sensitive or motor ganglionic cell, we shall find sensitive as well as motor filaments. Leydig observed ganglion-cells in the ramification angles of sensitive fibres, in the *Carinaria Mediterranea*. Bowman and Corti have noticed this arrangement in the retina, and I have seen multipolar ganglion cells situated thus in the *Macula lutea*; still farther confirmation on this point is necessary, as perhaps the retina may, from its alternating fibrous and ganglionic layers, have acquired a structure remarkably analogous to that shown by Gennari to exist in the grey and cortical substance of the cerebrum.

Ganglionic cells are only found in the angles of *motor* fibres in the great central organs of the nervous system. Hence the question has been asked, Whether the sympathetic ganglia have a centralising power; whether their cells exist separately as sensitive and motor, connected together by intervening fibres; or whether each multipolar cell directly unites sensitive and motor filaments together? This question is as yet undecided as regards the spinal connecting branches, on account of their length, and because we are in possession of no method by which accurately to distinguish microscopically between the two kinds of fibres. Certain observers, however, affirm that a direct connection exists between the multipolar cells and the sensitive and motor fibres. In ganglionic cells, whose long axis corresponds to the long axis of the ganglion, we often see two fibres entering at one pole, and going out at the other. If there were four fibres together of the same kind, the cell would form an anastomosis between them. Leydig found this once among the bipolar cells of the gasserian ganglion of the *Chimaera monstrosa*. If, in a small multipolar ganglion cell, taken from the solar plexus of one of the mammalia, we compare the number and direction of the nerves entering it and issuing from

it, with the number and direction of its processes, we shall find the most perfect harmony existing between them, i.e., each cell in such multipolar ganglion is united by fibres to all the nerves connected with the ganglion. It seems improbable, however, that in such cases the nerves entering and issuing contain either only sensitive or only motor fibres, for we know that in other multipolar or sympathetic ganglia (as for example, the ciliary, otic, and sphenopalatine), the nerves entering them contain filaments of both kinds.

The sympathetic ganglion-cells being the connecting media between the sensitive and motor fibres, excitations of the sympathetic-sensitive fibres are communicated by every cell to the sympathetic motor; by means of the spinal-sensitive uniting branches they are conveyed to the great nervous centres, the brain and medulla spinalis; and being thence transmitted by the spinal motor fibres, they act on the sympathetic ganglion-cells and their motor processes. Besides the sensitive and motor sympathetic fibres, there is a third class of sympathetic filaments, probably essentially nutritive, or trophic in their nature, although this is not shown by any physiological researches, for possibly the nutrition of these nerves depends on the influence they exert on the contractile walls of the vessels.—*Monatsberichte der K. Akad. d. Wissenschaften*. Berlin, January 1854.

ABSORPTION OF COLOUR IN THE SKIN OF A NEGRESS.

Charlotte is a woman 34 years old, and living within one mile of me, whose skin is as fair as that of any lady of Caucasian blood, and who was as black at the age of 11 as any African. She says her health has been uniformly good, with the exception of one "spell of bowel complaint," which occurred when she was about 10 years old; sometime after which a white spot appeared on her forehead, which gradually though slowly enlarged. In the meantime other spots appeared on different parts of the face, which also increased in size, until the whole face became perfectly white. The change in the colour of the face was completed in about six years, and she says that after her face "turned white," her whole body changed in one week. I saw her frequently during the metamorphosis, and noted its progress, during which time her countenance was so hideous that she was a fright to all the juveniles in the neighbourhood. She is at present strictly a white woman, except her eyes and hair, which are those of the negro,—the latter, however, being whitish all round the margin. The skin freckles, and is easily blistered by the sun. She is a good servant,—has ordinary intelligence,—has had ten healthy children, all of whom are as black as their father, who is a full-blooded African.—*Boston Medical Journal*.

BERNARD ON THE CONDITIONS UNDER WHICH CERTAIN SUBSTANCES, USUALLY RETAINED IN THE BLOOD, PASS INTO THE URINE.

During active digestion, the blood of an animal contains a certain amount of sugar, which does not pass into the urine; but if the mass of blood be diminished by bleeding, the urine soon becomes saccharine. If we inject a limited quantity of saccharine matter into the blood of an animal ($\frac{1}{4}$ gramme for a rabbit fasting, of the weight of 2 kilogrammes) the urine will contain no trace of sugar; but if the animal be bled before being subjected to this experiment, a certain quantity of sugar will pass into the urine.

M. Bernard explains these phenomena by considering that the loss of blood renders the animal of less volume, reducing it to the condition of one of smaller size. It is known that small animals may be poisoned by doses easily borne by larger animals of the same species; and animals, after being bled, cannot withstand the same dose of a poison which they supported previously. These facts were formerly attributed to the increased absorption caused by the withdrawal of blood; but in the experiments above related, absorption had no part, as the substances were introduced directly into the circulation.—*Soc. de Biologie*, 1853.

BERNARD ON THE PNEUMONIA WHICH FOLLOWS SECTION OF THE PNEUMOGASTRIC NERVES.

M. Bernard has found that in animals after section of the pneumogastric nerves in the neck, the lungs receive a *larger* amount of air than before the lesion. In the time that a healthy rabbit will absorb 20 cubic centimetres of air, a rabbit of the same size, in which both pneumogastric nerves are cut, will absorb 32 cub. centimetres. The pneumonia is caused by this exaggerated inspiration. The air-cells, over-distended, burst; extravascular succeeds the intravesicular emphysema; and blood is effused into the parenchyma from the ruptured vessels. Inflammation then ensues. The older the animals the greater is the resistance of the air vesicles against the distension, and the pneumonia is correspondingly more rare in them.—*Comptes rendus de la Soc. de Biologie*, June 1853.

SURGERY.

PROFESSOR VIRCHOW ON THE GROWTH OF HEALTHY AND RACHITIC BONE.

In a recent number of his "Archives," (Band. V. Hft. 4), Virchow has published a very elaborate monograph on the above subject, and in giving a brief resumé of it, we shall glance at the observations recorded by other investigators.

In rachitic long bones, the curvature principally occurs in the shafts, the enlargement in the articular extremities, and the shortening in the epiphyses. Their diaphyses soften, while their epiphyses swell; and although the bones are unusually broad, they are also abnormally short. The swollen epiphyses occasion, in the joints, a peculiar protrusion of the extremities of the corresponding bones; and, at the sternal ends of the ribs, they form a row of protuberances, which can be felt under the skin. The sternal extremities of the ribs may be thus affected very early: Virchow has seen it occurring as early as the second month, and also in the sixth month, with rachitic affection of the tibial epiphyses and of the metatarsal phalanges. Ruz is of opinion that in very young children distortion occurs generally in the ribs and upper extremities; in those from three to five years old in the pelvis and lower limbs; and in the spinal column in those who are more advanced in life, (*Gaz. Méd.*, 1834). This author also was the first who described carefully the internal structure of rachitic bone. He observed no clear boundary between the bone and cartilage at the junction-point of the epiphysis and shaft: he described the cartilaginous substance as bluish and semi-transparent, and apparently terminating irregularly in a whitish horizontal line. Underneath this lay an elastic, reticulated, and reddish texture, half an inch to one inch thick, resembling a fine sponge, and yielding blood on pressure. Between this and the medullary canal lay another spongy layer. Guerin, in 1847, described a fine structure of similar character, which he termed *spongoid*, with small isolated portions of cartilage scattered through it. These were small rounded bodies, of a bluish tinge, not firmly united to the spongy substance, and capable of being picked out by the point of the scalpel, leaving small excavations behind. Kölliker, in a series of more minute investigations, showed that the two layers—described by Bidder as the ossification-boundary of the epiphyses, viz.—a yellowish stratum next the bone, containing large cartilage cells arranged regularly in rows, and an outer bluish layer consisting of smaller cells, irregularly arranged,—existed also in rachitic bone, but that the former was enlarged, being two to five lines thick, while it is only half a line in healthy bone. The boundary line between the bone and cartilage was undulating and serrated, on account of the unequal ossification; and spicula of bone, one to four lines in length, projected into the cartilage. He also observed that the deposition of calcareous particles was deficient at the ossific boundary, and that nearly all the cartilage cells became converted into bone cells—before the matrix, (*Grundsubstanz*), and independent of calcareous deposition—by the thickening of their walls and the formation of fine porous canals. "Hence," says Virchow, "rachitis would seem to

depend on certain changes occurring in the cartilage, before the regular commencement, and without the immediate sequence, of ossification."

Meyer extended these researches, (see *Müller's Archives*, 1849, p. 359), and, in the texture alluded to, described three substances. In the greyish translucent matrix he observed dim brownish-yellow places, which here and there were reddish, and in these as also in the matrix there were whitish spots. The projecting portions of the adjacent ossified bone gave to this dim yellowish substance a serrated margin. The whitish specks alluded to are completely ossified bone; the dim yellow spots are those in which the formation of medullary cavities (*markräume*) is proceeding; and the greyish gelatinous mass is the cartilage, changed in the formation of its parent cells. Medullary-space formation, according to this author, occurs by the development and subsequent softening of fibres, and the cartilage cells at their circumference, becoming thickened in their walls and finally devoid of nuclei, amalgamate with one another and also with the surrounding matrix, leaving behind only their roundish and stellate cavities—so that the part assumes the appearance presented by bone deprived of its earthy constituents by muriatic acid. In the greyish substance he found primary and secondary cartilage cells, both larger than ordinary; and also unusually broad and dark intercellular tissue. Ossification occurs in it only in scattered patches, as it were *experimentally*.

Virchow's own observations confirm, in most respects, the researches of these authors; but he looks upon these structural changes we have described as occurring only in the advanced stages of rachitis.

He believes that bones increase in length by pure endogenous growth of the inner surface of the cartilage of the epiphyses, a new layer being always produced as soon as the former one becomes ossified. Ossification first commences in the intercellular substance, and in the thick-walled capsules of the cartilage cavities; part of this calcareous deposition becomes afterwards softened, and thereby are formed the medullary hollows of the spongy bone-substance. The following arrangement is found at the articular extremity of a long bone:—first, next the joint there is the ordinary cartilage; next comes the layer formed by endogenous growth, composed of large cells next the bone, and smaller ones externally, which have a bluish-transparent, oftentimes almost gelatinous appearance; next, there follows a yellowish, opaque layer of a finely reticulated texture, consisting of cartilage cells, surrounded by calcareous rings, which are being metamorphosed into bone cells; and finally, we have the spongy substance of the bone, with its spaces filled with very vascular medulla. The layer in which the cartilage cells are surrounded by bony rings is not, nor does it become true bone; for medullary spaces are formed in it, by the softening of more or less of its calcareous matter, which gradually increase in size, and between which there remain intervening cancelli. These peculiar alterations of the originally simple cartilage render it difficult to discover the true origin of the lacunae or bone-corpuscles. We are certain that although all the cartilage cells become surrounded by calcareous rings, they are not all transformed into osseous corpuscles; and it has therefore been doubted whether the lacunae ever originate from these bodies at all. In normal ossification the problem might be easily solved by actual observation, were it not that the deposition of calcareous matter in the cartilaginous matrix obscures the process in its cells. In rachitic bone, however, this is not the case.

When we make a longitudinal section of a long bone, in the first stage of rachitis, we find the medullary canal bounded, at either extremity, by a layer of fine cancellated tissue, the medullary cavities of which are filled with a dark red marrow, principally composed of small granular cells, which Virchow looks upon as formed by the endogenous growth of part of the original cartilage cells. The cancelli of this tissue are, for the most part, made of true bone. In it, Virchow has observed calcareous bodies of a round or ovoid shape, with central hollows, which were loosely attached to the surrounding substance, and corresponded to cartilage cells.

Next to this comes a yellowish layer, (the "*spongoid*" of Guerin), wherein ossification is visible, advancing in the somewhat striated cartilaginous substance lying between the cavities of the large cell-groups. The whole of this ossifying texture presents a very uniform reticulated appearance, enclosing cartilage cells in its bony network. External to this, and next the cartilage, Virchow describes a structure wherein ossification extends further than the intercellular substance to the thickened capsules of the cartilage-cells, forming incomplete bony rings, which encompass nearly two-thirds or three-fourths of the cartilage-cavities, leaving their peripheral surfaces open and free. This appearance can be best seen by a *longitudinal*, and the reticulated structure by a *transverse* section of the bone.

Next to this "spongoid" tissue lies a bluish layer, composed of large cartilage cells, a transverse section of which presents a fine reticulated structure, like that of the ossifying parts, although no trace of calcareous deposition can be found within its meshes. The intercellular substance here is yellowish, and has a granular and striated appearance: the cartilage cavities are lined by thick, double-contoured capsules, and contain pale, slightly granular, nucleated and nucleolated cells. In this layer also vessels and vascular canals are formed. The matrix, in certain directions, becomes distinctly striated, fibrillated, and at the same time, yellowish and opaque; and, in the direction of its striae, the cartilage cells become elongated, spindle-shaped, and relatively smaller, their nuclei increase, and probably latterly divide; and there is gradually formed a soft fibrous texture, through which are scattered numerous small cells, with dark, solid, and comparatively large nuclei. On the addition of acetic acid, this texture becomes dark, and juice expressed from it gives, with that re-agent, the ordinary mucous precipitate. In fact, says Virchow, "*the cartilage has become changed into a sort of mucous texture.*" This metamorphosis not only occurs at the boundary-line of ossification, but also at the perichondrium; and both rows unite, in certain parts, with one another, forming oblique anastomoses by connecting loops. Vessels soon appear in these directions, which form tolerably large canals in the interior of this mucous structure. Virchow was unable to determine whether these vessels originated from the prolongations of vessels previously formed, or occurred by direct *canaliculation* of the cartilage; but he was able, very distinctly, to observe their gradual advance beneath the perichondrium of the costal cartilages. The perichondrium here appeared swollen in certain parts, by the development of a dim opaque yellowish body at its inner side. After some time this body enlarged and formed a solid conical peg, (*zapfen*), in the interior of the cartilage. Acetic acid showed this to consist of narrow elongated cells, embedded in a striated cartilaginous matrix. In all of these bodies, Virchow distinctly observed the loopings of vessels.

Next to this layer there is the cartilage of the epiphysis, which, in its normal condition, in young children, is traversed by large reddish transparent vessels, which enter it from the vascular parts surrounding it on all sides. In many cases we see, in the epiphyses, points of commencing ossification, which consist of cancellated texture containing red medulla, and are generally surrounded by a fine spongoid layer, similar to that which lies at the boundary of the diaphyses. Between these bony centres and the large-celled bluish layer at the line of ossification of the shafts, there lies a layer of cartilage of the ordinary appearance.

In somewhat older children, and in more advanced stages of the disease, the arrangement and connections of the structures described, remain essentially the same, but their size and irregularity undergo remarkable alterations. Virchow has seen this advanced stage in children five weeks old. Ossification, in such cases, which hitherto has advanced regularly, begins now to proceed more slowly, and the boundary-line of the young bone becomes very irregular. The swollen part at the extremity of the bones, when seen in transverse section, is found to consist of the bluish texture, extending outwards in an oval form, by endogenous growth. Its aggregated layers, which correspond to the long axis of the bone, vary in distinctness; and between them are found numerous rounded and elongated me-

dullary spaces, which are reddish when blood-vessels are present, and yellowish or brownish-yellow when these are absent. A whitish-yellow line lies internally next the bone which corresponds to the layer of young bone; and between this and the bluish layer intervenes a red irregular margin, broad in some places, narrow in others, consisting of red points clustered together, which are recently formed, and vascular medullary spaces. Various scattered ossific points found here, especially in the intercellular portions of the matrix are finely striated, are composed of delicate fibrillae, and terminate in a thicker, stronger, shining layer, which forms the enveloping capsule of the neighbouring cell-groups. Homogeneous calcification occurs in this marginal layer, and the intercellular spaces become infiltrated with a finely granular yellow mass. But osseous deposition does not occur in this marginal layer alone, for it goes on also, in many places, within the cell-groups—principally at the points of junction between their individual cells. A thin stripe of calcareous matter often intervenes between two cells, while these are still bounded on both sides by an unossified layer. At other times ossification commences in the internal capsules, (leaving the dividing bridges within quite unaffected), and calcareous rings are formed which give the whole structure a uniform reticulated aspect.

(To be Continued.)

ON THE INOCULABILITY OF SECONDARY SYPHILIS.

The Imperial Academy of Medicine in Paris having decided against the truth of the doctrine of syphilisation, after the discussions of which an abstract is given in the February number, proceeded at subsequent meetings to consider whether or not secondary syphilis is inoculable.

M. Velpeau said, that the transmissibility of secondary syphilis was not a new question. Hunter had first directed special attention to the subject, and had decided in the negative; but his opinion met with little favour at the time, although of late years it has found a good many partizans in France. *M. Velpeau's* own opinion was, that not only those who have had chancres or a virulent gonorrhœa, or who are descended from syphilitic parents, are liable to the constitutional affection, but that all or almost all the manifestations of secondary syphilis, are contagious. His opinions were founded on several grounds:—1st. Universal belief. It was scarcely possible that what had been believed for centuries could be absolutely false. No doubt numerous fabulous modes of contagion had been described, as, for instance, by the breath or saliva, but still he believed the general belief to be founded on truth. 2d. The experience of medical men in different countries. Although it be conceded that in many of their observations there is a certain amount of error, still there must be some truth in such a mass of facts coming from such various sources. 3d. *M. Velpeau* would appeal to his own observations. He had begun by trying to inoculate animals with syphilis, and in this he had constantly failed; and he might observe that, in spite of what had been said to the contrary, he did not consider it proved that this inoculation was possible. But he had observed secondary symptoms inoculate themselves. Thus, a young man who had had chancres on the penis six months before, was affected with a small vegetation on the glans. *M. Velpeau* kept the prepuce in close apposition to this for some days, and a similar vegetation made its appearance in the latter situation. He had since made the same observation frequently, and had observed that condylomata (*plaques muqueuses*) were often propagated in the same way. These considerations had first established in the mind of *M. Velpeau* the possibility of the transmission of secondary syphilis. Other facts, to which he should immediately allude, had confirmed him in this opinion. Those who maintained the opposite opinion, were inconsistent with their own doctrines. They admitted hereditary transmission, but not the direct transmissibility of secondary symptoms. But what is hereditary transmission but the transmission of a constitutional affection? Another important proof of the truth of his opinion, was drawn

from what is seen in the case of nurses or infants affected with secondary syphilis. In many instances it had been observed that the unhealthy nurse could affect the healthy infant, and *vice versa*. Among other cases, *M. Velpeau* mentioned the following: A family consisted of a husband, wife, and four children, all healthy. The woman took to nurse an infant covered with an eruption, and suffering from sore throat. The family being poor, the same drinking cup was used by all. Soon afterwards, one of the daughters became affected with symptoms similar to those of the infant, and died; the nursling died; the mother and two other daughters were attacked with sore throat, and were cured by mercury. On the other hand, many cases might be quoted where a nurse affected with constitutional syphilis has transmitted it to her nursling. As to inoculation, *M. Velpeau*, without approving of it, would not absolutely reject it, though he considered it a mistake to suppose that it could furnish much reliable information. If it succeed, it only proves what hardly requires proof; if it fail, it does not prove that the affection was not syphilitic. Supposing that primary affections are always and easily inoculable, it does not follow that secondary affections should be inoculable with equal facility. But it had been shown in a number of cases that secondary affections could be inoculated. He would only refer to the observations of Wallace in England and Waller in Germany. For these reasons he believed that secondary syphilis was contagious.

M. Lagneau considered it undoubted that secondary symptoms were contagious; but, from the very fact of their being consecutive, their contagious power was not so strong nor so easily proved as in the case of the primary. In fact, in general, secondary symptoms could not be inoculated; sometimes, however, they might, though they were propagated readily by sexual intercourse. *M. Lagneau* referred to the case before the Academy, in which the individual had inoculated himself with pus from a secondary ulcer on the tonsil; the inoculation was followed by the characteristic pustule. The fact of the inoculability of secondary syphilis takes away from inoculation all its importance as a diagnostic agent.

M. Ricord regarded this question as most important, both with reference to pure science, to public and private hygiene, and to legal medicine. He had begun the study of syphilis without any preconceived views; much had indeed been already done by Hunter, but his doctrines had been established on his own experience. From clinical observations and experiments, he (*M. Ricord*) had proved, 1st. That it is only pus from a chancre, taken at a particular period, which is inoculable and capable of reproducing chancre. 2d. That a bubo, the result of absorption, following a non-indurated chancre, furnished like the chancre itself inoculable pus. 3d. That a bubo, considered venereal, may not furnish inoculable pus; because, the pus *surrounding* the affected gland may have been employed, not the purulent matter of the gland itself; because, the bubo may have been merely sympathetic; or, because the bubo may have been idiopathic, and have been mistaken for a direct manifestation of syphilis (*Bubon d'emblée*). 4th. Constitutional symptoms, whether secondary or tertiary, have not been found capable of inoculation. 5th. Syphilitic affections which are not inoculable are not contagious. 6th. Simple gonorrhœa, not symptomatic of urethral chancre, is not inoculable. Since he had established these doctrines, they had been confirmed by numerous other observers. Those who maintained the inoculability of secondary syphilis appealed to clinical observation and experiment. It was singular, however, that they had so few proofs to refer to; their cases were evidently exceptional; whereas if their opinion were correct the number should be immense, so many individuals being affected with secondary syphilis who take no precautions against infecting others. Many of the observations brought forward on the other side, left much to be desired. It was often very difficult to determine whence the first symptoms had been derived; and even supposing that patients have no desire to deceive, how can they always describe with accuracy the condition of the other party? In most cases it is no doubt easy to distinguish between primary and secondary affections, but sometimes it is exceedingly difficult. What is called *condyloma*, and which is considered as invariably a secondary affection, is sometimes the

remains of a chancre which has altered its form, but which is still a primary affection and is still inoculable. Here, then, is a source of error in diagnosis. Before saying that secondary affections are inoculable, it must be shown that the diagnosis was precise and free from all possible sources of error. This *M. Ricord* did not consider to be the case with regard to the observations which had been brought forward. He himself had attempted very frequently to inoculate secondary symptoms, but had invariably failed. With regard to the proof derived from the alleged transmission between nurses and infants, the facts stated were quite at variance with his own experience; and *M. Ricord* referred to others who had enjoyed the best means of observation, and who were on this point completely of his own opinion. *M. Velpeau* had told them of the contagiousness of vegetations, but in *M. Ricord's* opinion vegetations were not syphilitic at all, and it was anything but proved that they were contagious. The same cause which produced them at one point might have given rise to them at others. As to considering the possibility of the hereditary transmission of secondary syphilis as a proof of its contagious nature, he was surprised to hear *M. Velpeau* make such a statement. On this principle it might be maintained that phthisis, cancer, gout, or insanity, are contagious. *M. Ricord* said he did not wish to systematise and maintain that secondary symptoms were absolutely neither contagious nor inoculable, but in order to make him change his opinion, much stronger proofs than had been hitherto brought forward would be required.

M. Gibert did not lay much stress on experiments as proving the inoculability either of primary or secondary syphilis; he would only trust to clinical observation. Drawing his knowledge from this source, he felt justified in making the following assertions:—1st. The primitive symptoms of syphilis are multiple; 2d. The regular progress of development of secondary symptoms, which some have endeavoured to establish, is far from being constant. 3d. Secondary symptoms may exceptionally show themselves contagious, under particular circumstances of intimate cohabitation. 4th. Inoculation should not be employed for any purpose, even as a diagnostic agent, seeing that its results are not to be depended on.

M. Roux acknowledged the services which had been rendered by *M. Ricord*, and stated that he agreed with him on many points, but still as to the contagiousness of secondary symptoms he sided with the others.

On the conclusion of this discussion, which occupied the greater part of six meetings of the Academy, a vote was not taken on the subject; but it was decided that at some future period a committee should be appointed to inquire into, and if possible resolve, the question of the transmissibility of secondary syphilis. As yet, however, no steps have been taken in the matter.

DR LONDRES ON INSECT-LARVÆ UNDER THE HUMAN SKIN.

According to this author, there are often found in Surinam, below the skin, both of Europeans and negroes, the larvæ of an insect called there the "mosquito-worm," which resembles closely the *Oestrus Bovis*. These occasion furunculoid circumscribed tumours, the size of a nutmeg, which discharge a bloody serum through a small opening at the surface. These tumours are very painful, and, if not subjected to treatment, they form open ulcers. The treatment adopted is blowing tobacco smoke into the tumour through the aperture, and thereafter squeezing it, which causes the larvæ to crawl out of its centre. Dr L. found them rapidly cured by free incisions. He cannot yet determine whether these larvæ belong to the *Oestrus Bovis* species, or whether they are different,—the *Oestrus Hominis*. Howship mentioned to the Medical Chirurgical Society of London, the case of a soldier from Surinam, who had the *Oestrus Hominis* in his shoulder, and of a youth in Santa Anna, in South America, in whose scrotum they were discovered. Baron von Humboldt also saw Indians in South America, whose abdomens were covered with small tumours, which he conceived to be due to the subcutaneous presence of the larvæ of the *Oestrus*.—*Nederland Weekbl.* July 1852.

BLUMHARDT ON THE USE OF COLLODION IN BURNS.

This fluid, when applied to burns, promotes healing and prevents suppuration. Blumhardt has tried it, with great success, in three cases, one of them caused by explosion of gunpowder, and two by the ignition of spirits of wine, where the breast, neck, face, and hands were all severely scorched. Collodion was applied to the skin an hour and a half after the accident, by a hair-pencil; the redness, pain, and swelling, were thereby diminished, and the patients soon experienced no inconvenience save the tension occasioned by the firmly adherent pellicle. The inflammation completely subsided, and the recovery was rapid. He considers the collodion to act beneficially in two ways; *first*, by affording a safe protective covering to the sensitive cutis, and *second*, by giving a uniform support to the part, and relieving the capillaries from all undue distension.—*Württemberg Corresp. Bl. No. 56.*

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION XII.

INDIAN MIDWIFERY—NOTES ON, BY DR WISE, H.E.I.C.S.

Dr Simpson read the following communication from Dr Wise, author of the learned "*Commentary on the Hindoo System of Medicine.*"

Remarks on the Treatment of Women during Pregnancy and Labour by the Asiatics.

The vast countries which the Mahomedans conquered extended from Spain to China, embracing a portion of Europe and a large part of Africa and Asia; in all of which countries they have left traces of their manners and customs. Even among the Hindoos, so prejudiced in favour of their own habits, considerable changes have taken place, from the mingling of these two great races. In both, the treatment of females during parturition resembles so much each other, that the following remarks may be applied to either; although some differences in unimportant local customs may be found in parts of these extensive countries.

The Koran has always been considered by the Mahomedans as the standard to regulate their religious, civil, and scientific pursuits. One of its peculiarities is its robbing the female of her rights, and placing her in a degraded position in society, and from their ignorance, the minds of their offspring remain uncultivated, and are filled with prejudice and superstition. This want of confidence in females induces the Mahomedans to employ ignorant and prejudiced menials during the period of parturition, by which both the mother and offspring suffer. So frequently are their lives sacrificed, and so convinced are they of the superior method of proceeding of European practitioners, that they often require their assistance in the hour of difficulty and danger. On this account I had frequent opportunities of witnessing their method of treatment, and the tenets which they inculcated both among the Mahomedans and Hindoos, of which the following are a few particulars:—

From the time that the female becomes pregnant, various religious ceremonies and superstitious observances are used to promote the welfare of the foetus, and to prevent the influence of the evil eye. This is so much dreaded, that during pregnancy, the woman is rarely allowed to go abroad. At the same time, her peace of mind, happiness and comfort must be promoted, and gentle exercise enjoined with attention to diet, which is varied with the period of utero-gestation. On the fourth month curdled milk should be

administered ; on the fifth milk, on the sixth ghee (clarified butter), on the seventh ghee and milk, so as to promote the formation and development of the foetus ; while on the eighth month, to ensure an easy delivery, enemas with milk, oil, and ghee are administered, and the body is smeared with oil. This is followed by other arrangements for the delivery, which will be afterwards described.

During the period of utero-gestation, the female is to avoid anger, fear and grief, venery, fatigue, and watching, raising heavy weights, coughing, and violent exercise. Before the fourth month, bleeding, cupping, and purgation, and emetic medicines, are only to be used in cases in which it is absolutely necessary, when bleeding in small quantity, and laxatives may be employed. In all such cases external applications should be preferred. For a like reason, indigestible food, strong and pungent liquors, and disagreeable smells are to be avoided. Precepts which prove that considerable care had been taken by Asiatics in observing, and discrimination in inculcating the line of conduct, which ought to be pursued in such cases.

For lessening the distressing symptoms which so often accompany pregnancy, they recommend—for nausea, gentle exercise, keeping in the mouth mint leaves, and the seeds of the pomegranate. For an unnatural craving, gentle aperients, with mild acids, such as a syrup made of unripe grapes. A want of appetite is treated by poultices over the pit of the stomach ; and heartburn by sipping warm water, with gentle exercise, carminatives, with a diminution of food. Hemorrhage during pregnancy is treated by certain astringent leaves which are bruised, mixed with vinegar, and applied externally over the abdomen.

Before considering the means employed by the higher classes of natives in India, I shall add a few remarks on labour as it occurs among the rude tribes of Hindostan. The females of such races are accustomed to so much exercise and labour, that they are generally of a healthy habit of body, possess fine figures, live on simple food, and nature is allowed to follow her own course during parturition, which is more rapid, and attended with less suffering and danger than among more civilised nations.

This is promoted by the relaxed habit of body of Asiatic females, the early age at which they marry, their temperate habits, and healthy systems, which render childbirth peculiarly quick and safe. The poorer classes often merely go aside when labour commences, a few pains complete it, and the only assistance which they require is a piece of a broken pot to divide the umbilical cord after it had been tied. The secondines being removed, the infant is wrapped in a piece of rag, the mother cleans the mud floor of the hut, walks down to the neighbouring stream, and after she has washed and bathed, returns to her domestic occupation as if nothing particular had occurred.

I knew an instance of a slave girl being delivered without awaking a female sleeping with her. At Agra, a class of women are in the habit of walking several miles to graze their cattle, and when thus at a distance they are sometimes delivered in this easy manner. They wrap their offspring in a piece of cloth, bathe, and return in the evening several miles on foot, driving their cattle before them. Next day they continue their usual occupation. They suffer much in after years from such premature exertion ; as also from their early marriages, as they often become mothers before they reach their twelfth year. It is, however, to be recollected that in Asia, especially in a hot and moist country like Bengal, the female arrives at puberty between eight and nine years, and the male at fifteen or sixteen, which is three or four years earlier than in Europe.

Among such a primitive race deformities will sometimes occur, and unfavourable presentations will retard delivery, and render it dangerous ; but such events are much more rare than among refined, and, consequently, more civilized nations. When native practitioners are consulted on these occasions, they chiefly rely on prayers (*montres*), and charms ; such as expanding dried and shrivelled-up sea-weed, by putting it in water, or pulling up certain plants by the roots, which operation is famed to assist delivery.

In more populous parts of the country, and in large cities where a more artificial manner of living is pursued ; and among the higher classes of Hindoos and Mahommedans the form of body, and strength of constitution is enfeebled by ease and luxury; and the want of healthy exercise renders parturition more difficult and dangerous. In those periods of suffering and anxiety, the assistance of their elderly and experienced friends might be supposed to be sufficient, were they not prevented by the pernicious influence of caste. By Asiatics, parturition is a period of impurity, and this seems to be strongly impressed on the Mahommedans by the prejudices of the Hindoos. The important and most difficult art of midwifery among the Asiatics is considered degrading ; and low caste, feeble, and ignorant women are alone employed, by whom the processes of nature are interrupted, and the most pernicious method of treatment is followed to the great danger of the mother and child.

Women have been employed to assist in parturition from the earliest ages. These females appear in some countries to have been of some rank and weight in society, as we find in Egypt they resisted the cruel order of Pharaoh, who commanded the midwives to kill all the male children of the Hebrew women. Among the Greeks and Romans women alone were employed. Phanareto, the mother of Socrates, was a midwife ; and Plato, Hippocrates, Galen, and Acteus mention midwives. They were likewise employed to negotiate marriages, a duty they still perform in Bengal. The same system was pursued in Europe after the fall of the Roman empire till the seventeenth century, when medical men were employed as accoucheurs.

From a very early period practitioners were consulted in difficult cases of labour, as is mentioned in the second labour of Rachel, the wife of Jacob ; and, again, when Tamar was delivered of twins, and they are likewise mentioned in Exodus. The same practice is still pursued in Hindostan when females are employed in ordinary cases, and practitioners are called to assist in cases of difficulty and danger.

The midwives in Hindostan are named *dyhs*, and are of the lowest castes (*Haree* and *Chunar*) ; while the Mahommedan midwives are usually the wives of religious mendicants, named *tizodns*. All these midwives are extremely ignorant and superstitious ; so that even considerable experience is usually of little use in improving their opinions and method of proceeding.

In the ninth lunar month the pregnant female is placed under the care of these *dyhs* ; when the Hindoo female is removed to the temporary shed, which is erected without the dwelling-house, or to a low, damp, unwholesome room, where she is to be confined, as her person, during the period of uncleanness, would defile the dwelling-house. At this period of anxiety and suffering, she is separated from her friends, and placed under the charge of this ignorant and degraded midwife, with whom, on any other occasion, she would not associate.

A month before pregnancy is expected, a teaspoonful of almond oil is taken daily, and the warm hip-bath is frequently used, and warm water is thrown over the patient's head, while oil is applied to the abdomen, loins, and the external parts of generation to relax the parts, and thus to accelerate labour. With the same intention, in many parts of India, after the seventh month the woman's clothes are bound tightly above the foetus, from a fancied advantage in keeping it down in the abdomen, which is further promoted by pressing it frequently downwards.

To promote labour the patient is made to walk about, she is directed to inhale the smoke of burnt horns, and the eyes of salt-water fish ; holding the nose and mouth in the act of sneezing, is likewise considered of use in promoting delivery, and in producing abortion. This is a crime of frequent occurrence in Hindostan, and the midwives are frequently aware that they can produce it by separating the membranes round the mouth of the uterus, by which means the person often avoids the shame of being proved to have indulged in illicit intercourse.

The Hindoo female thus leaves her comfortable and warm dwelling for a

damp and cold hut, which is rendered still more dangerous by the hot drinks she is required to take, and the large fires continually burning to keep away evil spirits; while the matted hut allows the free ingress of the night air, which blows upon the heated body of the female extended on the cold and damp earth, often without the necessary clothing, and at a time when she is so subject to disease. The ignorant Hindoos suppose that devils take advantage of this period of weakness to enter into the body of the mother and child; to prevent which large fires are kept burning before the door of the hut, in which the parings of leather are burnt, and persons must walk over and shake their clothes to prevent the passage of any devils. In other houses, a plate of rice and cowries are placed on each side of the door to propitiate the same personages.

The patient is delivered while resting on her back, in the lap of a female sitting on the ground, while her knees are bent and separated by two other females, one sitting on each side. To facilitate labour the parts are lubricated with oil, and the *dyhe*, resting on her hams before the patient, instead of supporting the perineum, urges the female to assist nature in expelling the child; while she introduces the fingers of her two joined hands in a conical figure, and by separating the palms, stretches the external parts, and thus, they suppose, facilitates labour. This, however, is not the case. The mechanical violence irritates and swells the parts, diminishes the relaxing influence of the mucous secretion, and thus retards labour, and increases the suffering and danger of the patient's life. This is likewise produced by rupturing the membranes at an early stage of the delivery, by which the natural soft wedge, which facilitates so much labour, is removed, and a natural becomes a case of lingering labour, which, with the regimen and exposure, endangers the lives of the mother and infant.

When the infant has been delivered, the cord is tied near the abdomen of the infant with a piece of lint, catgut, or silk thread. The poorer classes divide the cord with a piece of broken earthen vessel, while others use a sharp piece of bamboo for this purpose. Among the Hindoos the *dyhs* perform this simple operation; but among the Mahommedans a violent controversy has long existed as to the person who should divide the cord. The orthodox declare that the low caste Chunar midwife should do it, as she is considered unclean. In many Mahommedan families experienced matrons are employed for delivering the female, and the low caste *dyhe* is only sent for to divide the naval string, and to remove the secondines, for which she receives a few pence, up to a couple of shillings, according to the circumstances of the family. When in good circumstances, the midwife is often retained with the female as a sick-nurse, and attends to the regimen of the mother and infant during one lunar month, or in some cases forty days, during which she remains in this stifling atmosphere, and is supposed to be unclean. During this time a good deal of the nurse's time is passed in warming the hand and pressing the palm upon the bodies of the mother and child, which, it is said, strengthens and warms. It is likewise supposed to hasten the completion of the bony union, gives compactness to the parts, and prevents boils and other complaints. The infant is soothed by the warm application, and remains asleep. This application is continued for many days, for half an hour evening and morning. In other respects, they too often neglect, or follow the dictates of superstition, exposing them to the most pernicious influence of cold. For this period of attendance the nurse usually gets eight shillings, or the wages of a common workman. A new sect of Mahommedans (Ferazees) declare that, it is improper to employ a low caste woman to divide the cord, and as it does not render the person unclean, it should be performed by the father, or one of the relations of the child. The pertinacity with which this question is discussed shows the superstitious ignorance of the people, while the modification proposed proves that more rational views are beginning to be held on the subject.

For unnatural presentations very considerable variation exists in their method of proceeding, in different parts of Asia. Face, head, and knee pre-

sentations are left to nature, and medicines are only given to promote delivery. The more intelligent Hindoo authors recommend, in the two last varieties, that the presenting parts be pushed up, and delivery accomplished by the feet. Nothing is done in arm presentations, but dragging it downward, in the vain hope of completing delivery. In the Hindoo writings it has been recommended to push up the arm, and bring down the head; and when this is not sufficient to save *the mother*, a hook is used to remove the dead foetus, as otherwise the mother will soon die if undelivered. The other means employed are emollients externally, and stimulants internally.

In rare cases an abscess has formed in the parietes of the abdomen, and the dead foetus is thus expelled. Such cases afforded a hint to practitioners to perform the Cesarean operation when the mother had been killed, near the termination of pregnancy, and the foetus was alive. In the East this operation has been employed from a very early age; and so apparent and necessary is it in certain cases, that I have known a case in Bengal in which two ignorant persons performed the Cesarean section with a razor, in a case of great deformity, and thus saved the life of the mother and child. In another case delivery could not be accomplished, and a native practitioner of Bengal told the husband that unless a dangerous operation was performed, both the mother and child must die. It was referred to the mother, who replied, "Do not mind my life, but save that of my child." The operation was performed, and both recovered.

When the infant has been removed the *dyhe* generally introduces her hand into the uterus, seizes the placenta round its circumference, which she presses towards its centre, and it is thus detached and removed. When there is any difficulty in removing the secondines, the patient is supported so as to rest on her arms and knees, when it is removed by the hand of the *dyhe*. A quantity of oil is poured into the external parts, and she is placed on her left side, when the delivery is considered completed.

In consequence of the barbarous method of interfering with the natural labour, the infant is often born very weak, or in a state of asphyxia, when they chew a peppercorn, and put it in the mouth of the infant. In other cases besides this, they remove the placenta, strip the cord towards the body of the infant, and place the placenta in a new earthen vessel with water, which is placed on the fire until the water boils. The cord is then divided in the usual manner, and the infant placed in cotton. This peculiar custom has the advantage of retaining the infant near the fire, which will have a favourable tendency.

During labour, it is considered most dangerous to allow the female to drink water, or any cooling liquid. Should she be thirsty, a little warm milk may be given. After delivery, she is kept on her left side by the fire with her head towards it. This artificial and distressing heat is continued night and day during her period of uncleanness, as it is considered that the patient should be kept warm both outside and inside. On this account her diet consists of boiled rice, with highly spiced fish broth, and she is obliged to sleep, but to speak as little as possible during the first seven days after delivery. With the same intention she is rubbed over with warm oil and turmeric, and washed with warm water, to remove the pent-up blood, which is supposed to produce fever, etc. Different hot infusions are kept in readiness in certain druggists' shops, which are considered very efficacious for women after delivery. They are usually composed of a great variety of spices, such as pepper, cloves, cardamum seeds, ginger, etc. These are given occasionally during the first two days; in other cases warm water, ghee, and in less scrupulous families, wine or brandy are given after delivery, as by this means diseases are supposed to be prevented and cured. One of the most famous of these receipts now before me consists of twenty-five heating medicines, which are directed to be infused in water, and given occasionally warm.

By such a heating regimen the female is subject to floodings, tetanus, convulsions, puerperal fever, and rheumatism, which frequently occurs from their exposure during or soon after parturition, when the female is under the care

of the *dyke*, who is altogether unfit for the charge either of the mother or child. When assailed with sickness, this ignorant nurse attributes it to the patient's not having been sufficiently heated, and tries to increase this intolerable evil.

In all eastern countries, the female, after the expiration of the period of uncleanness, is obliged to observe certain customs respecting their purification, which are of a very old date; so that the Jews (see Leviticus xii. 4), like the Hindoos, were considered unclean for the same period, and were, like them, purified by washing and expiatory offerings, when they were restored to their family and caste.

Dr Wise's paper led to various remarks on the customs of other tribes in regard to the treatment of women during pregnancy and parturition.

(*Society's proceedings to be continued.*)

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING VI.—*January 28, 1854.*—Professor BENNETT, President, in the Chair.

DR THOMAS ANDERSON ON THE INFLUENCE OF BELLADONNA IN COUNTERACTING THE POISONOUS EFFECTS OF OPIUM.

Dr Graves had first suggested, that in continued fever, with protracted pupils and coma, if an agent administered internally would occasion dilatation of the pupils, it might also relieve the other symptoms of cerebral derangement. Dr Anderson, acting on this theory, administered large doses of belladonna in two cases of poisoning by opium, which he related as follows:—

A patient, of whom I had charge, and labouring under delirium tremens, having received an over-dose of a solution of the muriate of morphia, became comatose. He had taken, in thirty-six hours, two ounces of the solution of the muriate of morphia, and it had been continued by the attendant after sleep was procured. When I saw him he was in profound coma, his breathing was stertorous, amounting to no more than four or five per minute, and his pupils were contracted to mere points. His pulse was excessively weak, and rather slow; it was quite impossible to rouse him. I ordered him immediately the following mixture:—Tincture of belladonna six drachms, in five and a half ounces of water, of which an ounce was to be given every half hour. Three ounces of the mixture were administered with great caution, after which his pupils began to dilate. The six drachms of the tincture of belladonna were taken, and in four and a half hours after the first dose of it was given, the patient was in the following condition:—The coma was entirely gone, respirations were between twenty-two and twenty-five per minute, the pupils were much dilated, the pulse had risen to nearly one hundred and twenty in the minute, and was also increased in strength. His countenance, also, from being cold and pallid, had become much flushed, and the whole body was much warmer. He replied readily and coherently to all my questions. He continued to improve for three days after, when rising suddenly to stool, he fainted, and before the assistance of the nurse could be procured he was dead.

A fortnight afterwards a woman, about fifty years of age, took, at 4 o'clock P.M., two drachms of laudanum, and at half-past 5 P.M. three drachms more. She was brought to the Infirmary at 8 o'clock P.M. After making vain attempts to rouse her from the coma, by walking her about, etc., the stomach-pump was used at a quarter past 8 o'clock. By this means her stomach was thoroughly evacuated, but no trace of opium was detected by smell or sight. It had probably been all absorbed. A current of electricity was then applied to her hands for nearly ten minutes, but without rousing her. I saw her at a quarter to 9 P.M. for the first time, and on being told that she had been poisoned by laudanum, I determined to try the effects of belladonna.

At that time her pupils were contracted to mere points, her respiration was stertorous, ten per minute, the pulse was feeble, and the extremities rather cold. Between 9 and half-past 9, I gave her one ounce of tincture of belladonna in three ounces of water, which was all swallowed, but with difficulty. In the course of the next half hour two drachms more were administered. At 11 P.M. the first alteration on the size of the pupil was observed; the respirations had also then increased to twelve or thirteen in the minute, and the pulse was much stronger. The symptoms continued to improve till 2 A.M., when all indications of opium poisoning had disappeared. The woman was then sitting up in bed talking to the nurses, with pupils dilated to a little more than their natural size, and still slightly sensible to light. The extremities were quite warm, the pulse was about 100, and of good strength.

She gave me a coherent account of her motives for taking the poison, of the amount of money she had spent in purchasing the laudanum, and the names of the druggists where it had been procured. She also replied sensibly to questions about her family, and the age and occupations of her children. She continued awake till nearly 4 o'clock A.M., after which she slept till 9 A.M. In the morning I found her pretty well, her pupils being no more dilated than they were four hours after the first administration of the belladonna. She complained, however, of nausea, but unaccompanied with vomiting. This symptom, along with the dilated pupils, had entirely disappeared in the course of two days. She was kept in the hospital, under observation, for ten days after the accident, at the end of which time she was dismissed, perfectly well. The tincture of belladonna, used in both these cases, was of the strength of four ounces of the leaves to two pints of rectified spirit, and prepared by percolation. Half a drachm is considered a full dose. I have seen dilatation of the pupil produced by a drachm given at once.

The committee on Dr T. Anderson's communication reported, that they had designed to test Dr A.'s views by experiments on animals, but had found, on inquiry, that the animals commonly used for experiment were almost entirely insusceptible of the poisonous action of opium or belladonna. Where the effects were so different from those observed in man, it was obviously impossible to pursue the investigation which they had intended. The committee, therefore, while recognising that Dr A.'s views require more extended observations in order to confirm them, deemed them worthy of the attentive consideration of the Society.

Dr W. T. Gairdner read a communication on

THE CHEMISTRY OF THE LIVER, IN HEALTH AND DISEASE.

(This paper will be inserted in full in the next number of the Journal.)

Dr Bennett made remarks, in which he directed the attention of the Society to Virchow's recent discovery of a substance presenting the chemical reactions of cellulose in the corpora amylacea of the human brain, and spinal cord, and, more recently, in the Malpighian bodies of the spleen affected by the waxy degeneration.

Dr Chaumont observed that, by the addition of the elements of water to the formula of fibrine, it could be converted into the equivalents of cellulose, ammonia, and fat.

MEETING VII.—*February 11, 1854.*—Professor BENNETT, President, in the Chair.

The Committee on Dr Cartwright's communications reported, that after an impartial consideration of the subject, and after repeated experiments, the results of several of which had already been related to the Society, the Committee were of opinion, that the facts observed by them were decidedly opposed to Dr Cartwright's views of the heart's action, and were confirmatory of the doctrines usually received. At the same time, the Committee highly appreciated the ingenuity and research displayed in Dr C.'s communications and experiments.

ANATOMY OF THE GIRAFFE.

Dr Cobbold exhibited to the Society the cranium, ligamentum nuchæ, trachea tongue, and buccal apparatus, together with part of the spinal chord, the viscera, and organs of generation, removed from a male giraffe, which had recently died at Wombwell's menagerie.

From inquiry, it was ascertained that the animal in question had been in this country only seven months; it measured about fourteen feet in height, and was believed to be about two years old. Up to a few days before death, no symptoms of disease had showed themselves, and the creature was regarded as an exceedingly fat and healthy specimen. The first indications of debility appeared in the anterior extremities; this was attributed to cold by the keepers, the severity of the season, and the exposed situation of the caravans having told with unfavourable effect upon the collection generally. Friction, embrocations, and bandages, were applied, but the limbs gradually gave way, until complete paralysis supervened, and the animal gradually sank about eight days after the first symptoms of internal disturbance. The usual quantity of food was consumed until a very few hours before death.

On dissection, the paunch, reticulum, psalterium, and stomach, occupied the greater part of the abdominal cavity. The intestines being situated further backward, and somewhat behind. The paunch or rumen, in particular, was enormously distended with food, to which the great tumefaction of the abdomen observed before the dissection commenced, was principally due. Little or no fluid had accumulated in the peritoneal cavity, the omenta contained scarcely the smallest trace of fat, and the colon and cæcum were only moderately distended with fæces, which had assumed their characteristic pellet-like form.

The alimentary absorbing surface appeared healthy throughout. The spleen, apparently of the normal size, was very soft, and readily tore on being handled; the pulp (which was almost absolutely black), separating and running out like a thin fluid, leaving the matrix, consisting of the trabeculæ, arterial and venous trunks, quite bare in places. This disintegration, although the animal had been dead four days, was not considered to be due entirely to post-mortem changes. The pancreas was healthy, and in its usual position. The liver was placed far forward, and in contact with the diaphragm. It presented a very simple form, is elongated lengthways, compressed, and slightly fissured, so as faintly to indicate a bilobular character, presenting also a decided elevation or bulging from the under surface, corresponding to the lobus spigelii, the external border of the lobe being more particularly prominent. To the naked eye, and to the touch, the organ appeared healthy, but here and there occur small hard points, which will be further examined at leisure. No gall bladder exists.

On removing the abdominal viscera *en masse*, the tissues surrounding the left kidney were found in a semi-putrid condition; the substance of the renal organ was broken up, its consistence, like that of lard, having a dark-brown colour, and an intensely fetid odour; the capsule enclosing the corresponding viscus of the right side was more firm, but, on being opened, the contained gland manifested the same far advanced state of decomposition. The bladder was small, contracted, and empty.

The contents of the thorax were next removed. The lungs were collapsed, the left more completely so; they were soft, and of a very dark hue externally. These features were more especially observable in the left lung, which, on being cut up, was found much congested and gangrenous in places; softening and disintegration of some of the larger bronchi had also commenced, but in the lung of the right side, which was less congested, the appearances were altogether more natural, and the bronchi exhibited a healthy condition. The heart also was sound; its anatomical peculiarities in reference to the great disparity existing between the auricles and ventricles are very striking.

It may here be remarked in connection with the morbid lesions already

enumerated, that it appears rather surprising so great an amount of internal disease should have existed, and yet at the same time it had been supposed that the animal was in a very healthy condition. Dr Cobbold having informed one of the keepers concerning the results obtained by these post-mortem investigations, much surprise was expressed, especially at the statement made respecting the almost entire absence of fat throughout the body, an unusual circumstance in the Ruminants, and one which Dr C. took occasion in the present instance to point out as in itself a criterion sufficient to afford strong and presumptive evidence that the functions of nutrition and secretion had been for a long time carried on very imperfectly. The final giving way of the extremities, so speedily followed by death, finds its immediate explanation in the post-mortem appearances presented in the spinal chord, complete ramollissement of that portion of it corresponding to the neural foramen of the last cervical and first dorsal vertebræ having taken place. Throughout the remainder of the cerebro-spinal axis the chord was closely enveloped by the sheath of the dura mater, and both it and the brain were firm in texture.

The impoverished state of the nutritive functions was farther shown by the molar teeth, several of which are much decayed. The dental formula stands thus, the four posterior grinders, exposed by removal of a portion of the gum being included :—

$$\text{Incisors } \frac{0-0}{4-4}, \text{ molars } \frac{6-6}{6-6} : = 32.$$

The two outermost cutting teeth have divided crowns.

In the course of this lengthened verbal communication, Dr Cobbold dwelt upon the peculiarities of the tongue and buccal papillæ, and showed the rudimentary uvula to consist of three small horny papillæ, closely approximated; the notions entertained by Sir Everard Home, respecting the erectile character of the prehensile tongue, and the refutation which these ideas had sustained by the valuable researches of Professor Owen, were cursorily alluded to.

Dr Cobbold concluded by directing the attention of the Society to a demonstration under the microscopoe, of the fibrous tissue of the ligamentum nuchæ of this giraffe. Professor Quekett had described and figured the above-mentioned structure, as giving a transversely striated appearance; these striæ, when examined with a power of 500 diameters, indicate the existence of a series of openings (similar, it is presumed, to those found in the fenestrated arterial coat described by Henle) which do not extend across the entire diameter of the fibres! Dr C. stated that several gentlemen and himself had very carefully searched for these characteristic markings, according to the directions prescribed by Mr Quekett; but neither he nor they had succeeded in detecting the openings in question.

Dr Gairdner made a communication (accompanied by demonstration) on

PECULIAR CORPUSCLES FOUND IN THE BLOOD, SPLEEN, AND LIVER OF A CHOLERA
PATIENT,

which had been recently observed by Dr Cowan. (This paper was inserted in the last No. of the Journal.)

Drs Gairdner, Sanders, and Mr Kirk were appointed a committee to report on the subject.

ON THE PRESENT STATE OF OUR KNOWLEDGE OF CANCER.

Dr Bennett, at some length, reviewed the present state of our knowledge of cancerous and other morbid growths. He pointed out the discrepancies which existed among recent writers on this subject, and stated, that in his opinion our acquaintance with the structure and development of these growths was now so far advanced as to enable us to form some general principles which might guide scientific men in their further investigation. If pathologists could agree as to what they would call cancer, and as to what they would understand by the term malignant, much confusion would henceforth be avoided. He

proposed that a committee should be appointed to consider this matter, and to report at a future meeting. This was agreed to. Drs Bennett, Gairdner, Haldane, and de Chaumont to form the committee.

REACTION OF CELLULOSE IN THE SPLEEN.

Dr Sanders showed under the microscope a demonstration of a waxy spleen, in which the blue reaction characteristic of cellulose had been produced by the action of iodine and sulphuric acid. In this case, the waxy degeneration was of the diffuse kind, affecting the tissues of the spleen generally, and not the Malpighian bodies, so that the latter were not distinguishable, and the waxy material, which gave the blue colour, was distributed irregularly through the substance of the organ. In the spleens with waxy transparent Malpighian bodies, Dr S. had not hitherto succeeded in obtaining this reaction, first pointed out by Virchow. The conditions of the reaction require to be more accurately determined before its great value as a test, particularly of the waxy material, can be appreciated.

A dissecting microscope, made by Nachet, was shown to the Society by Professor Balfour.

MEETING VIII.—*February 25, 1854.*—Dr GAIRDNER, Vice-President, in the Chair.

SPECIMENS OF DISEASED EYES.

Mr Kirk exhibited to the Society two specimens of diseased eyes, for which he was indebted to Dr G. H. Pringle. The boy from whom they were taken was eighteen years of age, and had lost the sight of both during an early attack probably of purulent ophthalmia. Before the attack of fever, of which he died, he had been but just able to distinguish strong light from darkness.

The cornea of one eye was staphylomatous and quite opaque. On making a section from the entrance of the optic nerve to the front of the eye, the anterior chamber was found obliterated, the lens absent, and the iris thrown against and united with the posterior surface of the cornea. The retina had been separated from the choroid coat by a dropsical effusion of albuminous fluid; thus the retina and hyaloid membrane were suspended loosely in the posterior chamber, attached only at the entrance of the optic nerve, and at the junction of the cornea and sclerotic. The vitreous humour was completely disorganised, being thin and watery. The cornea of the other eye, with which he appreciated light, presented a dense central opacity, the circumference was nebulous. On section the anterior chamber was obliterated, the lens gone, the posterior chamber occupied by a bloody clot of a rusty colour. In the centre the retina was found compressed into a puckered membrane, running from back to front. There was no central cavity whatever; the vitreous body having been entirely absorbed. Anteriorly the retina was thrown against the iris, which adhered to the cornea.

Under the microscope the clot was found to consist of disintegrated blood globules, and of a very large number of granules. It seemed to have been a mixture of serous and bloody effusion. The elementary structures of the retina were but little changed, the hyaloid membrane might be removed from the internal surface of the corrugated retina, and consisted of delicate nucleated fibres.

Dr Gairdner remarked that he had recently met with a case of destruction of both eyes by purulent ophthalmia occurring after small-pox.

DR GRIEVE ON THE CHOLERA AT DUMFRIES.

To the Editor of the Monthly Journal of Medical Science.

SIR,—I perused with much interest Mr Blacklock's observations on the

origin of cholera in Dumfries, in the current number of your Journal. It is certainly to be regretted Mr Blacklock should have confined his remarks to the origin of cholera only, and that he has not favoured us with his sentiments on the nature, and the results of his treatment of it, the opinions of a gentleman of his standing and experience being entitled to respect and consideration. The importance of the subject, and a desire to contribute a few more impartial and "well ascertained facts," and to point out some errors connected with the sources from which Mr Blacklock has drawn his information, must be my apology for again trespassing on your kindness.

Mr Blacklock alleges that his information is derived from the minutes of the Local Board of Health of 1832 and 1848. These official documents, however, cannot be implicitly relied on, for so great were the excitement and panic prevailing here in 1832, and so inadequate was the medical staff to the requirements of the pestilence-stricken town, that many cases then occurred which were never reported to the said Board, while others, I am sorry to say, terminated fatally without ever having been seen by a medical man at all. Many other cases, I have reason to believe, were reported as cholera, which were merely cases of diarrhoea, and visited only once by their medical attendants! Mr Blacklock himself affords proofs of the inaccuracy and incompleteness of these minutes, in not being able to state the result of the fourth case (Hume's),¹ and in alleging that the fifth case (Callan's) occurred in the *Globe Close*, High Street. It so happened that I was the first called to see Callan, who was then in his workshop in *Herries' Close*, High Street, and finding him already in the collapsed stage, I requested the able assistance of my friend, Dr Charles Bell. This man lodged in Queensberry Street, some distance from High Street, and he was known to be labouring under choleraic diarrhoea when he went thither in the morning. Mr Blacklock then, in a foot-note, gives us another specimen of the questionable authenticity of these *official* records, when he states that, "about this time, *Branaghan, a waterman, was attacked, and died in Paddy's Close, English Street, but by some mistake was not recorded.*" The first five or six cases occurred in English Street, the four next in High Street, or, more correctly, in Queensberry Street (Callan's), and in St Michael Street, which is a considerable distance from English Street; after which, the disease became general over the whole town, upwards of two hundred cases occurring within the short period of fifteen days, viz., from 15th to 30th September. Surely this sudden rise and fearfully rapid propagation of the malady is more reconcileable with the operation of some generally diffused atmospheric poison or miasm, than with Sir Gilbert Blane's theory of communication by "emanations from the human body."

The official records of 1848 were even less trustworthy than those of 1832. Mr Blacklock states that the first case of cholera appeared in a confined close in the High Street on the 15th November, and the second case in the same house on the 18th. Now, my friend, Dr William Scott, authorizes me to state that he attended a decided case of cholera (Robert Hunter) in English Street on the afternoon of the 17th, and that the boy, Thomas Taylor, reported as having been attacked on the 18th in the same close as the first, was not taken ill till the 21st, and died early on the morning of the 22d, without being seen by a medical man. Dr Scott likewise informs me, that his patient in English Street, who happily recovered, had no communication whatever with the inmates of the "confined close in the High Street." The fact is, that, in the earlier stages of this epidemic, in consequence of an unfortunate misunderstanding which arose between the local authorities and the resident medical practitioners, no regular or systematic reports were supplied to the Board, until the arrival of Dr Sutherland, when a consistent plan of superintendence

¹ Hume's case was among the first, if not the very first, that recovered, and he is now, I am happy to say, alive and in good health. From notes taken at the time, I see I reported his attack on the 18th, and his recovery on the 27th September.

was organized, and the town divided into wards or districts, to which medical men were appointed.

I have no intention on the present occasion of entering upon the general question of contagion, but, from the same premises—assuming them to be correctly stated—from which Mr Blacklock deduces a contagious character for cholera, I am disposed to claim for it an epidemical character only, the phenomena being more accordant with, and more easily explicable on, the general principles of atmospheric influences, altogether unconnected with, and independent of, emanations from one person to another.

I take this opportunity of mentioning the singular and striking fact, that the town of Annan, with a population of nearly five thousand, situated nearly equidistant from Carlisle and Dumfries, between which places a constant communication was kept up *via* Annan, this town was directly in the main line of traffic, and was entirely exempt, though the disease prevailed alike in Dumfries and Carlisle, both in 1832 and 1848. Moreover, several of the inhabitants of Dumfries, who fled to Annan during both epidemics, were seized with cholera there immediately, or soon after their arrival, the presumption being, that the virus of cholera was lurking in their systems when they left Dumfries. These were undoubted cases of importation, and yet there was no extension, so far as I am aware, of the disease to the natives of Annan. I could here adduce, were it not encroaching on your valuable time and space, many other instances in the county of Dumfries, of infected persons having removed into healthy districts without any extension of the disease ensuing to persons with whom they had intercourse, but refrain from doing so, having already published several instances of that kind. Vide my *Practical Observations on Cholera, in the Medical Times*, 1849.—I am, etc.

JAMES GRIEVE, M.D.

Dumfries, 14th March 1854.

MEDICAL REFORM.

Since the publication of our last number, numerous meetings have been held in various parts of the kingdom, either to support or oppose the two bills now before the profession. Mr Brady, however, has vigorously pushed on his bill in the House of Commons, which was actually read for the second time on the 16th ultimo. The following is now the eighteenth clause of this measure:—

“ Clause XVIII. And whereas certain Graduates and Licentiates of British and Irish Universities, Fellows, Members, and Licentiates of the various Colleges of Surgeons in the United Kingdom, and Members and Licentiates of the Societies of Apothecaries of England and Ireland, practise or act as Physicians, Surgeons, or Apothecaries in different parts of the United Kingdom, not being Graduates of any or either of the Universities, Fellows, Members, or Licentiates of the Colleges aforesaid, or Members or Licentiates of the Societies of Apothecaries in that part of the United Kingdom wherein they reside and practise; be it therefore enacted and declared, that it shall be lawful for the registrars aforesaid, and they are hereby required on application for that purpose, to register all such Graduates of British and Irish Universities, Fellows, Members, and Licentiates of the Societies of Apothecaries aforesaid, in the register to be kept for that part of the United Kingdom in which he or they shall or may respectively reside and practise or act as a Physician, Surgeon, or Apothecary: *Provided, nevertheless, that nothing herein contained shall be deemed, taken, or construed to abridge or lessen any of the existing powers, privileges, rights, or remedies of the Universities, Colleges, and Halls enumerated in schedule (D) to this Act annexed.*”

This clause would positively be perfect, and would afford a just amount of relief from all the disqualifications of existing statutes, were it not for the few lines we have placed in italics. As it is, the bill is a mere mockery. These few lines, evidently introduced by the London boards, not only secure their present privileges, but, taken in conjunction with other clauses of the bill,

absolutely disqualify the graduates of the Universities of London, Edinburgh, Glasgow, Aberdeen, St Andrews, and Dublin from practising in England without the necessity of undergoing fresh examinations, and paying more fees. Dr O'Connor, who accompanied a deputation to Lord Palmerston, pointed out to the minister that there are in England 2500 graduates of universities in practice, and only 666 licentiates or fellows of the College of Physicians in London. In Scotland, there are 560 medical graduates of all kinds, and only 65 fellows of the College of Physicians of Edinburgh; while in Ireland, the various graduates in practice are 890, while the fellows and licentiates of the College of Physicians in Dublin amount only to 130. In each of these divisions of the kingdom, the majority of those practising as physicians would thus be disfranchised. To imagine that a bill which will produce such results can become law of the land is strange; and yet, unless certain measures be taken, and the importance of this matter impressed on members of parliament, it may, from ignorance of its effects, receive the sanction of the legislature.

Unfortunately the bill supported by Dr O'Connor and the members of the Association presents exactly the same fault. It is inconceivable to us that gentlemen who bring forward such strong and conclusive arguments against Mr Brady's bill, do not see that they apply with equal force to the measure which they themselves seem anxious to introduce. The following is clause twenty-two of the Association Bill:—

“Clause XXII.—Every person who shall be registered, and shall possess a certificate in force, according to the provisions of this act, shall be entitled to practise medicine *throughout that part of the United Kingdom for which his certificate was issued*; and every person who shall be registered in one part of the United Kingdom, may transfer his name to the register of any other part of the United Kingdom in which he may be about to practise, on production to the Registrar of the last named part of the United Kingdom of his certificate of the current year, *and of the diploma of the College of Physicians or the diploma of the College of Surgeons, of that part of the United Kingdom to the register of which he so transfers his name*; and the register shall THEREUPON grant to such person transferring his name a certificate, which shall remain in force till the first day of February then next ensuing.”

What difference is there, we should like to ask Dr O'Connor, between the disqualifying power of this clause, and that against which his arguments were directed in Mr Brady's bill? We cannot urge our objection to this measure in more forcible terms than has been done in Mr Syme's recent pamphlet, which we recommend to the consideration of so-called medical reformers. By this bill “an Edinburgh Doctor of Medicine intending to cross the border, and practise in Northumberland or Cumberland, after passing the various examinations, and performing all the exercises requisite for Graduation, would have to undergo the examinations of the new Board proposed to give the practical License, and then betake himself to London, again submit to trial by the College of Physicians or the College of Surgeons. How the Profession would be benefited by such a preposterous succession of examinations, with their corresponding accumulation of fees, it is not easy to imagine; and how such a system is compatible with the grand principle of equal privilege, founded upon equality of education, seems no less incomprehensible; since, according to it, there would be an impossibility, not only as at present, of qualifying in Scotland for general practice in England, but also of qualifying in England for practice in Scotland.”

In short, equality of privilege is and must be an essential element in every measure of medical reform. Whatever be the difficulties to be overcome in obtaining this end, they must be vanquished, or the result is no medical reform at all, and we are infinitely better as we are. That in a “United Kingdom” a London doctor shall not practise in Scotland, or a Scotch doctor in England, is one of the evils to be amended, instead of being rendered, as is done by both these bills, more stringent and intolerable.

THE MEDICAL SERVICE OF THE ARMY AND NAVY.

In a letter inserted in the "Times" newspaper, Mr Guthrie has pointed out the ineffective condition of the army service in the last war, as well as in the more recent campaigns in India. He says:—

"The British army in the south of France, in 1814, had everything that could be reasonably desired in the shape of medical assistance, and its able chief, Sir James M'Grigor, was not slow in recommending rewards, which the Duke of Wellington was pleased to approve. Every surgeon was consequently at his post; none were in the rear who could possibly avoid it; the duty was on every point well done, and the army was thoroughly efficient. During the first three-fifths of the same war everything was exactly the reverse. The sick were more numerous, and were, as well as the wounded, badly attended to, and the troops were consequently less efficient for their numbers. Few of the older surgeons ever wished to see the army or an enemy, if comfortable quarters could be found elsewhere; fewer did more than they could help, for there were no rewards—there was little or no promotion for good service. It was to be obtained only through interest with the medical gentlemen at home who clung to their own friends and their immediate patronage.

"The labour these officers have to undergo after a battle is not understood. If gentlemen in authority will be pleased to tie and untie their own shoestrings for half an hour without lifting their feet from the ground, they will comprehend the difficulty the surgeon will have to overcome, who is obliged to work in nearly a similar manner for the whole day and part of the night. Every badly-wounded soldier is lying on the ground; every manipulation to be gone through is more inconvenient than tying a shoestring, and human nature cannot sustain it beyond a reasonable point. That point passed, scientific surgery ceases to be attended to, and just as much is done, and no more can be done, than will satisfy the unfortunate soldiers that they are not neglected. They believe they are suffering from their own misfortunes, rather than from the parsimony of those who rule over their destinies. The real medical duty of the Peninsular war was done by men much under thirty years of age. A regimental surgeon past fifty can rarely bend his back, or kneel in the manner it is desirable a regimental surgeon should do; and an inspectorial officer much after sixty is almost always as inefficient before the enemy for everything like long-continued personal labour."

The following letter also gives an idea of what actually occurred in India:

"Ferozepore, Dec. 1845.

"I have just brought in the wounded of our regiment, 175 in number, to this station, where there is a commissariat, and some accommodation. I am here single-handed, the regiment being encamped about four miles off. The labour I have undergone has been excessive. I am one of four, three of whom are absent. One claimed the sanitary depot, a second was left at Kussowlee, a third is in camp with the regiment, and I am left here alone. How can I be expected to practice scientific surgery surrounded by 175 wounded men, all clamouring and beseeching for assistance? I have no time to do anything satisfactorily. I have, however, managed to do four amputations to-day, and dressed the greater number of serious cases, including two amputations I brought off the field, and am quite weary of the bloody work. Instead of seventy-five doolies or litters to which our regiment, 750 strong, was entitled on the evening of the 16th, when we began our march, the commissariat could only supply twenty-four, each being short of two bearers; the others soon began to drop off, owing to the severity of the work, so that by the time we reached Moodkee only twelve doolies remained for service. There was no medical chief appointed that I heard of. It was, perhaps, thought the fighting would soon be over, and the wounded but few. If so, we reckoned without our host. We had in the field with us, according to your directions, a quantity

of water, some brandy, wax candles, and a fair proportion of medical and surgical stores, and followed close in the rear of the regiment till the men began to drop around us, chiefly with horrible wounds from cannon-shot, which the enemy fired with wonderful rapidity. We halted under a tree, the only shelter to be had, the country being a sandy plain, as level as a table, and the hospital soon became a dreadful scene of mangled bodies. The men of the band brought in the wounded, and we were getting on well until a great misfortune befel us. I do not know, nor can I believe, that ours was the only field hospital of the division, but certain it is, we were soon besieged by wounded from every corps. Cavalry and artillery came galloping in and carried off our doolies. When it was discovered we had water, there was an end to all order; we could only save one small vesselful by placing it in charge of a sentry with a loaded musket. When it became dark we lighted our candles, but had scarcely done so when an order came to put them out, or the Sikhs would be down upon us. Shortly afterwards we were ordered to join the regiment; but in the dark no one could tell where it was. It was said to be on our right, but by sad mischances we wandered down to the Sikh camp, when the enemy began to fire upon us. We all made off as fast as we could, the doolie bearers throwing down their loads, which ended our hospital establishment. We could not, with our deficient means, carry off all the wounded from our field hospital, and we saw many poor fellows lying near the entrenchments we could not remove." (They were all murdered in cold blood).

We give another extract from this admirable letter :—

"The medical staff present during the first week at Toulouse consisted of the inspector-general, two deputy inspectors, eight staff-surgeons, two apothecaries, eighteen assistant staff-surgeons, and twenty-three regimental assistant-surgeons, but not one regimental surgeon, they having all remained with their regiments. This number was augmented in the second week by two staff-surgeons, seven assistant staff-surgeons, one regimental assistant-surgeon, and four apothecaries. The ten staff-surgeons are dead; but there is not one of the forty-nine assistant-surgeons now living who will not declare that from six in the morning until nine at night he laboured as hard as he had ever done before.

"The number of wounded of the royal army in the four great battles in India, amounted to three times that at Toulouse; the proportionate number of men shot through the chest and sent to England would be by computation 171, instead of 9; a difference which can only be accounted for by the fact that the inspector, the two deputy inspectors, the eight staff-surgeons, and the eighteen assistant staff-surgeons, were none of them to be found with the army on the Sutlej. The regimental medical staff were totally incapable of doing the duty required of them, and the medical department of the Indian army was even more inefficient in point of numbers. The same Herculean efforts were made by the Governor-General and the Commander-in-Chief to procure doctors as after the battle of Waterloo, and with a similar result—they came too late.

"When Charles VI. of France sent to Spain, in aid of Pedro the Cruel, the renowned warrior Bertrand de Guesclin, at the head of the *reiters*, the *lansquenets*, the free *compagnons* of his army, from whose prowess he had derived the greatest advantages, and heard in due time that their bones were whitening the fields of Castile and Navarre, he drew consolation from the fact that they could not return, and would not be able to claim the rewards, the pensions, he had promised, and to which their services entitled them. After a lapse of three centuries, no better consolation can be afforded to the people of Great Britain under a similar but more severe infliction.

"The royal army of Great Britain is not composed of mercenaries. Its soldiers are the blood, the bone, the sinew of the nation, on whose indomitable valour alone can dependence be placed in the hour of danger. By them the victory must be won; by them the loss must be sustained; and a country

grateful for their services should watch over them in their necessities, as a mother over her children."

It is now generally understood that this appeal has not been useless, and that great exertions have been made by the authorities to increase the number of the medical staff. Three assistant surgeons are at least necessary for every regiment about to enter into battle, besides a proportionate augmentation of the superior officers, whose services may be concentrated at the point of action.

It is also gratifying to know that the incessant arguments and exertions of Sir George Ballingall, on the necessity of erecting professorships of military surgery, are at length likely to be gratified, and that Edinburgh will no longer be the only school in this country where surgery in its relations to military matters is to be taught. On this subject we have only to warn the authorities against carrying out the proposition of forming one central school at Chatham or in London; for, in the words of Sir George, "Let such a school be made as great, and carried as far as first-rate talent and favouring gales can carry it; but let not Scotland and Ireland, which have taken the start in this matter, be deprived of the opportunity of qualifying students to become candidates for admission into the public service. Nor let the medical officers of the army and the fleet (of whom a vast proportion have ever been educated in Edinburgh and in Dublin), when returning home from foreign service, with broken health and valuable experience, be compelled to resort to London for a course of military surgery."

With regard to the navy, no ameliorations have been introduced, and the assistant-surgeons are still obliged to mingle with midshipmen, dress upon their own sea chest, and have no cabin or place where they can consult a book, or keep up their professional knowledge. While an assistant-surgeon in the army dines at the officers' mess, and mingles with them on a footing of equality, he of the navy is condemned to the cock-pit, to the companionship of the youngers, and is altogether regarded as an inferior. It cannot be denied that a man who can get into the army will never think of joining the navy, and that in consequence the medical service of the latter must be deteriorated. If advancing the status of the army surgeon be a "soldier's question," improvement in the condition of the navy surgeon is still more decidedly a sailor's question.

THE ROYAL MEDICO-CHIRURGICAL SOCIETY AND THE "LANCET."

To the Editor of the Monthly Journal of Medical Science.

Sir,—At the last General Meeting of the Fellows of the Medico-Chirurgical Society of London, a resolution was passed, which contained a recommendation to the Council to consider the propriety of excluding the "Lancet" from the Reading-room of the Society; the resolution was carried by 46 votes against 19. Such act of excluding a periodical is one of great importance, and of general interest to the profession. As an Edinburgh graduate and subscriber to your Journal, I am anxious that the grounds upon which that recommendation was based should be briefly placed before your readers, the more so, as untrue motives have been attributed to those who supported it.

It was felt that the influence exercised over the medical profession by a certain class of the periodical press of this country was very great, and that it was frequently so used as neither to exalt nor sustain the honour and dignity of the profession; that in many instances, it had been shamefully perverted to the fostering of personal interests, and to the sacrifice of the general good; that private resentments and animosities had been gratified, right perverted, and editorial responsibility entirely forgotten; that instead of guiding and leading the profession onward in the path of high honour and honesty, it had rather made itself subservient to the growth of those evil tendencies already too widely spread amongst its members; that instead of correcting those evil tendencies it had participated in them; being instead of a great teacher of duty, a fellow-actor in the commission of such evils; and that thus it was both destroy-

ing the character of the profession, and seriously damaging it in the eyes of the general public, who think that these journals reflect the condition of medicine and its practitioners.

All this was felt, and a body of men were determined to make the attempt at least of withstanding what they deemed this powerful current of evil, and if possible to arrest its course; and they thought no better way of testing the feeling of the profession in this matter could be found, than by bringing the subject fairly before the General Annual Meeting of the above Society.

Be it then clearly understood, that the resolution before-mentioned was based entirely on *moral grounds*. It was wished that the profession should thereby know, that a large body at least, of the first Medical Society of the Kingdom was determined no longer silently to permit such perversions of great power to bad purposes; and it was hoped that results might ensue, which would produce a more healthy condition than has hitherto reigned in the constitution of our profession.

The "Lancet" has stigmatised the effect as "dastardly;" but it is manifestly a very bold one, to submit to the abuse of the press in support of what many may call Quixotic ideas. It has called the 46 gentlemen who voted for the resolution "conspirators;" but the conspiracy is simply a combination against the intolerable abuses of a powerful engine. Reference to the names of these "conspirators" shows that they are those of the men—almost all of them connected with the public hospitals of this metropolis—who will, hereafter (according to all human calculations) occupy the positions now held by the highest in our profession. Yet according to the "Lancet" "no man of any repute, excepting Sir J. Forbes, voted in the majority." Again, this gentleman, and Dr Cormack (who did not vote) are "assassins to their calling." Such are specimens of the honest mode of criticism and of abuse practised by this press which we condemn.

Objectors to our views said—"after all, "the press is *generally* bad;" but surely it is a novel justice, to refuse to punish one criminal, because others have sinned likewise. And again, "the press is only the mirror of the profession and reflects its bad habits," to which we answer that there is a powerful body of the rising men of the profession whose opinions and feelings it does *not* represent, and who are determined no longer to submit to the tyranny which has so long hung over it.

We think, and I trust the sentiment will find an echo on your side of the Tweed, that this demonstration of feeling indicates that brighter days are in store for the profession; that that moral energy and strength (which neither legislation nor bye-laws of colleges can establish or create), and which have so long lain dormant, are at length fairly aroused. The lesson taught the "Lancet" will assuredly not be lost wherever else a similar lesson may be required.—Your obedient servant,

M.D., EDIN.

London, March 25, 1854.

[Whilst we approve of the sentiments so well expressed by our correspondent, it should be understood that the "Lancet" is not the only journal in fault. In fact the success of that periodical has called imitators into existence, who have copied its tone but not its talent. Invective has in consequence become coarser and coarser, and has at length given rise to a desire of retaliation, when the original offender was naturally the first object of attack. In London, also, the injustice of Mr Gay's expulsion from the Free Hospital, which was defended by the "Lancet," acted as an excitor of, as well as a director to, the pent-up indignation of the respectable members of the profession. In Edinburgh a similar outbreak would be directed against the "Medical Times and Gazette." That journal, under the head of "Medical Gossip, or Doings in the North," has circulated the most unfounded calumnies and malicious reports of the members of the profession in this city. Already a consi-

derable amount of ill feeling and acrimony have been engendered in consequence, and the languid condition of our Medico-Chirurgical Society this session exhibits the disinclination which some of its members experience to be misrepresented and ridiculed in the pages of the London weekly press. This system of anonymous personality and vulgar abuse, while it causes the truly eminent men to withhold from our society verbal and written communications, in order to escape insult, offers every facility to the ignorant and unscrupulous writer to puff himself and his friends into notice. As, therefore, even the metropolis does not seem large enough to furnish the weekly quantum of scandal and ill nature for the London weeklies, we are personally interested, and hail the honest endeavours of the young hospital physicians and surgeons to check this enormous evil, as a great good. Numerous societies, book-clubs, and individuals, have already given up their subscriptions to the "Lancet." How the agitation will end we do not know, although we feel satisfied that ultimate advantage will arise from it. In the meantime, the "Lancet" is endeavouring, by bravado, to terrify its opponents, who are "conspirators" and "assassins." The cry of "independence of the press" has been raised on one side, "of liberty not licentiousness" on the other. The Medico-Chirurgical Society, on Friday the 24th ult., saw both parties assembled in great force, with a view to a pitched battle and trial of strength; but, strange to say, actual collision was prevented by an attempt at diplomacy, and the subject is remitted to the Council. In short, this war against the "Lancet" holds the medical world in suspense. The Russian war or the rebellion in China becomes insignificant compared with it. It will be our office from time to time to report the events and progress of the campaign.]

THE CHOLERA.

THE cholera has again made its appearance in several parts of England, Scotland, Ireland, and France; and though much has been done in the way of purification and hygienic regulations, still more remains to be accomplished. We extract the following from a leading article of the "Times" for March 13th:—"Have any of our fashionable or fairer readers ever heard of Dunk Street? It is a locality quite different in appearance from Belgrave Square. Dunk Street is in Whitechapel—an agreeable spot, teeming with foulness of every description—every kind of matter that can offend every sense. When the cholera is in London, an inhabitant of Dunk Street is in about as secure a position as a lady or gentleman upon a trap-door with Calcraft's hand upon the bolt, and the chaplain well on with the burial service. We don't recommend anybody to take lodgings in Dunk Street next July or August.—But we need not go so far as Whitechapel. Kensington may be considered one of our most fashionable quarters, and, towards the parks, no doubt it keeps up a fair appearance. Kensington, however, by the reports of the inspectors, would appear to be something like the belle of the season with a cancer, which she keeps from the knowledge of her admirers. In the Notting Hill districts of Kensington parish there is a locality called 'the Potteries,' where the state of the roads is as bad as the pigsties themselves. Again, at Merthyr Tydvil, says Mr Holland, the inspector, 'I saw a young woman filling her pitcher from a little stream of water gushing from a cinder heap, the surface of which was studded with alvine deposits—and the rain then falling was washing the feculent matter into the pitcher, no doubt for domestic use.' Despite all we have said of the filth and abomination of the localities named, Scotland would appear, from the extracts from the reports of Lord Shaftesbury, to enjoy a pre-eminence in the grandeur of dirt, stink, and muck. If a little of the energy expended by the Scottish Lion and Unicorn Society upon the adornment and blazon of their national coat-of-arms could for the next six months be transferred to the purification of their great towns, the result might afford matter for congratulation."

VARIETIES.

THE SENATUS OF THE UNIVERSITY *versus* THE TOWN COUNCIL OF EDINBURGH.—This important appeal is now before the House of Lords. Its object is to determine whether the right of regulating the curricula for the learned professions belongs to the Senatus or the Town Council; in short, whether academic honours shall flow from a university, or from the worthy bailies and burgesses of the city. A little incident occurred, which strongly points out the mere business view of the matter taken up by the Council for the town. He said "he had before him a minute of the Town Council 1766, which stated that they had accepted the resignation of Dr Cullen, and had appointed a successor, whom they had bound over to observe all the acts and bye-laws made, or to be made by them, touching the government and administration of the College. The successor alluded to seemed to have been one Dr Joseph Black, late physician in Glasgow." Lord Brougham—"One Dr Black! why, he was the very greatest of all the Professors that ever appeared in the University—beyond all question. Dr Black was the founder of modern chemistry. Dr Cullen was also a very great man."—*Scotsman*.

PHYSIC AND MUSIC.—Every one knows that Esculapius was the son of Apollo, and this relationship accounts for the frequency with which we find good physicians to be good musicians also. Our annals are rich in distinguished doctors, who have had excellent voices, or who played with skill on various instruments. Boerhaave was a capital performer on the flute; and it is a curious coincidence that the late great toxicologist of France, and the eminent living toxicologist of Great Britain, should have both possessed splendid bass voices. The editor of the *Union Medicale* comments on the number of musical confrères he meets with in the salons of Paris (Janvier 14): and we can assure him that in Edinburgh the same thing is observable. Indeed, most of our public professional dinners and entertainments would now be considered dull affairs, without the songs and glees with which they are enlivened by the Medical Glee Club of this city.

ADULTERATION OF MILK.—There is consumed daily in Paris about 300,000 litres of milk, which pass through the hands—1st, of the agriculturists; 2d, of the gleaner or collector; and, 3d, of the retailer. It has been proved by calculation that the quantity produced by the agriculturists, in going through the hands of the gleaner and retailer, is augmented by one-third before it arrives at the consuming public. This is accomplished principally by the addition of water, with a little sugar and white of egg.—*Gaz. des Hospitaux*.

TRIAL OF MR SMITH FOR MURDER.—On the 13th ultimo, Mr Smith of St Fergus, Aberdeenshire, was placed at the bar of the High Court of Justiciary, charged with the crime of murder. A very few of the witnesses having been examined during the first day, the court adjourned, and the jury, as usual, were accommodated in an hotel, under the surveillance of an officer. During the night, however, one of the jury became ill, and his mental powers were so affected, as to disqualify him from discharging any farther duty. A new trial is to take place. We are happy to state that this Mr Smith, though he has been practising for some time as a medical man, and has been considered in his district as a respectable person, is not, in fact, a member of the profession. He is one of those whom a registration would have exposed, as he has no diploma whatever.

DURATION OF LIFE AMONG MEDICAL MEN.—From an investigation into the subject by Dr Guy, he draws the following conclusions:—1. That the duration of life is greater among physicians and surgeons than among the general practitioners of medicine and surgery. 2. That this greater longevity of physicians and surgeons is only, in part, explained by a less amount of exposure to

contagious diseases, and other professional risks. 3. That the duration of life of members of the medical profession, does not differ materially from the duration of life of the clergy. 4. That the duration of life of medical men has somewhat increased during the last three centuries.—*Statistical Journal*.

ARTIFICIAL MULTIPLICATION OF FISH.—The care now bestowed on the artificial multiplication of fish in France, has, according to a recent report made by M. Coste, to the Academy of Sciences, not only succeeded in greatly increasing their number, and diminishing their price as food, but in introducing new and valuable kinds into the lakes and rivers of that country. In this manner have been introduced the salmon of the Danube (*salmo nucho*, Lin.); *salmo umbla*, Lin.); *corigonus fera*, Jur.; and in the lake of Ballon (*Vosges*), the large trout of Switzerland (*sal. lemanus*, Cuv.)—*L'Union Med.*

DEVELOPMENT OF INTESTINAL WORMS.—Mr Leuckart of Dresden fed white mice in two separate cages for a long time, and ascertained that they were in no way affected with animal parasites. He then mingled with the food eggs taken from the *Tœnia crassicollis*, which was given to the animals in one cage. These shortly became infested with the *cœnuria*, whilst those in the other cage remained free from them—proving that the development of the egg of the flat worm was the real cause of the presence of the cystic worm.—*Gaz. des Hospitaux*, Feb. 23d.

MULTIPLICATION OF THE LEECH.—An establishment has been founded in the commune of Clairefontaine, near Paris, by M. Bonne, for the reproduction and nourishment of leeches. By watching the habits of these animals, he has at length succeeded in producing them, in great numbers, for the drug markets. They must be from two to three years old before they are useful for medical purposes. They are kept by M. Bonne in a marsh, and collected three times a year for the purpose of being fed on the blood of calves.—*Bulletin de l'Academie*.

ENGLISH LAW, AND DISPENSING POISONS.—A woman in Dudley, Worcestershire, sent to a druggist's shop for an emetic. A boy of sixteen years of age gave her a powder of *nux vomica*, which killed the woman a few hours after she had taken it. At the coroner's inquest, the jury found a verdict of "accidental death," at the same time censuring the druggist for allowing inexperienced persons to vend poisons, and *expressing their regret that they could not punish him*.

NEW DOMESTIC ANIMALS.—Attempts are being made in France to acclimatize the buffalo, the camel, and the lama. The last-named animal is capable of furnishing the most useful products—such as milk, meat, and the finest wool, and, is in addition, a beast of burden.—*L'Union Medicale*. Feb. 16.

PUBLICATIONS RECEIVED.

Class Book of Botany, being an Introduction to the study of the Vegetable Kingdom. By J. H. Balfour, M.D., F.R.S.E., Professor of Medicine and Botany in the University of Edinburgh, etc. With upwards of 1800 illustrations. Edinburgh. 8vo. 1854. Pp. 1114.

A Clinical Introduction to the practice of Auscultation, and other modes of Physical Diagnosis, in diseases of the Lungs and Heart. By H. M. Hughes, M.D., Assistant Physician to Guy's Hospital, etc. Second Edition. London. Small 8vo. 1854. Pp. 302.

Physiology in Harmony with the Bible, respecting the value and right observance of the Sabbath. By James Miller, F.R.S.E.,

etc. Small 8vo. Edinburgh. 1854. Pp. 78.

Practical Observations on Gout and its Complications, and on the Treatment of Joints stiffened by Gouty Deposits. By T. Spencer Wells, F.R.C.S., etc. Small 8vo. London. 1854. Pp. 288.

The Varieties of Pock Delineated and Described. By Walter Cooper Dudgeon. Small 8vo. London. 1853. Pp. 31.

The Question considered—Is it justifiable to administer Chloroform in surgical operations, after its having already proved suddenly fatal in upwards of fifty cases, when pain can be safely prevented, without loss of consciousness, by momentary benumbing cold? By James Arnott, M.D., late

Superintendent of the Medical Establishment at St Helena. 8vo. London, 1854. Pp. 31.
 Abstract of a Clinical Lecture on Abdominal Tympany. By Dr O. Ferrall. Reprint.
 An Appeal on the Eastern Question to the Senatus Academicus of the Royal College of Edinburgh. By a Russian. 8vo. Edinburgh. 1854. Pp. 24.

Dr Conquest's Outlines of Midwifery, intended as a text-book for Students, etc. A new Edition. By James M. Winn, M.D., etc. With numerous illustrations on wood by Bagg. 12mo. London. 1854. Pp. 323.
 Letter to Lord Viscount Palmerston on Medical Reform. By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh. 8vo. Edinburgh. 1854. Pp. 11.

OUR EXCHANGES.

We have revised our list, and must decline continuing the exchange with several periodicals which, from the nature of their speciality, their medical inferiority, or irregularity in transmission, are of no use to us as journalists.

British and Foreign Medico-Chirurgical Review. Received regularly.
 Edinburgh Medical and Surgical Journal. Exchange accepted with the new series.
 Pharmaceutical Journal. Received regularly.
 Quarterly Journal of Microscopical Science. Received regularly.

Psychological Journal. Received regularly.
 Medical Times and Gazette. Received regularly.
 Association Medical Journal. Received regularly.
 Dublin Medical Press. Received regularly.
 Dublin Hospital Gazette. Exchange accepted.

Our American exchanges are received very late, and, generally speaking, long after we find them on the library tables of our medical institutions. We beg particularly to direct the attention of the editors and publishers to this fact, and hope they will take care to address to the MONTHLY Medical Journal, Edinburgh, 23, George Street.

The American Journal of the Medical Sciences. The January No. has just come to hand.
 The American Medical Monthly. Exchange accepted. Nos. I. and II. have been received, and we have requested our publishers to forward all the numbers of the Monthly for the present year.

The New York Journal of Medicine. [Last No. received for January 1854.
 The Philadelphia Medical Examiner. Last No. received for January 1854.
 The Boston Medical and Surgical Journal. Received very irregularly. Last No. received is for October 1853.

With several other American Journals which come to hand in a very straggling manner, we must decline to continue the exchange.

Archives Générales de Médecine. Received very irregularly.
 Journal de Médecine et de Chirurgie Pratique. Received regularly.
 Revue Médico-Chirurgicale. Ditto.
 Bulletin Général de Thérapeutique. Do.
 Gazette Médicale de Paris. Do.
 L'Union Médicale. Do.
 Gazette des Hôpitaux. Do.
 Vierteljahrschrift für die Practische Heilkunde. Received regularly.
 Zeitschrift der k. k. Gesellschaft zu Wien. Regular.
 Hygiea. Irregular.

Journal für Kinderkrankheiten von Behrend und Hildebrand. Regular.
 Zeitschrift für Rationelle Medicin, von Henle und Pfeufer. Ditto.
 Verhandlungen der Physikalisch-Medicinischen Gesellschaft in Würzburg. Ditto.
 Casper's Vierteljahrschrift für Gerichtliche Oeffentliche Medicin. Ditto.
 Nederlandsch Weekblad voor Geneeskundigen. Ditto.
 Bibliothek for Læger. Irregular.
 Nederlandsch Lancet. Regular.
 Henke's Zeitschrift für die Staatsarzneikunde. Ditto.

We beg our foreign correspondents, and the Editors of foreign journals, to address their communications for this Journal, to the care of Messrs Williams and Norgate, foreign booksellers, Henrietta Street, Covent Garden, London. No parcels sent by post can be received, unless the carriage be prepaid.

Dr Merriman's paper next month.

Erratum.—In Mr Spence's 1st case recorded in the last No. it should have been said that the operation was performed on the 4th of March.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On some Points in the Pathology of the Liver.* By W. T. GAIRDNER, M.D. With Seven Analyses by Dr JAMES DRUMMOND.

(*Read to the Physiological Society of Edinburgh, January 28th, 1854.*)

THE subject of the waxy degeneration of organs having been recently before the Society, I am induced, as a sequel to the observations presented on that and other occasions as to the pathology of the liver, to bring forward in the present paper the few chemical facts which I was enabled to collect during the period of my service in the department of morbid anatomy in the Royal Infirmary of Edinburgh. Though these observations are neither sufficiently numerous, nor, perhaps, sufficiently complete individually, to satisfy the chemist accustomed to the great precision of many modern analytical inquiries, yet I believe they are calculated to be useful in guiding the views of those whose skill in pathological investigations is such as to enable them to pursue the subject by further researches. At all events, the name of our esteemed colleague, Dr Drummond, who made the analyses which I am about to detail, will be to the Physiological Society a guarantee that they are correct so far as they go. For the conclusions derived from them as to the chemical pathology of the liver, I am alone responsible.

In the specimens of diseased liver of which Dr Drummond undertook the analysis, I requested him to ascertain, 1st, the amount of substances converted into vapour at a heat of 212° ; 2d, of *non-vaporizable elements*; 3d, of *fatty matter* soluble in ether. Portions of the liver, carefully selected so as to afford fair specimens of the general glandular substance, were in all cases the subjects of analysis; and careful anatomical examinations were at the same time made and recorded by Dr Drummond, as well as in most instances by myself. By circumscribing as much as possible the labour of the individual analyses, I had hoped to procure a larger series for comparison; but owing to the very numerous duties which devolved upon Dr Drummond as well as on myself, I find it necessary to reject several, where from one cause or other the data are not quite satisfactory. There remain seven analyses, two of which refer to

specimens of extreme fatty disease; two to well-marked, though not quite extreme, cases of waxy degeneration, as I have described it to the Society (see February number of *Monthly Journal*, p. 186); and three to instances of cirrhosis or hob-nail liver, under more than one of the different forms in which that disease is observed.

TABLE I.—ANALYSES OF LIVER.

	Water (p. c.)	Fatty Matters by Ether (p.c.)	Solid Residue after Fat Ex- tracted (p. c.)	Proportion of Solids (exclud- ing fat) to Wa- ter (p. c.)	Proportion of Fat to Solid Tissues (p. c.)	Probable Amount of Fat in entire Liver.
L	76·17	2·50	21·33	28·0	11·7	600 grs.
II.	76·39	1·77	21·84	28·6	8·1	450 grs.
III.	68·58	3·82	27·60	40·2	13·9	900 grs.
IV.	72·05	4·28	23·47	32·5	18·2	1000 grs.
V.	53·7	32·3	14·0	26·0	230·7	Between 1 and 2 lbs. avoirdupois, weight of liver being 5 lbs.
VI.	50·3	34·5	15·2	30·1	226·9	
VII.	62·5	3·2	34·3	54·8	9·3	1300 grs.
VIII.	64·2	4·8	31·0	48·3	15·8	1800 grs.
IX	73·2	13·6	13·2	18·0	103·0	?
X.	73·5	5·4	21·1	28·7	25·5	2400 grs.
XI.	66·4	8·8	24·8	37·3	36·	1200 grs.

- I. Healthy (Von Bibra¹). No particulars of liver. Sudden death from fall.
- II. Healthy (Boudet²). No particulars.
- III. Healthy (Beale³). Sudden death by fall while in perfect health. Male, æt. 31.
- IV. Do. Female, æt 40. Death from apoplexy.
- V. Fatty (Drummond⁴). Exquisite case of fatty liver, enlarged.
- VI. Fatty (Drummond). Do. do. Sp. gr. 1005.
- VII. Waxy (Drummond). Waxy, rather congested. Weighed 5 lbs. 6½ oz.
- VIII. Waxy (Drummond). Do. do. Sp. gr. 1055. Weight 5 lbs.
Neither case presents the most advanced and heavy type of waxy liver.
- IX. Cirrhosis (Drummond). Marked case. No particulars.
- X. Cirrhosis (Drummond). Do. do. Enlarged; 5½ lbs.
- XI. Do. (incip. atrophy) (Drummond). Not far advanced, atrophic form; weighed 2 lbs.

¹ Von Bibra. *Chemische Fragmente über der Leber und die Galle*, p. 164.
² Simon's *Animal Chemistry*. Sydenham Society's Translation, vol. ii. p. 429.
³ *British and Foreign Med. Chirurg. Review*, July 1853, p. 227.
⁴ Nos. 5 *et seq.* are unpublished Analyses, by Dr James Drummond.

As we found it by no means easy to procure specimens of human liver which fulfilled all the conditions of health, *i. e.*, which were procured from persons dying in full health, and presented both to the naked eye and the microscope, perfectly normal appearances, we have not any analyses on which we can absolutely rely as standards. Indeed there is only one way of procuring such standard or test-examples of healthy human liver; *viz.*, by a selection, after careful examination, from among the organs of persons dying by violence, and ascertained to be free from every description of organic disease. The unaided eye will, as every one accustomed to these investigations knows, often fail to detect an amount of fat in the organ which must be considered as decidedly abnormal, although not, perhaps, amounting to a serious disease of structure; and persons who, from habitual intoxication, or generally dissipated habits, are especially liable to violent death, are rarely free from these slighter forms of deranged health, even when they are to all appearance robust and free from positive disease, as manifested by symptoms.

In the absence of sufficient and satisfactory evidence as to the chemical constitution of the perfectly normal liver, I have placed in comparison with the analyses made by Dr Drummond four others, which have been already published, as representing, in the opinion of chemists and physiologists of high character, the constitution of this organ when unaffected by any apparent disease. Two of these analyses were in cases of violent death; one was an apparently healthy liver in a case of death by apoplexy; and of the remaining case there is no information. Judging from the other facts before me, I should think that in two of them at least, if not in three, the quantity of fatty matter was in excess, as compared with the strictly normal condition; while it seems to me very probable that in one of them (III.) the liver approached more or less distinctly to the waxy condition.

Remarks.—In regard to the construction of the table, it seems only necessary to remark, that the first column, "Water," represents the weight lost by evaporation at 212° ; that from the residuary weight that of the matters extracted by ether is deducted, by which means the second and third columns in the table are ascertained. The fourth and fifth columns are calculated directly from the preceding, on principles which require no explanation. The last column is an attempt to estimate the quantity of fatty matter in the entire liver in some of the cases; but as the weight of the organ has not been always stated, this calculation is necessarily in part hypothetical. In the livers noted as healthy (I. to IV.) the weight is assumed as the normal average, or about $3\frac{1}{2}$ lbs. In the two fatty livers, which were both much enlarged, but unfortunately were not weighed, 5 lbs. is assumed as a not improbable weight. In the waxy livers the weight was ascertained, being in neither case extreme; in one,

5 lbs. 6½ oz., in the other 5 lbs. The calculation is made for two only of the cases of cirrhosis, in one of which the liver was enlarged, and weighed 5½ lbs.; in the other it was diminished in volume, and weighed 2 lbs. only.

From these data, it appears to me that we may, at least provisionally, deduce some results of importance, to be further tested by future observations.

1. *As to the Water.*—This ingredient abounds more in the healthy liver than in any of the well-known forms of chronic degeneration. Its probable fluctuations lie between 70 and 80 per cent., and any considerable fall below the former figure probably indicates a tendency towards either the waxy or the fatty condition of the liver. There can be little doubt that the amount of the water depends to a considerable extent on that of the blood or serum in the vessels, or among the cells of the glandular structure; an excessive percentage of water is therefore less significant of hepatic disease than the opposite condition.

In the *fatty liver*, the *percentage* of water is lower than under any other form of disease, being in one case as low as fifty per cent. By consulting the other columns of the table, however, it will be found that this apparent decrement is entirely due to the great excess of oil, which in this disease is infiltrated into the glandular structure in such quantity as to affect the percentage of all the other chemical constituents. It appears from the fourth column that the proportion of the water to the solid elements remains unaffected in the fatty liver, and it is therefore probable that neither the one nor the other is necessarily disturbed in this form of disease, otherwise than by the super-addition of oily matter.

In the *waxy liver*, on the other hand, the water is materially decreased in quantity, both relatively to the whole weight, and to that of the solids.

In *cirrhosis* the proportion of water is extremely variable, and cannot be reduced to any general expression.

2. *As to the Solids.*—After the fatty matter, and other extractives removed by ether have been deducted, the remaining solids consist, for the most part, of the glandular elements (epithelium-cells and nuclei), fibres, vessels, and the solids of the blood in the organ. The solid residue therefore is a rather complex quantity, but one which it is not easy to devise a method of separating accurately into its component parts. Nevertheless, the study of the table under this aspect gives some instructive particulars.

In the *fatty liver*, the percentage of solids is, like that of the water, considerably reduced. The same remark is here applicable as has been made above, with respect to the water.

In the *waxy liver*, a very marked increase in the percentage of solids goes along with a considerable diminution in that of water. Thus, it will appear from the fourth column, that considering the water as a fixed quantity represented by 100, the solids in the

normal liver, as well as in the fatty liver, vary between 20 and 30,¹ while in the waxy degenerations they are represented in one case by 48.3, and in the other by 54.8. An increase so considerable as is indicated by these numbers is very characteristic of this degeneration, and serves at once to distinguish it from the fatty liver, with which it has sometimes been confounded by pathological writers.

In *cirrhosis*, the examination of the solids gives results even more variable than that of the water. They are sometimes above and sometimes below the normal average.

3. *As to the Fatty Matters.*—The variations presented by the fatty matters of the liver, even in the state of apparent health, and much more in that of disease, exceed those of all the other chemical constituents. The fact of these variations is very familiar to the anatomist who habitually tests the condition of the glandular epithelium by means of the microscope. A similar change is also well known to take place in several other situations in the animal body. But the extent to which the accumulation of oil proceeds in the liver, and this without any marked structural disorganization, and without any more than a *relative* decrease in the weight of the other constituents, places the so-called fatty degeneration of this organ in a quite special pathological position. It will be seen from the second column in the table, that while the normal proportion of fatty matters ranges from 1 to 4 per cent. of the entire liver, it rises in the fatty condition to above 30 per cent.; and while in the normal state it stands in the relation of from 8 to 18 fatty matters as compared with 100 solids, it reaches in the pathological condition the enormous proportion of 230 as compared with 100 of solids. Or, to take perhaps a more striking, though not so exact a manner, of indicating the extent of the morbid accumulation, it appears from the last column that while the amount of fatty matter in the entire liver in the state of health is from *one to two ounces*, it attains in some cases of disease the surprising quantity of *one, two, or even possibly three pounds*, where the liver is much enlarged.

In the *waxy liver*, which has often been described as a variety of the fatty condition, the aggregate of fatty matter is frequently increased, owing to the increased size of the organ; but this is by no means generally the case, and microscopic examination generally leads to the conclusion that the waxy organ is unusually poor in oil. In the two cases analysed the percentage of oil was nearly normal.

Cirrhosis is in a large proportion of cases complicated with an unusually fatty condition of the epithelium, which sometimes, as in Case IX., approaches that of aggravated forms of fatty liver; but I have seen many cases of this affection without any such complication.

¹ Except in Case III., which in every point of view presents a slight approach in composition to the waxy condition.

4. *Summary*.—In Table II. I have given a series of empirical formulæ deduced from the preceding observations, and representing, in a useful form, the more general results of this inquiry, with respect to the aggregate constitution of the liver. It is only necessary, in conclusion, that I should connect these results with those of anatomical investigations.

The *normal liver* is composed of very numerous hepatic glandular cells, disposed amid a network of capillary vessels and biliary ducts, and fed by a large amount of blood. It may be regarded as consisting, in a chemical point of view, of water to the amount of about three-fourths; of an insignificant amount of fatty matter; and of various albuminous solids, amounting to nearly one-fourth of its weight.

The *fatty liver* consists of precisely the same elements in a similar structural arrangement. The aggregate of solids and water may be slightly increased or slightly diminished; their percentage, however, is invariably considerably reduced, owing to the enormous development of oil in the glandular cells, by which the volume of the organ, as well as its weight, may be almost indefinitely increased.

The *waxy liver* is a true degeneration; for this condition is not, like the fatty liver, due merely to a morbid increase of one of the non-essential elements of the organ, but to a metamorphosis of its most essential, *i.e.*, its glandular structure, into a much more dense albuminous material than in the normal condition; a change which results in total destruction of the function, as well as the structural characteristics, of the gland-cells, and in a great increase of the solids of the organ at the expense of the fluids, the blood ceasing to circulate freely, and the water being diminished from three-fourths to three-fifths, or possibly in some cases to one-half of the weight; the solids being correspondingly increased, the fat either increased or unchanged.

Cirrhosis of the liver is either an independent degeneration, or it may spring from the waxy or the fatty conditions. In the former case, it assumes at once the atrophic form, and all the elements may simultaneously disappear; the atrophy commencing, however, in the glandular epithelium, and thus giving rise to an apparent relative hypertrophy of the fibrous tissue. This was the condition in Case XI., which is represented in the fifth column of Table II. In other instances cirrhosis is a complex lesion, and, especially when the liver has been greatly enlarged, it participates largely in the chemical characters of the waxy or fatty liver.

Finally, the *waxy and the fatty liver* are not unfrequently met with as a combined lesion. In such cases the liver is usually very much enlarged, dense and opaque yellow on section; intermediate in specific gravity between the two typical forms of disease, and therefore approaching the normal density; and it presents under the microscope a degenerated epithelium, and very numerous but unusually small fatty granules and globules. I have never made the

analysis of such a case, but it is impossible to doubt what would be the result. In the purest forms of the waxy disease, again, I believe the amount of fatty matter would be found very small indeed, at least if one can judge from the results of microscopic examination.

TABLE II.—EMPIRICAL FORMULÆ REPRESENTING THE CHEMICAL CONSTITUTION OF THE ENTIRE LIVER IN HEALTH AND IN VARIOUS PATHOLOGICAL CONDITIONS. (*Avoirdupois Weight.*)

	Normal Liver.	Fatty Liver.	Waxy Liver.	Cirrhosis with Hypertrophy.	Cirrhosis with Atrophy.
	Lbs. oz.	Lbs. oz.	Lbs. oz.	Lbs. oz.	Lbs. oz.
Water	3 0	2 8	3 6½	4 2	1 6
Solids	0 15	1 0	2 0	1 3	0 7½
Fat	0 1	1 8	0 2½	0 5	0 2½
Total weight	4 0	5 0	5 8	5 10	2 0

ARTICLE II.—*Contributions to Teratology.* By A. MERCER ADAM, M.D., Dumfries, Member of the Physiological Society of Edinburgh.—(*Continued from p. 248.*)

NO. IV. THE FUNDAMENTAL LAWS OF TERATOLOGY.

“NATURE,” Soemmering truly remarks, “does nothing undesignedly; for, even in her malformations, the greatest regularity is observable.”¹ Modern investigations in teratology, and recent advances in embryology, have clearly shown that malformations are neither the blunders and failures of nature (as was thought by Aristotle), nor yet *lusus naturæ*, her eccentric and inexplicable freaks. To render more intelligible what we may have to consider hereafter, I shall briefly state the three most important of these teratological axioms.

a. *Analogous parts have an affinity for each other.*

This harmony of union is uniform and constant; it constitutes the “*lex proprietatis*” of Fleischmann, and the “*affinité de soi pour soi*,” of St Hilaire. Dissimilar parts are only united when (as in the case of the bladder and rectum, or pharynx and larynx) they are originally developed from a common mass. Arteries are never

¹ *Abbildung und Beschreibung einiger Missgeburten*, 1791, p. 35

seen coalescing with nerves, nor the alimentary canal with the abdominal aorta; but in malformations by duplicity, the fusion always takes place between similar parts; sternum is united to sternum, vertex to vertex, pelvis to pelvis, and so on. And not only so, but in such cases the corresponding organs of the united twins are joined together; muscle corresponds to muscle with wondrous certitude, the nerves join nerves, and the arteries their fellow-vessels.

We may here observe that malformations arise, as well from the non-union as from the abnormal junction of several parts. According to Serres'¹ theory of "eccentric" development, the embryo is primarily formed from two lateral halves, and hence all the organs situated centrally, as the heart, uterus, bladder, etc., are originally double. Of the corresponding organs of the two halves, some are intended to be united, and others to remain distinct and double. Now, should embryogenesis at this stage be disturbed, the future appropriate conditions of these organs may be totally disordered; and it is evident that, in such a case, the non-junction of parts which ought to be united, will as surely produce a structural malformation, as will the fusion of others destined to remain separate. We see this exemplified in the uterus, the lateral halves of which, instead of uniting into a symmetrical whole, often remain separate, causing the organ to retain its embryonic type, and remain double or bicornuous throughout life.

The corresponding or homologous organs, which are most frequently abnormally united, are those between which no important texture intervenes. Thus the kidneys, separated by no distinct dividing structures, are oftener amalgamated than the eyes or the ears, between which lie osseous septa; and when in these latter cases union does occur, the partition walls are either found to be in a rudimentary condition, or to have been secondarily destroyed.

b. Malformed organs never wholly lose their normal types.

However deformed a part may be, it never so completely loses its ordinary distinguishing characteristics as to defy recognition. Thus the bones composing the anencephalic cranium, rudimentary and malformed though they be, are all severally distinct and capable of recognition by the anatomist.

c. Malformations seldom occur singly.

This law needs little illustration. How often do we find harelip, talipes, and spina-bifida, co-existent with defective formations of the cranium; and how often do we see anomalies of the fingers, toes, and genital organs, in cases of diplogenesi! Indeed, it would seem as if, when defective development occurred in one part of the body, nature compensated for the atrophy by the increased growth of some

¹ Memoir on Transcendental Anatomy in *Annales des Sciences Natur.*, vol. xxi.

other region. This general principle was first noticed by St Hilaire¹ and Breschet,² who designated it the "loi de balancement," or "balancement organique." The inference which these authors would deduce from it is, that organic hypertrophy can only occur in utero, at the expense of the nutrition of some other part. But there are many cases in which no such compensation can be observed; thus, for example, the vascular and other systems may be found quite normal, while the nervous is but imperfectly developed; and *vice versa*. Still, in the majority of instances, the general law holds good, that the malformation of one part affects the development of others in a greater or less degree.

In the simultaneous occurrence of malformations, we generally find that the organs secondarily affected are those which are homologous or repeated in the body. Hence the frequency of anomalous conditions of the teeth, ribs, vertebræ, fingers, and toes—both in regard to number and structure—in all the graver types of foetal deformity.

NO. V. THE HEREDITARY CHARACTER AND FREQUENT RECURRENCE OF MALFORMATIONS.

No fact concerning malformations seems more surely determined than their hereditary character. Let us cite a few illustrative cases:—Naef delivered a woman of a bicephalous child, the father of which had had a brother who was also born with two heads.³ Anna relates the case of a man who had eleven children by his *first* wife, nine of whom were born dead, and two alive, but all of them with harelip. The first child which this man had by his *second* wife had also harelip; the third, by this marriage, had malformation of the jaw (*Wolfsrachen*); and the fourth harelip again.⁴ Two relations of this man had been born similarly malformed. The foregoing, like many analogous cases, proves that a well-formed man may procreate, with different women, deformities of the same kind. I have seen a father and son having each twelve fingers and twelve toes. Congenital luxation of the femur has been known to occur in cases where the mother suffered from that deformity.

Vrolik considers that hereditary transmission exerts a decisive influence over malformations, causing their gradual decrease and final disappearance in the last born. This hypothesis is feasible enough, inasmuch as we know that many hereditary taints become, in the course of time, by healthy intermarriages, completely extinct in succeeding generations.

Malformations, having once appeared, are very prone to recur,

¹ Hist. des Anomal., vol. iii. p. 401.

² Dict. des Médéc. "Déviations Organiques."

³ Osiander. neue Denkwürdigkeit, vol. i. p. 193.

⁴ Meckel. Pathol. Anatom., vol. i. p. 19.

and when they do so they are generally of the same species. Many cases are on record proving this fact; we content ourselves with the mention of only one or two. A woman bore three children within three years, and in all of them the forearms and lower parts of the legs were wanting.¹ Another woman who several times bore twins was finally delivered of a double monstrosity.² In fact diplogenesi often recurs in the same woman.³ I delivered a woman, in Edinburgh, last summer, of her fifth child, which had harelip. All her former children were similarly deformed, and all of them had been operated on, for this affection, by Professor Syme. Dr Martin⁴ relates the case of a lady—who had previously born two perfect and well-developed children, and who enjoyed excellent health—giving birth successively to seven which were malformed. Of these, six were anencephalic (five of them born at the full time, and one an abortion two inches long); the remaining one was a mature foetus, having a normally formed cranium, but being so defective in the development of its flexor muscles, that its legs, arms, fingers, and toes could not be bent.

NO. VI. THE DURATION AND PHENOMENA OF PREGNANCY WITH MALFORMED CHILDREN.

Obstetrical works take very little notice of pregnancy with malformed children; but, from all the information we possess on the subject, we think it is evident that, in such cases, *the period of utero-gestation may be, and often is, quite normal in its duration and symptoms*. Professor Hohl,⁵ of Halle, collected and carefully examined the statistics of 128 cases of pregnancy with deformed children, and the general results he arrived at I shall place here, for the sake of distinctness, in a tabular form.

DURATION OF PREGNANCY IN 128 CASES OF MALFORMATION.

		Nature of Malformations.	
97 attained the normal length,	{	Per defectum,	28
		Parasitical,	3
		Syreniform,	2
		Diplogenesis,	64
		—	97
31 were born prematurely,	{	Per defectum :—	
		Chiefly acephalia	14
		and anencephalia,	
		Diplogenesis	17
		—	31
		Total,	128

¹ Flachland. Observat. Pathol. Anatom. 1800.

² Du Vernoi. Act. Petropol. iii. p. 188.

³ Bischoff: Wagner's Handwörterbuch, vol. i. p. 910.

⁴ New York Journal of Medicine. March 1849.

⁵ Die Geburten missgestalteter, Kranker, und todter Kinder. Halle, 1850.

Of the above 31 cases, in which pregnancy was of abnormal length, the births occurred as follow:—

Before the 1st month there were born,				8
"	5th	"	"	3
"	6th	"	"	1
"	7th	"	"	12
"	8th	"	"	6
"	8th and 9th	"	"	1
"	9th	"	"	5
Total,				31

The foregoing tables show that, in three-fourths of all cases of malformed children, pregnancy attains the usual length; and that when birth occurs prematurely this happens most frequently in the seventh and eighth months of utero-gestation.

There are, generally speaking, no peculiar *symptoms characterising the period of pregnancy with malformed children*, by which their existence may be suspected by the mother, or diagnosed by the accoucheur. But, as in certain cases, abnormal appearances and sensations have been observed, we consider it our duty to mention them in this place.

a. Great abdominal distension from superabundance of liquor amnii is the most common of these. Naegele¹ frequently observed it in cases of foetal malformations; and Hohl² remarked it in five cases of anencephalia, and in four of diplogenesia. Ramsbotham³ mentions the case of a woman who had born six children, three of which were anencephalous, and who had a much greater quantity of liquor amnii with each of the malformed children than with any of the others. My friend, Dr Grieve of this place, informs me that he recently delivered a multiparous woman of a child having defective formation of the cranium, and that there was great superabundance of liquor amnii, more than double the quantity in any of her previous pregnancies. The greatest quantity of liquor amnii I ever witnessed was in a case I attended last autumn, where the child was still-born, having anencephalia and spina-bifida. So great was the superabundance of the fluid, that the whole floor of the apartment was saturated by its discharge. How is this to be accounted for? We know that excess of liquor amnii is not unfrequent when the infant is diseased or dead; and in the case of malformed children we may conjecture that it probably results from some obscure intra-uterine inflammatory action affecting the membranes, or, perchance, in cases of anencephalia, from the bursting of some hydrocephalic sac.

b. Oedema and great weight of the mother's body.

These have been mentioned, as pathognomonic signs of pregnancy

¹ Lehrbuch der Geburtsh., 7th ed., p. 248.

² Op. cit., p. 58.

³ Principles and Pract. of Obstet. Med., 1841, p. 661.

with malformed children, by Haller, Detharding, Villeneuve, Klein, etc.; but they seem to be so variable and uncertain as hardly to be depended upon for diagnosis.

c. Peculiarity of the foetal motions.

This is even a less trustworthy sign than the foregoing; for, as far as I have been able to ascertain, there exists no uniformity in the intra-uterine movements of malformed children. In some cases these are described as being vigorous, in others as very weak. They are often very powerful in anencephalic foetuses, where the brain is wanting,—a proof that the motions of the child in utero are not cerebral, but are purely excito-motory or reflex.

In fine, I consider that we have no means of ascertaining, with any degree of certainty, the existence of malformations during the intra-uterine life of the foetus.

It may perhaps not be out of place here to inquire *whether malformations occur more frequently among primiparous or multiparous women?* The only author I am aware of who has given us any statistical information on the subject is Professor Hohl, the results of whose investigations I shall once more quote.

CONDITIONS OF THE MOTHERS IN 69 CASES OF MALFORMATIONS.

		Nature of Malformation.	
24 were primiparous,	{ Per defectum	9	
	{ Diplogensis	5	
		—	14
55 were multiparous,	{ Per defectum	17	
	{ Per excessum, viz.		
	{ Diplogensis	35	
	{ Parasites	3	
		—	55
Total,		—	69

Hence it would seem that of malformations in general four-fifths occur among multiparæ; that of these the greater number are per excessum,—principally resulting from duplicity; and, on the other hand, that the minority, happening among primiparous women, are chiefly caused by defective development. May we suppose, in explanation of this, that the formative powers of nature become increased, or more fully developed, after they have been, or in proportion as they are called into action? Obstetrical statistics do not warrant any such conjecture; for, although twin births happen oftener among multiparous women, the frequency of their occurrence is not in a ratio corresponding to the number of the mother's pregnancies.¹ It will be seen by the following table (taken from

¹ Dubois and Hardy assert that twin births occur more frequently among primiparæ, but their opinions are proved to be incorrect, alike by the statistics of Collins, cited above, and those of Professor Levy, of Copenhagen, the most recent authority on the subject. According to the latter obstetrician, 7742 births have occurred during the last ten years in the Copenhagen Lying-in-

Dr Collins¹), of 240 cases of twin births, that while 72 occurred in first, and the remainder in subsequent pregnancies, the frequency of their occurrence decreased in proportion to the number of the previous pregnancies.

Statistics of 240 Twin Births.

Number of pregnancy, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13,
Number of women, 72, 35, 37, 20, 22, 15, 15, 6, 6, 7, 1, 1, 1.

It would seem from these statistics that the formative power (*vis formativa nature*) becomes weakened by being frequently called into action; and we think we may safely conclude—reasoning analogically—that, in this respect, malformations by duplicity are governed by the same laws as normal twins.

ARTICLE III.—*The Anatomy and Surgical Relations of the Fascia of Scarpa.* BY JOHN STRUTHERS, Fellow of the Royal College of Surgeons, Lecturer on Anatomy, Edinburgh.

THE fascia which I shall describe under this name, is that usually termed the deep layer of the superficial fascia, or the true superficial fascia, of the groin, and frequently also the fascia of Scarpa. Although the description of this fascia by Scarpa, as contained in the translation of his work on hernia, is very brief and imperfect, I shall retain the title fascia of Scarpa, as it is preferable to have a short specific name for a part to which frequent reference is to be made.

This fascia, I believe, is not commonly understood fully, in the simplicity of its anatomy and importance of its surgical bearings, and is, at least in some text books, incorrectly described.

It is described as a deeper layer of the superficial fascia of the abdomen, passing down over Poupart's ligament, adhering to it as it crosses, and then terminating on the fascia lata across the groin, or by some it is said to pass down the femoral region, as a deeper layer of the superficial fascia there, separated from the other by the superficial vessels and glands, and spreading over the saphenous opening so as to form the cribriform fascia.

In the common mode of conducting the dissection of the groin, the true origin of the fascia is divided; and unless the dissection is made with a view to the demonstration of this fascia, its nature and connections cannot be seen or understood.

Dissection to demonstrate the Fascia of Scarpa.—Having reflected the skin of the groin for some inches both above and below Poupart's ligament, divide the superficial fascia of the thigh two or three

Hospital; and of these 117 were of twins. Of these 117 cases, 51 were born by primiparous, and 66 by multiparous mothers.—*Vide* his paper in *Hosp. Meddelelser.*, Bd. 5; and in *Schmidt's Jahrb.* 1854. No. 3, p. 326.

¹ Practical Midwifery, p. 331.

inches below Poupart's ligament, and, using the point and handle of the scalpel, turn it up, off the surface of the glands and superficial vessels, to about an inch above Poupart's ligament. Next, by an incision curving down from the anterior superior spinous process of the ilium to near the symphysis pubis, divide the fascia through its whole depth, down to the tendon of the external oblique; and now dissect the whole fascia down, close off the external oblique. The lax cellular tissue here, yields almost to the handle of the scalpel, but at, or immediately below Poupart's ligament, the handle of the scalpel is suddenly and firmly arrested. The dissection is now done. Now take the femoral end of the dissected superficial fascia in one hand, and the abdominal end in the other hand, lifting them up a little and stretching them up and down, and, on looking in below, a thin semi-transparent fibrous membrane is seen, passing between the superficial fascia and the fascia lata. This is the fascia of Scarpa, fully displayed; but if the glands and surrounding cellular tissue be now carefully picked away from its lower or femoral aspect, the fascia will be more clearly seen.

Description of the Fascia of Scarpa and its Relations.—The fascia of Scarpa is a thin semi-transparent membrane, of considerable strength, and not merely cellular, but fibrous in its nature. It arises from the fascia lata, across the groin, immediately below Poupart's ligament; passes up over the latter, and, after a course of an inch, as a separate membrane, joins and blends with the common superficial fascia of the abdomen. It is separated from the common superficial fascia opposite Poupart's ligament, and for an inch, or half an inch, above it, by the transverse set of lymphatic glands and the superficial vessels. Immediately above the position of these glands, it unites or blends with the common superficial fascia, and is not naturally separable from it farther, that is, not without artificial dissection, although the fibres of it are no doubt continued up and across the abdomen, as the deeper aspect of the common superficial fascia.

This fascia belongs altogether to the abdominal region of the groin; it does not exist in the femoral region; but begins immediately below Poupart's ligament, as a prolongation upwards from the fascia lata, as a thin aponeurotic membrane, as broad as the groin itself, and about an inch in depth as a separate membrane, from its attached margin, or origin, obliquely upwards to where it becomes united with the common superficial fascia.

The error of some, in describing this fascia as passing downwards into the femoral region, forming a deeper layer of superficial fascia there and also the cribriform fascia, has no doubt arisen from the circumstance that the true origin of the fascia, from the fascia lata, is divided with the knife, under the impression that it is only an adhesion of the fascia to Poupart's ligament which is being divided; but if this be divided, the fascia of Scarpa is destroyed. For some inches below Poupart's ligament, no doubt there is the appearance as

if the common superficial fascia consisted of two layers, separated by the femoral glands and superficial vessels; a thicker and superficial, containing more or less fat; and, behind and between the glands, laminated cellular tissue as a deeper layer. But these are not separate fasciæ; they both belong to the common superficial fascia, are both cellular in their nature, and quite continuous with each other, except at the points where a gland or vein necessarily separates them in making a bed for itself. It is therefore unnecessary and erroneous, to describe the superficial fascia below Poupart's ligament as consisting of two separate layers or separate fasciæ.

It is also incorrect to describe the fascia of Scarpa as passing over the saphenous opening and there forming the cribriform fascia; first, because this fascia stops, or rather begins, close, as close as possible, below Poupart's ligament; and secondly, because the cribriform fascia is a different structure, the correct understanding of which is bound up with a correct understanding of the nature of the saphenous opening.

The *cribriform fascia* is simply a thin covering to the saphenous opening, and limited to this; prolonged from the outer edge of the opening, and losing itself by covering and uniting with the femoral sheath, which, on the inside, adheres to the pubic portion of the fascia lata on the pectineus muscle. The outer and upper parts of the edge of the saphenous opening do not naturally exist as defined margins, until this true prolongation from them is divided with the edge of the knife; and the cribriform fascia is, therefore, a thin prolongation of the fascia lata itself across the opening, and not a portion of the superficial fascia adhering to the opening as it crosses it.

Relation of the Fascia of Scarpa to the Fascia of the Perineum.—Besides those already described, the fascia of Scarpa has another important connection, in its continuation around the spermatic cord, towards the perineum, where it becomes continuous with the true superficial fascia of that region; a connection which is usually correctly described. The fascia in the perineum is variously called the deep or true superficial fascia, the deep layer of the superficial fascia, or the superficial fascia. Amongst these various definitions the student is often at a loss to know which structure is being described, and to avoid this source of confusion and derive the advantage from the use of a short specific name, I am in the habit of calling it the *fascia of Colles*, as the general connections of this membrane, and especially the course of infiltrated urine determined by the fascia, appear to have been first fully described in this country by the late Mr Colles of Dublin, in his excellent and practical treatise on surgical anatomy; and if anatomists would agree to call these two fasciæ the fascia of Scarpa, and the fascia of Colles, it would save much perplexity to the student.

The fact is, there is only one superficial fascia in the body,—the common superficial fascia,—equally extensive with the skin, com-

posed fundamentally of cellular tissue, and containing more or less or no fat according to circumstances, but whether very thin, or very thick from fat, it is still the same cellular membrane, composed of areolar or filamentous, or what is after all much better to be called by the old name of cellular tissue; and although, especially when fat is laid down in it stratum after stratum, presenting sometimes an appearance of layers, it is still one fascia, the common superficial fascia, reaching from region to region as the skin does.

Then we come to the deep fasciæ or aponeuroses, not cellular, but fibrous membranes, thicker or thinner according to circumstances, and serving the altogether separate purpose of binding down the muscles; and not usually, like the skin and superficial fascia, common from region to region, but with its special anatomy in each region, connected with its bones and binding down its muscles. Now, the fasciæ of Scarpa and Colles are, in their nature, intermediate between the superficial fascia and the aponeurosis. They are deep and fibrous membranes, aponeurotic, at their origin from fibrous structures or aponeuroses, but become cellular at their other connection, as they blend with the superficial fascia; they pass between the deep and the superficial fasciæ and partake of the nature of each as they join each.

The fasciæ of Scarpa and Colles form one continuous membrane.—The latter joins the base of the triangular ligament, or true deep fascia, of the perineum, and is also there, like the latter, continuous with the thin fibrous membrane or fascia on the outer surface of the levator ani muscle; at the sides, it is fixed to the rami of the ischium and pubes, and upwards from this, it is fixed to the fascia lata, keeping some distance outwards from the pubes. Here it runs directly into the fascia of Scarpa; which now forms a continuation of it, as a barrier across the groin, immediately below Poupart's ligament. The origin, or fixed or deep line of attachment, of this united fascia, then, is,—the base of the triangular ligament, the rami of the ischium and pubes, the pubic portion of the fascia lata over near the origin of the gracilis and adductor longus muscles, and the fascia lata all the way across the groin immediately below Poupart's ligament. The posterior part—fascia of Colles—is continued forwards into the scrotum, blending with the common superficial fascia, and thus forms a pouch, closed behind in front of the anus, closed laterally at the sides of the perineum, and open forwards into scrotum and upwards around the cord. The anterior part, or fascia of Scarpa, continues the attachment of the sides of the pouch across the groin, so that the pouch, continued up from the scrotum along the cord, now widens out and opens laterally and upwards on the abdominal wall, between the continuation of the fascia of Scarpa and the surface of the tendon of the external oblique muscle, in which space there is a plentiful and lax cellular tissue.

Relation of the Fasciæ to Infiltration of Urine.—When the urine escapes from the urethra into the perineum, it passes forwards.

It cannot pass backwards to the ischio-rectal fossa or anal region, on account of the reflection of the fascia of Colles to join the base of the triangular ligament; it cannot gravitate downwards along the inside of the thighs, owing to the lateral closure of the pouch; and it must therefore pass forwards, in the direction where no obstruction is presented by the lax cellular tissue. This may be readily illustrated by introducing a blow-pipe, or bellows, through an opening in the fascia of Colles, in front of the anus, when the air will readily force its way forwards and inflate the scrotum, but does not reach backwards or side ways.

The infiltrated urine, if not evacuated by incisions, made deep enough to reach it, continues to ascend over the cord, passing up to the hypo-gastric or even to the umbilical region, but it does not gravitate downwards to the front of the thigh. This is prevented by the fascia of Scarpa, which, forming a fibrous barrier across the whole groin, fixed to the fascia lata, effectually prevents the fluid from passing downwards. It therefore passes upwards, and infiltrates the plentiful and very lax cellular tissue which lies close on the outer surface of the tendon of the external oblique; the continuation upwards of the fascia of Scarpa, as the deeper aspect of the superficial fascia of the abdomen, still preventing the fluid from becoming subcutaneous and descending to the thigh.

Relations of the Fascia of Scarpa to Hernia.—The fascia of Scarpa forms one of the coverings of the cord, as it passes to be continuous with the fascia of Colles, turning round in below the cord, and forming a kind of tube or canal within which the cord descends to the testis in the scrotum. The fascia of Scarpa, then, assists in directing a complete inguinal hernia down into the scrotum, and forms one of its coverings. It does not form a covering to the femoral hernia, but a complete femoral hernia lies upon it; and its union, a little above Poupart's ligament, with the common superficial fascia, will tend to obstruct the farther upward passage of a femoral hernia. The femoral hernia, after it has passed down the crural canal (which is rather, as far as the hernia is concerned, a broad ring than a canal) becomes complete by turning round the lower margin of the falciform process, there being now no longer any dense structure to keep it from swelling forwards, and now passes up so as to lie upon the falciform process and on the inner part of Poupart's ligament. Here it lies on the fascia of Scarpa, and has insinuated itself into the space occupied by the glands, and, meeting with the angle of union between the superficial fascia and the fascia of Scarpa, it will be arrested in its farther progress upwards, although at the same time, irrespective of this, there is little natural tendency in the femoral hernia to continue to ascend, but rather simply to swell out in all directions, if it continues to enlarge.

The true relation of the fascia of Scarpa to the fascia lata was, as

far as I am aware, first correctly described by Mr Liston¹ in a Memoir, the chief purpose of which was to show that the crural arch, or deep crural arch, is formed independent of Poupart's ligament, by the iliac portion of the fascia lata passing in behind Poupart's ligament, and joining with the fascia transversalis; the union of the two, arching inwards over the vessels and crural ring, and forming a tight arch, which remains so after Poupart's ligament has been altogether dissected out. He describes the true origin of the fascia which I have described as that of Scarpa, and corrects the notion that it passes down upon the thigh. He thus very correctly describes the terminations or connections of the fascia lata at the groin—"The iliac portion, forming the falciform process, is again divided into two layers; the one passing anteriorly to the abdominal muscles, and named the superficial abdominal fascia; the other, again, lining their inner surface, and passing betwixt the transversalis muscle and peritoneum, forming the fascia transversalis. The lower part of the abdominal muscles and Poupart's ligament thus lie in the angle of splitting, or union of all these fasciæ, and the crural arch is formed by this junction."

He refers to the relation of this fascia to inguinal hernia, but not to its relation to the fascia in the perineum, or to the course of the infiltrated urine, nor does he give a more extended view of its connections than that stated or implied in the above quotation, which, however, shows that he was acquainted with its true origin from the fascia lata close below Poupart's ligament.

The description I have given, embraces what I believe to be the true anatomy and surgical bearings of the fascia of Scarpa. The reason why it is by some incorrectly described, and perhaps in none of our anatomical works described clearly as having the anatomy I have endeavoured to explain, is, I believe, that its origin, as a continuation upwards of the fascia lata is usually divided, either from above or from below, after which the fascia cannot be seen, that in fact this fascia is but seldom seen in dissections. But if the dissection is conducted according to the method above described, the demonstration of the fascia is very easy, and it only requires to be once seen in this way, to satisfy the dissector that this is its true anatomy.

The whole anatomy of this fascia is very simple, and, together with its surgical bearings, may be shortly described thus—

The *fascia of Scarpa* arises from the fascia lata close below Poupart's ligament, passes upwards for an inch, and blends with the common superficial fascia. It is separated from the lower part of the tendon of the external oblique, behind, by very loose cellular tissue, and, before, from the common superficial fascia by the superficial glands and vessels; and is continued inwards around the cord, becoming continuous with the fascia of Colles or true superficial

¹ Memoir on the Formation and Connections of the Crural Arch. Edin. 1819.

fascia of the perineum. It is a thin aponeurotic or fibrous membrane, forming a barrier or septum across the groin, by passing between the fascia lata and the common superficial fascia. When the urine, infiltrated in the perineum, has been directed upwards along the cord by the fascia of Colles, the fascia of Scarpa prevents it from passing down the front of the thigh. In relation to hernia, —it assists in directing an inguinal hernia into the scrotum, and forms one of its coverings; it is covered by a femoral hernia, and tends to prevent the femoral hernia from passing upwards on the abdomen, by means of its union with the common superficial fascia.

ARTICLE IV.—*On the Prevalence of Chromato-Pseudopsis or Colour-Blindness; its Evils, and the Means of Diminishing its Frequency.*
By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.
—Continued from p. 323.

AFTER ascertaining the points which I have mentioned, it became an object of much interest to ascertain whether any of the parties whose cases I have described, would fail to perceive the red end of the spectrum as perfectly as others did. I selected Mr Hughes, Dr Y., Mr B., and T. R. for trial; and Prof. Kelland (to whom I have been under many obligations throughout this inquiry) kindly allowed me the use of the necessary apparatus for the production of the solar spectrum, and assisted in the examination, at which Prof. Robert Hunt and Mr Walker the oculist were also present.

The trials were made on March 4th 1854, and were fewer than we could have wished; but the short period during which sunlight served us, left us no choice but to make a rapid investigation.

A good spectrum was produced on a white paper screen, and the gentlemen under trial were requested, one by one, to describe the colours which they saw in the solar image, and the length to which it extended.

Mr Hughes, who mistakes red for black only when these occur in thin lines, appeared to perceive as long and as many coloured a spectrum as any of us did.

Dr Y., whose judgments on colours are always vacillating and uncertain, was indecisive in his answers, and we did not, as time pressed, make a prolonged examination of his case.

T. R., who is the best marked example known to me, of a strongly marked congenital tendency to confound red with black, was tried both with the spectrum of the sun, and with that of the lime-ball light. He appeared to see as lengthened a solar spectrum in the direction of the more refrangible rays as any of us did, but at the less refrangible end, when asked by Prof. Kelland to follow his pencil as it was carried from the middle of the spectrum towards the red, and to announce when he ceased to see colour, he invariably

arrested the pointer, whilst the red still seemed to our eyes vivid and full. So far as we could judge, from one-sixth to one-eighth of the red was invisible to this gentleman.

The lime-ball spectrum was much fainter and less brilliant than that of the sun, but nearly the whole of the red was not discerned by T. R.

Mr B., it will be remembered, was congenitally perfect in his vision of colours, but became colour-blind after a severe accident which occasioned concussion of the brain. (*Ante* vol. xlvii., p. 506). Bright blue and yellow are the only colours which he distinctly perceives, and these only close at hand; but he is not aware of any special tendency to confound red with black.

He saw a much shorter solar spectrum than any one else, the blue and the red end being both truncated, and two colours only, yellow and blue, being recognised. He was tried with the prismatic image in various directions, and at successive intervals, but he always gave the same answer, pronouncing fully one-third of the red extremity to be undistinguishable to him from those portions of the sheet of paper on which the spectrum did not fall. A faint, stray light accidentally cast upon the paper was plainly distinguishable to him from the surrounding darkish space, but the extreme red, which, to the eyes of Prof. Kelland, Prof. Hunt, Mr Walker, and myself, was very vivid, was to him invisible. When requested to look at the solar spectrum through a cobalt-blue glass, which, to normal eyes, cuts off a portion of the red, but not the extreme red, he lost the red altogether.

The lime-ball spectrum, when looked at by Mr B. with the naked eye, was in the same way invisible at its less refrangible extremity, and what seemed to us a clearly defined red, conveyed no sensation of either light or colour to his eyes.

Although the examination I have reported was more hurried than was desirable, no doubt remained on our minds, that a portion of the spectral red, both of the sun's light, and of that of incandescent lime was invisible to T. R. and to Mr B.; and further, that the missing red was not replaced by yellow or any other colour, or by a colourless luminous impression, but so far as could be ascertained in a room not absolutely dark, was exchanged for black, or at least a deep grey.

Altogether, then, the proof of blindness to red, as a peculiarity of the colour-blind eye, is, I think, complete, but I wish to guard against being supposed to refer to it as constant in those in whom it occurs. On the other hand, it is only occasional, and the conditions of its occurrence are as yet imperfectly known. The modifications which Savart's researches have compelled us to make on Wollaston's conclusions regarding *shrill deafness* warn me against affirming that there are no circumstances in which T. R., Mr B., or others like them, would see the whole red of the spectrum. A prismatic image of great intensity of colour might be visible throughout its entire

length, where a feebler spectrum was only partially visible ; and the concentration of the red rays by a lens might enable them to produce their normal impression on the most markedly colour-blind eye.

I have been slow to publish the results which are announced in this section, from their incompatibility with the conclusions of Sir John Herschel and Sir David Brewster, who regard the colour-blind as perceiving as long a spectrum as the normal-eyed do. It is not my object, however, to affirm that to every colour-blind eye the spectrum is, in all circumstances, shorter than to every normal one, or that the same eye, whether normal or not, always sees a spectrum of the same length. The opposite is as likely to be the truth ; and it may be expected that future inquiry will show that my inferences are reconcileable with those at least of the first-named philosopher.

The views of Sir David Brewster on colour-blindness, as known to me from his "Letters on Natural Magic," which I believed to contain his fullest views on the subject, are not so easily reconciled with my results ; for, in accordance with his beautiful doctrine of a triple spectrum, he urges that an eye blind to red, but perceptive of yellow and blue, must continue to see these, and especially yellow, in the less refrangible end of the spectrum, and as each of the three-coloured spectra is equally long, the absence of one of the three (namely, the red), though it altered the colour, could not alter the length of the residual two-coloured image.

Since reducing my results to writing, however, I have found a fuller statement of Brewster's views, which not only accords in many respects with the conclusions I have reached, but anticipates certain of them. Apart, accordingly, from the interesting nature of his speculations, it is an act of simple justice to point out where he has anticipated me. His later views are contained in a paper entitled, "Observations on Colour-blindness, or Insensibility to the Impressions of Certain Colours."¹

After a reference to Wollaston's observations on deafness to shrill notes, he proceeds :—"I have proved from numerous experiments, that when the retina is rendered partially insensible by the action of light upon any one part of it, *it first becomes insensible to red light* ; and hence we have a distinct reason why *red-colour blindness* is the general character of the defect under consideration ; and I am persuaded that any defect of sensibility produced by the action of light, or by any other cause, will, if carefully examined, be found to be a maximum with *red* light.

"In experiments of this kind, in which what we may call *artificial*

¹ Phil. Mag., August 1844, p. 134. This paper is referred to in the notes added to the translation of Wartmann's paper "On Daltonism," in Taylor's Scientific Memoirs for 1846, by the editor of that work. Its title, however, is not given, nor are the opinions of Brewster, quoted above, noticed by the editor, whose quotations and references led me to suppose that it was simply a correction by Sir David (as in part it is) of certain misapprehensions of his views by Prof. Wartmann, so that I did not consult it till quite recently.

colour blindness is produced, the intensity of the light is always diminished; but it remains to be determined by accurate observation whether the *red* end of the spectrum (for example), when seen *yellow* by an eye defective in its judgment of colours, is *brighter* or more *obscure* than it would have been had no such defect existed. I am persuaded, from many observations I have made, though I do not consider them as decisive of the question, that the object is seen more obscure, and that certain of the rays emanating from it are not appreciated by the nervous membrane. If, on the other hand, every ray from the red object is efficacious, and the only effect is the substitution of *yellow* or *green* in place of *red*, then we might expect that the object would appear brighter, in so far as a *yellow* sensation produced by a given number of red rays should be brighter than a *red* sensation produced by the same number.”¹

In another part of the paper this view is again referred to:—
 “According to the doctrine of the triple spectrum, the red space consists of *red*, *yellow*, and *blue* light, the *red* predominating, and the *blue* being extremely feeble. Now, the late Mr Troughton, whose colour blindness was examined by Sir John Herschel and myself, on separate occasions, saw this *red space*, *yellow*. Hence, according to my views, he saw a space containing much *yellow* and little *blue*, the *red* light being, as it were, absorbed, in consequence of the nervous membrane being insensible to its action. If this be the case, there must have been a diminution of light in the red space seen by Mr Troughton, and I am persuaded, from the experiments I made upon his eyes (he confounded with green leaves red petals which were far more luminous than the leaves), that this was the case; but whether it was to the extent of the total defalcation of the red rays I will not venture to assert. But it is not necessary that it should be so; *the defective perception of red light may be accompanied with a more acute perception of the other colours*, in a manner analogous to what takes place in the chemical spectrum, where the removal of the red rays produces an increased action of the rays which are left.”²

From these important observations it will be seen that Sir David Brewster has satisfied himself that a normal eye loses sensibility sooner to red light than to that of any other colour; that an eye defective from any cause in sensibility to light, will show its defect most when tested by red light; and that, in one well marked case of colour-blindness, red bodies appeared less luminous than others which were in reality darker. These observations are quite in accordance with the total insensibility of red, to which I have drawn attention in this section, and the confusion of red petals with green leaves, to which the author has referred, as proving the obscurity of red to Mr Troughton's eyes, is so nearly universal among the colour-blind, that I may thus far adduce Sir David as at one with me in regarding red as appearing dark or darkish when seen by them.

¹ Op. Cit. p. 136.

² Ibid. p. 139.

He holds, however, also, it will be seen, the remarkable opinion that the red light of the spectrum may appear much brighter, instead of much darker, to a colour-blind eye than to a normal one, in consequence of the former not perceiving the red, but instead thereof, yellow light of the same refrangibility, which makes a much stronger impression upon the retina.

Additional observations can alone determine whether the anticipated phenomenon ever occurs. Its occurrence, however, would not be at variance with the appearance of red or crimson bodies as black to a colour-blind eye, as they do not transmit or reflect yellow rays to affect the retina. It is otherwise with scarlet bodies, which should appear as bright yellow to eyes which simply ignored red. But it is with green, not with yellow, that scarlet is systematically confounded by the colour-blind, as a review of all the recorded cases will fully demonstrate, and the green thus identified with scarlet is not a pale or yellowish green, but a full, and often dark, shade of that colour. Thus, Dalton compared sealing wax to one side of a laurel leaf, and a red wafer to the other, and his doctor's scarlet gown to the leaves of trees; nor was his case in this respect peculiar, for the colour-blind are constantly found unable to distinguish the petals of the scarlet geranium from its leaves, the flowers of the wild poppy from the unripe corn amongst which it is growing, and the holly berry (or mountain-ash), and rowan berry, from their foliage. Moreover, those who thus mistake scarlet, regard green as darkish colour, and confound it with drab.

From these facts, coupled with the observed invisibility of red, I cannot avoid inferring that a colour-blind eye does not simply ignore red in a compound colour, or receive no impression from it, when it looks at scarlet, but that it sees the *red* as *black*; and I suggest that it is the mingled sensation of this black with the accompanying yellow, that causes scarlet, and even orange, to appear a drab-like green.

A similar phenomenon appears in connection with mixtures of red and blue. It is eminently characteristic of the colour-blind (as has been abundantly shown) to mistake such mixtures in all their shades from pink and crimson, to violet and purple, for blue. It perplexed me long in examining such cases to find that even the redder crimsons and the paler shades of these, such as occur in the lips, and in the petals of the fuschia, in the rose, and the pink, were spoken of by the colour-blind, in such decided terms as appearing blue to them, that I could not help inferring that they received the impression of a much deeper blue than normal eyes would have obtained from purples, or red-blues, simply deprived of their red.

Since, however, I have observed the total invisibility of red, I have inferred that the colour-blind do not see a red-blue as blue *minus* red, but as blue *minus* red *plus* black; in other words, the red is not merely abolished, but is replaced by black, and the reddest purple must, in such circumstances, appear a very dark blue.

I am thus inclined to regard the confusion of scarlet with green

and of purple with blue, as phenomena of the same nature, and depending upon the same cause as the confusion of red with black, from which they differ only in degree. I do not, accordingly, anticipate that the colour-blind will generally be found to see the red end of the solar spectrum brighter than others do, but the reverse, although it would be unwise to dogmatise on the peculiarities which may be found in detached cases of colour-blindness.

I have further to mention, that since reading these conclusions, I find that Prof. Dove has drawn attention to certain phenomena connected with the normal vision of red, which are important from the light they throw upon the subject before us.¹

Dove has frequently observed that on leaving a picture-gallery at the approach of night, a parting glance showed that "the red colour had altogether disappeared, while the blue appeared in all its strength. Artists," it is added, "are well aware of this fact." This is a phenomenon presented to the normal eye, plainly differing, only in degree, from that observed by the fox-hunter, who, as already mentioned, in dim twilight, lost the power of distinguishing a scarlet from a black coat.

The longer visibility of blue than of red in faint light, Dove has shown may be rendered manifest by looking at a stereoscopic combination, the halves of which are drawn in white on a black ground, through coloured glass.

If the one eye gazes through a red glass, and the other through a blue one, the red lines are the most vivid whilst daylight is intense, but, "as the twilight advances, the red becomes weaker and weaker; it finally disappears altogether, and instead of the relief formed by the combination of the red and blue outlines, the blue alone is observed as projection. . . . If two red glasses be now placed before the openings of the stereoscope, nothing whatever is seen; while with two blue glasses [even though ten times thicker than the red glass], the relief appears in blue lines, and remains distinctly visible for a quarter of an hour longer. Thus, the fact of the earlier disappearance of the red rays is placed beyond a doubt."²

It thus appears from the general experience of painters, and from the special trials of Brewster and Dove, that we all become sooner blind to red than to other colours, so that between us and the colour-blind, emphatically so-called, there is but a difference in degree, and they may with perfect justice insist upon applying also to us the name which we have devised for them.

¹ On the Stereoscopic Combination of Colours, etc. By H. W. Dove. Translated in abstract by Dr J. H. Tyndall. Phil. Mag., Oct. 1852, p. 241.

² Op. Cit. p. 247.

(To be continued.)

ARTICLE V.—*On the Statistics of Infantile Cholera.* By S. W. J. MERRIMAN, M.D., Physician to the Royal Infirmary for Children, London.

THE medical profession, and the public at large, through the profession, have great cause to thank the Registrar-General for the pains he takes to ensure accuracy in the bills of mortality. It is well known that, in the London weekly reports, the actual certificates given by the attending practitioners are published in some cases which are more especially interesting, but in the case of deaths from cholera, the entire details have been constantly published without omission, so that all who see these weekly reports are enabled to form their own opinion upon their correctness from the evidence thus laid before them.

It has seemed to me for some time past that an incorrectness exists in these reports in the number of deaths from cholera (so called) attributed to infants and young children. I have accordingly classified, in a new way, the returns of mortality from this disease for the last six years, and my suspicion appears confirmed by the results which I have thus obtained. As I examined the causes of death from "cholera," published week by week in London, I discovered several varieties of disease, which are denominated, and therefore classified as, "cholera." Sometimes the cause of mortality is set down to Asiatic or malignant cholera, and this at all ages; sometimes it is specified as English or summer; sometimes it is simply termed cholera; then there are other deaths attributed to cholera infantum, and to choleraic diarrhoea; but all are grouped into one column, headed "cholera." The idea which has entered my mind is, that the cases of cholera infantum, and some others occurring in very young children, should not be classified as "cholera" by the Registrar-General, as they differ from the well-known severe forms of that disease to a considerable extent; it then results that if these cases are omitted, there would be no unit small enough to express, at certain seasons of the year, the ordinary weekly average mortality from cholera, although at times, during epidemic seasons, the number of deaths from this cause would be very great for many weeks together. An inspection of the tables I have drawn up gives strong evidence on this point, and I hope that my professional brethren will consider the subject a little, for it is clear that the published returns must be classified according to the nomenclature adopted by the certifying practitioners, and that if they use the term "cholera," the compilers of the tables have no power to alter the classification, be it right or wrong; their power can only extend to making several divisions of this class, analogous to that which I have adopted; they would thus avoid the difficulty, but cannot solve it.

There is so large a mortality in infants and very young children from the debilitating effects of liquid discharges from the bowels,

arising from improper food, whether in its nature or the mode of its administration; from the unhealthiness of the locality where the children live, or from their own diseased constitutions; and from teething; that one is tempted to consider some of the returns of mortality which are attributed to diarrhoea by one practitioner, to cholera by another, as produced by the same essential disease: the difference in the name being caused by some slight differences in the symptoms, barely amounting to diagnostic divisions; or by the feeling in one practitioner, that the use of a strong term in his certificate must lead to an erroneous classification by the registrar, whilst another practitioner, not recognising the necessity the registrar is under to enrol the case under the name given by the practitioner, uses the strong term whenever the symptoms have exceeded in severity a standard fixed by himself.

Perhaps my argument will be better understood if I transcribe some of the returns for last year, which are credited to the "cholera" column, but which, I suspect, differ so far from the usual character of cholera, as to deserve a different heading. I find the death of a child, aged 4 months, set down to "cholera Anglica, 4 days;" "aged 3 months, English cholera, about 3 days;" "aged 3 months, summer cholera, 3 days;" "aged 6 months, infantile cholera, 13 days;" "aged 3 months, cholera infantum, 2 weeks;" "aged 9 weeks, chol. inf., 60 hours;" "aged 7 weeks, chol. inf., 7 days;" "aged 19 days, chol. inf., 3 days;" "aged 1 year, diarrhoea, 5 weeks, chol. inf., 1 week;" "aged 1 year and 9 months, measles, cholera, 4 weeks;" "son of a gentleman in Paddington, aged 4 weeks, cholera, 12 days;" "aged 1 year, chol. inf., 4 weeks, anasarca, 2 weeks." All these occur in the first nine months of 1853, and seem to be little other than varieties of diarrhoea, destructive from their duration, or from the tender age of the patient; yet being returned as cholera, they have been necessarily grouped with cases of undoubted cholera of extreme severity, as if caused by one and the same disease.

I have arranged the deaths from "cholera" under the following seven groups, viz., the cases reported as Asiatic or Indian; those called Malignant, Virulent, Spasmodic, Algide or Asphyxia; those returned as Epidemic or Endemic, which after some consideration I determined to classify separately, as I could not satisfy myself as to the exact meaning of the epithets; those returned as English, Summer, Sporadic or Bilious; as Cholera which in a few instances is also called Cholera sicca, and Cholera morbus; as Choleraic Disease, which I find described in the various certificates as Choleraic Diarrhoea or Disease, and as Choleraic Typhus or Fever; and lastly, the cases returned as Cholera Infantum or Infantile; and let me here remark, that three-fourths, at least, of these last cases are of infants under one year old, and that very few occur after the completion of the second year; though children of four or five, and one of nine years old, are returned as having died from this form of disease. I have grouped separately the mortality of each year up to the fifth birthday, then, of the five years together up to the ages of ten and

fifteen ; then of adults from fifteen to sixty ; and lastly, of all ages above sixty. The results show that in the three first tables, which should represent the most formidable cases, the mortality of infants is very small ; but that in the remaining tables the mortality is very large, the numbers in the first year exceeding much those immediately succeeding, except in the two epidemic years 1849 and 1853, which are exceptional in one respect, viz., the mortality from what is simply called Cholera ; but this apparent anomaly is in part explained by the observation, that in families where the adults have died from the more severe attacks of cholera, the children, as distinguished from infants, have succumbed in many instances to the same disease, and therefore the number of deaths in these two years, increases with the age of the children instead of diminishing.

My own experience among the children of the labouring classes, who dwell in such numbers on the south bank of the Thames, shows me that discharges from the bowels may be ordinarily grouped into three general heads, viz., Dysentery, Diarrhoea, where ordinary foecal matters are discharged, and a Diarrhoea where the motions are described as pale or colourless, and extremely liquid. These latter are much under the control of nitrate of silver in small and repeated doses, but they are frequently fatal from want of sufficiently early advice. I always return such cases as deaths from diarrhoea, believing them to belong properly to this disease, and not to cholera, for I find them frequently the sequel to a long-continued foecal diarrhoea, which has worn out the system before my advice was sought ; but it may be that some practitioners, arguing upon the rice-water appearance of the motions, may return the cause of death as cholera. If this is the true solution of the mystery, why so large a proportion of the deaths from "cholera," are those of infants, it is one which should be corrected ; but whatever be the cause of the use of the term cholera, I think that the deaths of very young children attributed to it should be excluded from the "cholera" group, and be inserted in the diarrhoea column, unless the Registrar-General should think it desirable to divide the general heading of "cholera" into several columns, each with its characteristic name, when, of course, these cases would fall into their respective groups, and we should have a correct idea of the number of deaths caused respectively by Malignant and by English Cholera, and by Choleraic Diseases, which last group might include the "Infantile" disease also.

There is too much reason to anticipate, with great dread, a return this year of the mortality from "cholera" similar to that of 1849, when the deaths from this disease alone amounted in one week to 2026 in the metropolitan district ; it is therefore highly desirable to distinguish in the future returns the cases of violent disease, which kill by the overpowering nature of their attack, or by the fever consequent upon the disease passing into its second stage, from those deaths caused by lengthened duration of a milder form in persons of tender age and unhealthy constitutions.

ANALYSIS OF RETURNS OF DEATHS BY "CHOLERA" IN THE LONDON DISTRICT.

ASIATIC CHOLERA.													MALIGNANT CHOLERA.													EPIDEMIC CHOLERA.												
Under Ages of													Under Ages of													Under Ages of												
Year	1	2	3	4	5	10	15	60	100	Total.	1	2	3	4	5	10	15	60	100	Total.	1	2	3	4	5	10	15	60	100	Total.								
1848	1	5	6	5	2	22	12	106	15	174	2	1	3	4	1	53	7	72	...	1	1	1	2	1	6	1	13							
1849	3	8	13	14	8	63	38	229	48	424	2	4	5	3	3	9	3	82	16	127	1	3	...	4	2	10							
1850	2	1	...	4	...	7	1	1	1	...	2	2	7							
1851	3	2	5	...	1	2	3	1	1							
1852	1	1	1	1	1	5	1	3	...	4						
1853	...	4	14	13	9	30	13	167	31	281	...	1	1	1	2	8	...	29	5	50	1	2	4	1	8							

ENGLISH CHOLERA.													CHOLERA.													CHOLERAIC DISEASE.												
Under Ages of													Under Ages of													Under Ages of												
Year	1	2	3	4	5	10	15	60	100	Total.	1	2	3	4	5	10	15	60	100	Total.	1	2	3	4	5	10	15	60	100	Total.								
1848	14	7	6	3	6	6	1	29	12	74	23	12	12	11	11	37	19	127	31	183	...	1	1	...	2								
1849	3	1	1	3	1	4	1	17	10	41	269	443	401	349	323	1069	548	7275	1795	12472*	11	7	4	1	2	1	1	15	9	51								
1850	13	7	...	2	2	6	1	17	5	53	5	4	1	4	1	5	1	15	4	40	...	1	1	1	...	4								
1851	24	8	4	1	1	1	...	14	13	66	12	3	1	2	...	14	12	44	...	1	3	2	6									
1852	22	6	2	...	2	3	...	15	12	62	6	2	2	2	1	1	1	11	12	38	1	...	1									
1853	14	7	3	2	2	5	...	20	14	67	14	31	16	15	13	38	20	182	28	357	9	7	7	4	3	4	18	7	62									

* N.B.—In the year 1849, the returns are printed almost universally "cholera," the epithet Asiatic, English, etc., being omitted. I have also included a few cases returned as "diarrhoea with collapse."

CHOLERA INFANTUM OR INFANTILE.

Not divided into ages, as three-fourths of the deaths occur under the age of one year.
1848, 27 deaths; 1849, 12; 1850, 16; 1851, 88; 1852, 52 (whereof 1 aged 9); 1853, 50 (whereof 45 under 1 year, 9 under 2, 3 under 3, 1 under 5, and 1 under 10.)

ARTICLE VI.—*Pelvic Abscess and the Fascia of the Pelvis in the Female.* By WILLIAM O. PRIESTLEY, M.D., M.R.C.S.L., late Vice-President of the Parisian Medical Society.

DURING the last few years the attention of the profession has been drawn, by several authors, both in our own country and on the continent, to the subject of pelvic cellulitis as occurring in the human female. In our own country, interesting contributions have been made to its pathology by Dr Churchill, Dr Doherty, and Dr Henry Bennet, in his last edition on "Inflammation of the Uterus." Our French neighbours have likewise made important researches into the subject. Practitioners now know the disease to be of common occurrence, although but a short time ago it was little known or understood. This change is doubtless to be attributed to the more accurate knowledge of the phenomena which constitute the disease, and the symptoms indicating its presence.

It is scarcely necessary to say, that by pelvic cellulitis is meant inflammation of the cellular tissue in the interior of the pelvis. This cellular tissue binds together the respective organs, and in some situations exists in considerable abundance, being more especially lax when surrounding those organs, which, in performing their functions, vary much in their degree of distension, or require free and easy motion within the pelvic cavity. Besides the loose cellular tissue, however, contained in the pelvic basin, there are strong processes or laminæ of fascia, which support the viscera, unite the organs with each other, and keep them in their respective positions. It must be evident that inflammation occurring in the interior of the pelvis, must be modified not a little, by these fascial arrangements. A phlegmonous tumour may be limited by the firm attachments, and if pus be formed, it will have a tendency to pass along the direction of least resistance, until it meets with some natural outlet, or is discharged at the surface of the body.

To illustrate the importance of studying the fascia of particular regions, where fluids may be influenced in direction, by its insertion or attachments, I may mention, that it is a fact well known to surgeons, that when urine is extravasated, in the anterior half of the perineum, it cannot pass backwards to the ischio-rectal fossa, on account of the reflection downwards, over the transverse perinæi muscles, of the deep layer of superficial fascia, to be united to the base of the triangular ligament. The knowledge of such an anatomical fact is of great value to the surgeon, and encourages the anatomist to study the fascia of other regions of the body, with the hope of still further benefiting surgery.

So far as I am aware, no anatomist in our own country has studied carefully the fascia of the interior of the female pelvis, with the view of elucidating the subject of pelvic cellulitis. Professor Paul Dubois, some years ago, felt the need of such an investigation, and M. Jar-

javay,¹ probably at his recommendation, published a more detailed and correct description of the pelvic fascia in the female, than had before appeared.

Professor Simpson, who was perhaps one of the first in this country to teach publicly that pelvic cellulitis was, from its importance and frequency, worth careful and separate consideration, had been in the habit of directing the attention of his pupils to the fact, that, until the fascia in this region was carefully studied by some anatomist, with the special object of explaining some of the phases of this disease, its pathology could not be clearly understood.

At Dr Simpson's suggestion, the writer, while in Paris, made some dissections, at the *École de Médecine*, of the parts in their normal condition, taking advantage, at the same time, of the presence of a number of cases of pelvic cellulitis in the hospital wards, to study the positions of the phlegmonous tumours, and the symptoms caused by their presence.

The dissections of the pelvic fascia were made, by carefully reflecting upwards the peritoneum lining the pelvic cavity, removing the fat and lax cellular tissue, emptying the containing sheaths of their muscles, while the sheaths themselves were left as entire as possible; and removing any such portions of bone as hindered the dissection, or obstructed the view into the anterior. After this the viscera were slightly distended, the fascia and peritoneum stretched in their normal positions, and the whole dried at a moderate temperature. The bones were previously cleaned, and deprived of any fat they might contain. The whole being now varnished, the fascia became transparent, and the relations of the several laminae were much more distinct and evident, than if only seen in the moist and flaccid condition. The following description, although agreeing to a considerable extent with that of M. Jarjavay, still differs in some important particulars, as will be seen in the sequel. It seems necessary, in order to make as clear as possible a subject difficult to comprehend without actual dissection, to give a detailed description of the pelvic fascia, although a repetition of what is already published in our anatomical works, thus becomes unavoidable. Some notes of cases are introduced illustrating the different positions of tumours, or the course taken by pus in seeking an outlet. These, it is hoped, may add interest to the subject.

It is convenient for facility of description, rather than from any existing arbitrary division, to speak of the fascia in the interior of the female pelvis, as consisting of three systems. The first, and that which forms a framework for the rest, is the "superior pelvic fascia" of Cruveilhier, closing in the lower outlet of the pelvis, and perforated in the median line by the bladder, vagina, and rectum; of the other two divisions, one is placed above this, and forms connections between the organs before perforating the pelvic fascia, and between

¹ Archives Générale de Médecine, 1849.

the organs and the bony boundaries ; the remaining system is below the proper pelvic fascia, enclosing muscles, vessels, and nerves, and constitutes the perineal fascia.

The superior pelvic fascia is firmly adherent to the bony walls of the true pelvis, from behind the symphysis pubis to the side of the sacrum, and between these two points has the following attachments and disposition :—Commencing anteriorly at the upper angle of the pubic arch, the attachment of each lateral half, curves slightly upwards to reach the superior margin of the obturator foramen, along the upper border of which it is fixed as far as its outer boundary, where it leaves a foramen for the transmission of the obturator vessels and nerves. Now, passing backward, a little below the level of the brim of the pelvis, it seems to terminate by an abrupt and strong insertion at the anterior and upper limit of the great sacro-sciatic foramen ; but again proceeding backwards, it terminates at the side of the sacrum, reaching as high as the first sacral vertebra.

Passing horizontally backwards from its attachment behind the symphysis pubis, above the angle of the pubic arch, and closing in the interval between the anterior borders of the levatores ani, this fascia encounters the neck of the bladder and vagina. These canals it envelops in a common sheath of considerable strength, a thin fibrous lamina, however, passing between the two, and making two separate compartments in the common sheath. So intimate is the connection between the vagina and neck of the bladder, caused by their investing fascia, that until the latter canal opens into the vesical cavity, it seems imbedded in the anterior wall of the vagina. The fascial attachment of the bladder and vagina to the pubic arch, is further strengthened by two bands of fibrous tissue, passing from behind the pubis to their investing sheath, separated only by a small interval in the median line, and lying above the urethra. They are designated by anatomists as the anterior true ligaments of the bladder, but are nothing more than the fascia, strengthened by two additional bundles of longitudinal white fibrous tissue.

The portion of pelvic fascia, which takes its origin from the upper border of the thyroid foramen and brim of the true pelvis, descends vertically, and slightly inwards, over the upper part of the obturator internus muscle, which it covers, until it reaches a white tendinous line (pubo-ischian), when it follows a new direction, which is nearly horizontal, until it reaches the side of the vagina and the most inferior portion of the bladder. Here it joins the fibrous sheath, just mentioned, as enclosing the vagina and neck of the bladder, and with it expands itself upwards as a cellular layer on the fundus of the bladder and neck of the uterus.

At the anterior margin of the great sciatic notch, the fascia which descends vertically over the obturator internus muscle, is firmly united with the periosteum, in a line running from the brim of the pelvis, to the spine of the ischium (ilio-sciatic line) ; by this arrange-

ment a partition is formed between the obturator internus, and pyriformis muscles.

A thin fibrous lamina, pierced by foramina, for the transmission of the sciatic and gluteal vessels, passes backwards and inwards from the ilio-sciatic line, to the lateral aspect of the sacrum. This is the fascia of the pyriformis muscle. It adheres to the bone above the great sacro-sciatic foramen, as far back as the origins of the pyriformis muscle; below it is united to the upper border of the lesser sciatic ligament, from which it is again reflected inwards, over the coccygeus muscle towards the rectum. Posteriorly, this portion of fascia is united to the periosteum of the sacrum, internal to the sacral foramina. By this arrangement the lamina of the two sides converge towards each other inferiorly, in such a way, that in front of the last piece of the sacrum, they are united; and in conjunction with other fascia, form a tube-like investment for the rectum, as this organ passes downwards in the median line to the outlet of the pelvis.

The superior pelvic fascia, from the arrangement just described, forms a floor to the cavity of the pelvis, which is sufficiently strong to support such of the abdominal viscera as may occasionally rest upon it, and firm enough to retain in situ the pelvic organs which perforate it.

It may be divided into a vertical and horizontal portion; the former is parietal, covering the upper part of the obturator internus muscle and pyriformis, while the latter, visceral, is reflected inwards towards the viscera. The horizontal portion, taking the two sides together, forms a lozenge, presenting four angles and four borders. The anterior and posterior angles are respectively at the pubis and sacrum, while the lateral ones are at the spines of the ischia on either side. Each anterior border is limited by the pubo-ischian line, and the attachment to the upper border of the lesser sciatic ligament, stretching from the lateral to the posterior angle, forms the posterior boundary of the lozenge. The posterior angle is not complete like the anterior one, as the central portion of the sacrum intervenes between the insertion of the fascial laminæ internal to the sacral foramina.

This strong diaphragm, formed by the superior pelvic fascia, completely separates the pelvic cavity from the perineal region below it, and scarcely any vascular communication exists between the two. The only vessels at all, of comparative size, which pass out of the cavity of the true pelvis by perforating its fasciæ, are the branches of the internal iliac artery, which pierce the pyriformis fascia and the obturator artery, which passes through a foramen at the upper boundary of the obturator fascia. This isolation of one region from the other, and the non-existence of vascular communication, accounts for the facts, that seldom or never do phlegmons, contained in the cavity of the true pelvis, implicate the cellular tissue in the perineal region. If pus be formed, it will make a circuit round the organs

contained in the pelvis or force a passage upwards, if before it finds no outlet, rather than pass downwards into the ischia-rectal fossa. This is because most resistance is opposed to its downward progress by the superior pelvic fascia. Where the foramina are left, however, for the passage of vessels out of the pelvic cavity, pus may escape from the interior, although with difficulty where no apertures exist. Mr Hawkins¹ gives a case where an abscess escaped by the great sacro-sciatic foramen, the formation of pus being determined by caries of the anterior aspect of the sacrum. Fluctuation was discovered in the gluteal region, and an incision being made by the side of the great sciatic nerve, the probe passed into the pelvis by the great sacro-sciatic foramen. Mons. Chomel also mentions that in a patient who had abscess in the interior of the pelvis, the pus escaped with the vessels, through the obturator foramen, and formed a tumour on the inner part of the thigh.²

The iliac fascia, although not contained in the cavity of the true pelvis, is no doubt a portion of the proper pelvic fascia. That it is a continuation of this system seems proved by the fact that although generally adherent to the brim of the pelvis, the attachment along the brim may be wanting. Marchal de Calvi³ records that an abscess occupying the compartment formed by the fascia enclosing the obturator muscle had passed upwards under the fascia iliaca, and so into the thigh, where it formed a tumour. Generally, however, the iliac fascia, which is much more delicate in its posterior half than it is anteriorly, is attached to the inner lip of the crest of the ilium; from this stretching inwards over the psoas and iliacus muscles, it passes beneath the iliac vessels, and becomes firmly adherent to the ilio-pectineal line. Upwards it is attached on the inner side of the psoas to the sides of the bodies of the lumbar vertebræ and their ligaments, as far as the diaphragm. The two muscles, iliacus and psoas, are, however, separated by a thin lamina which runs inwards towards the brim of the pelvis. Anteriorly the iliac fascia is united to the fascia transversalis, and the outer two-thirds of Poupert's ligament. Internal to this it passes into the thigh, forming the back part of the sheath of the femoral vessels, and is continuous with the pubic portion of the fascia lata. Intervening between the iliac fascia and the peritoneum, which lies above it, is much laminated cellular tissue, and a thin aponeurotic layer, which takes its origin among this cellular tissue, and proceeds over the iliac vessels to enter the broad ligament of the uterus.

When inflammation attacks the cellular tissue, in one or other iliac fossæ, the tumour may be situated above or beneath the iliac fascia, or occupy both positions at the same time. If beneath the fascia, the swelling will be felt deeper seated, and should abscess

¹ London Med. Gazette, Sept. 1832.

² Gazette Médicale. T. i., p. 328.

³ Des Abces Phlegmoneux Intra-pelviens. 1844.

form, it is most likely to pass into the thigh, bulging somewhere near the small trochanter, as in psoas abscess, its passage in this direction being favoured by the loose tissue present to facilitate the motion of the flexor muscles, which pass under Poupart's ligament. Abscess occurring between the peritoneum and the anterior aspect of the iliac fascia, generally extends backwards towards the loins, or forwards towards the fold of the groin. The progress backwards is favoured by the lax cellular tissue, and the horizontal position generally assumed by the patient. If primarily in this situation it seems to have little disposition to seek the pelvic cavity by passing inwards, as the tissue is denser passing over the iliac vessels than it is in an anterior or posterior direction.

The following are notes of a case taken in the Parisian Hospital "La Clinique," where a "sub-cæcal" abscess destroyed nearly the whole of the sub-peritoneal cellular tissue, in the iliac fossa, without passing into the cavity of the pelvis :—

CASE I.—Marie Patrie, æt. 16 years, entered as a patient in M. Nelaton's wards, Dec. 15, 1852. In the month of July, in the same year, she had suffered from fever, and subsequently made an imperfect recovery. Repeated attacks of diarrhœa followed, with pain in the bowels, more especially on the right side.

For a month before her entrance to the hospital these symptoms had become exaggerated, and in addition to the pain she had discovered a tumour a little above the fold of the right groin. This swelling approached the surface, and at last pointed and discharged a small quantity of pus, a little above Poupart's ligament. When examined in the hospital, she seemed feeble and threatened with hectic. A large tumour was found occupying the right iliac fossa, and a probe introduced into the aperture above Poupart's ligament passed easily in a direction backwards for several inches. From the constitutional symptoms, M. Nelaton had no doubt the suppuration was considerable in extent. The aperture was enlarged and kept open, and stimulants with other appropriate remedies were given, but the patient sank and died in about a fortnight after her admission to the hospital.

At the post-mortem examination a large abscess was found occupying the right iliac fossa. Below it was bounded by Poupart's ligament, and passed upwards in the sub-peritoneal cellular tissue nearly as far as the kidneys. Internally its extent was from the crest of the ilium to the brim of the true pelvis ; but it passed no further inwards towards the pelvic cavity, being opposed here by condensed tissue. The peritoneum formed the roof of the abscess, while the iliac fascia formed the floor, the muscles below being little injured. The purulent fluid, which was considerable in quantity, was circumscribed laterally and in front by a condensed pyogenic membrane, but posteriorly this membrane did not exist, the pus everywhere infiltrating and breaking up the cellular tissue as far as the lumbar region. The cœcum was surrounded by the abscess, and its cellular attachments in a great measure destroyed. Large patches of ulceration were found on the mucous membrane of the cœcum ; some progressing and others healing ; but no perforation could be discovered. The iliac vessels were not obliterated, but their coats were thickened, and their calibre slightly diminished.

But again, a phlegmon, when of large size, is more likely to be prolonged upwards into the iliac fossa, when primarily situated in the pelvic cavity, as the resistance is much less upwards than that offered by the floor of the pelvis in the opposite direction. Examples

of this are frequently met with in practice ; the following one is of this nature :—

CASE II.—Eugénie Placide, æt. 24, entered the Hôpital de la Pitié, Jan. 12, 1853, under the care of M. Nonat. She had been confined twenty days before, in the clinical wards of M. Dubois, and had suffered violent pain afterwards in the abdomen. She had been discharged at the expiration of ten days after delivery. When examined by M. Nonat, the patient had considerable fever, and in the left side of the abdomen, where acute pain was complained of, a tumour of considerable volume was discovered. It occupied the iliac region, and descended for three or four fingers'-breadth above the level of the pubis. The surface of the tumour was hard and smooth. On making a vaginal examination, the neck of the uterus was found much engorged, and very painful to the touch ; the whole of the interval between the uterus and pelvic walls was occupied by a tumour of large size. This tumour was almost immovable, hard, and extended above the brim of the pelvis, where it might be felt to be continuous with the swelling in the iliac fossa. In this case no suppuration was detected. By antiphlogistic measures and local bleedings the tumour gradually diminished in size, and on the 14th of Feb. she left the hospital, not well, but apparently fairly recovering.

The disposition of fascial laminae, situated above the true pelvic fascia, which I have thought it better to describe as a separate division, has, until lately, been thought unworthy of more attention than is generally given to loose cellular tissue filling recesses, or interposed between organs to facilitate their motion. In this general way it is mentioned by authors as subperitoneal cellular tissue. It differs, however, much from such collections of areolar and fatty tissue, as we find for instance, in the ischio-rectal fossa, inasmuch as, in some positions, it is condensed and laminated, and is sufficiently strong to give direction to pus or other extravasated fluid.

If, then, the peritoneum be carefully raised from the iliac fossa on either side, a quantity of laminated cellular tissue before mentioned, is seen interposed between that membrane and the iliac fascia. This, taking the character of a fascia as it approaches the cavity of the true pelvis, forms the upper part of a sheath which encloses the iliac vessels and some lymphatic glands which accompany them. The lamina does not end here, however, but proceeds inwards to the pelvic cavity, and becomes attached to the contained organs. Anteriorly, it first encounters the round ligament of the uterus, which it sheaths and accompanies as far as the inguinal canal. Here it takes a firm insertion by additional fibrous tissue, to the external and inferior boundary of the internal abdominal ring, and thus separates the round ligament from the iliac vessels. Now dipping downwards it encounters the obturator vessels and nerves, gives them sheathing processes, and then uniting itself to the obliterated hypogastric artery, it passes to the side of the bladder, where it disappears as loose cellular tissue. In the whole of its extent the layer is fascia-like, but thin and transparent, except that portion which intervenes between the obliterated hypogastric artery and the bladder, which is more lax and filled with pellets of fat.

A little posterior to the round ligament of the uterus the same lamina after crossing the iliac vessels, follows the ovarian vessels to join the fibrous capsule of the ovary, and is lost behind on the sheath of the common iliacs.

From its attachment to the ovary it sends a division vertically downwards to form a septum, which is described by M. Jarjavay¹ as the "anterior aponeurosis of the broad ligament." This aponeurosis is oblique in direction and divides the lateral half of the pelvis on each side of the viscera into a larger anterior portion, and a smaller posterior one.

Somewhat quadrilateral in form, it is attached along the ilio-sciatic line externally, internally it is united to the fibrous coverings of the vagina and cervix uteri, while inferiorly, where it is strongest, it is inserted into the superior pelvic fascia, in a line which corresponds to the partition between the levator of the anus and the coccygeus muscles which lie beneath. Behind this septum are the divisions of the internal iliac vessels, which supply the pelvic organs, imbedded in loose cellular and fatty tissue. The obliterated hypogastric artery perforates this anterior aponeurosis, and receives a sheathing process from it as its trunk passes forwards. The partition thus formed is thin and easily broken down above, near the brim of the pelvis; at its insertion below it is strong and distinct. It would no doubt oppose the passage of fluids from before backwards, or in an inverse direction. The anterior portion of the lateral half of the pelvis in front of it is filled with lax areolar tissue, without any resisting portion, as far as the anterior boundary, and this is even continuous with the other side round the front of the bladder; so that there is ample room here for a large inflammatory tumour, and the loose structure there present, would favour the diffusion of pus.

Although the cellular tissue, as just mentioned, is continuous in front of the bladder with that of the opposite side, it does not seem to be prolonged between the bladder, and vagina and uterus. On each side strong white fibres pass forward from the side of the vagina and neck of the uterus to the lateral aspect of the bladder, and thus a compartment is formed containing loose tissue between the vagina and base of the bladder. This compartment is occasionally the seat of a circumscribed phlegmon, an instance of which I will describe shortly with two other cases.

When pelvic cellulitis is present the tumour is frequently found somewhat behind and to the side of the vagina, but entirely in a lateral position as regards the rectum. Very probably in these cases the cellular tissue behind the vertical septum is inflamed. The increased vascularity from the presence of many vessels in this compartment, we should expect to predispose it easily to take on inflammatory action.

¹ Loc. cit.

Three different positions of pelvic phlegmons is shown in the following cases :—

CASE III.—Caroline Grozenger, æt. 23, became a patient in the Hôpital de la Pitié, the 14th of January 1853. Two years before she had been married, and since that time had suffered much from ill health. The menstrual periods, which before marriage had been regular in recurrence, since then had only occasionally appeared. Two months ago the patient began to experience severe pain in the lower part of the abdomen, more especially on the right side, and she noticed that her body was slightly swollen. She suspected herself pregnant, and this idea was strengthened by her having morning sickness and swollen breasts. A scanty menstrual discharge appearing dispelled the illusion, and the pain and swelling increasing, she sought relief at the hospital.

On examination, the abdomen was found swollen by some ascitic effusion, but no great tension was present. Through the vagina the cervix uteri was felt to be augmented in volume, and directed slightly to the right side. To the left, between the uterus and the pelvic parietes, the finger encountered an elongated tumour about the size of an orange, which mounted up as high as the pubis, and could just be felt in that position through the abdominal walls. Its surface was even, somewhat elastic, and painful to pressure. It seemed united at one extremity to the neck of the uterus, and encroached somewhat on the upper part of the vagina. The tumour could be felt through the rectum, but was too far forward to be well defined when examined by organ. There was considerable fever, and a pulse of 95 pulsations. There had been no pain in making water, or in emptying the bowels. The woman had a decidedly chlorotic appearance, and a "bruit de diable" was heard over the vessels of the neck. This patient was cupped several times over the abdomen, and kept on low diet, but for some days made no improvement. The swelling increased, shooting pains were complained of in the left thigh, as well as in the abdomen, and once a distinct rigor occurred. Diuretics were afterwards prescribed with tonics; and when discharged from the hospital at the end of a month she seemed much better. The effusion had in a great measure been absorbed, but the pelvic tumour remained.

CASE IV.—A patient, who had had a pessary introduced for retroversion of the uterus, was seized with rigors and pain in the lower part of the abdomen. Feverish symptoms followed, and the pulse reached 120. Great pain in passing water supervened, and frequent desire to void the urine.

On examination by the vagina, a flattened tumour was felt in front of the finger, as much as three or four inches in circumference. It was elastic to the touch, and painful on pressure, reaching a little higher than the vaginal cul de sac. When a sound was introduced into the bladder, the tumour could be felt between it, and the finger placed in the vagina. Leeches were in the first instance applied to the abdomen, with fomentations; afterwards counter-irritants were employed for several weeks. Under this treatment the tumour began to disperse, and at length disappeared.

CASE V.—Marie Louise Vachette, æt. 41 years, placed herself under the care of M. Nonat, the 22d Sept. 1852. The patient was the mother of fifteen children, the youngest being then four years of age. She asserted that after each confinement she had been in the habit of leaving her bed on the fourth day, and had never experienced any inconvenience until delivered of her last child. She left her bed on the third day after this confinement, and in consequence took cold; the lochia being suppressed, and a feverish attack of no little intensity followed. Violent pains were experienced in the left side of the abdomen, and in the left hip bone, and tenderness over the abdomen was present. These pains and tenderness had been constant up to the time of her entering the hospital, and although many simple remedies had been tried dur-

ing the four years, no relief had been experienced. The abdomen, on examination, was found somewhat tense, and tender to pressure. By placing the hand on the hypogastrium, a solid and movable tumour was found rising above the pubis. This was found to be a voluminous uterus, and through the vagina the cervix was felt enlarged and painful to the touch. The sound passed easily into the uterine cavity, and showed it to be about five inches in length. In the broad ligament to the left of the uterus, in the direction of the sciatic notch, was discovered also a smooth tumour about the size of a small egg. It was situated somewhat behind the uterus, and could easily be felt to the left side of the rectum. The tumour was immovable, elastic, not lobulated, or very painful to the touch, and some arteries of considerable size were found beating on its surface.

This was diagnosed as a chronically inflamed uterus, but was probably one of those cases of subinvolution described by Dr Simpson. The tumour in the broad ligament was believed to be a chronic inflammatory one of the cellular tissue in that situation, and this idea was considerably strengthened by finding that it was much diminished in size after bleeding, and the use externally and internally of iodine. The position of the phlegmonous tumour in this case, seemed to be in the cellular tissue enclosing the divisions of the internal iliac vessels.

(*To be continued.*)

ARTICLE VII.—*Surgical Cases, with Remarks.* By JAMES SPENCE, Fellow of the Royal College of Surgeons, Lecturer on Surgery, etc.

CASES OF STRANGULATED HERNIA.

CASE I.—*Strangulated Femoral Hernia; Division of Constriction External to the Sac; Recovery.*

Mrs R., æt. 70. I saw this patient with Dr Gordon on the 19th March 1849. Dr G. had been sent for in the morning, and found that the hernia had come down, or at least that it had been first noticed about twenty-four hours previously. He had tried to reduce it by the taxis, but without success. On examination, I found a small femoral hernia tense, unyielding, and very painful; there had been no motion from the bowels since the first appearance of the rupture, although enemata and purgatives had been given; there was constant vomiting and great general prostration; the belly was tympanitic and tender to the touch.

Under these circumstances, I put the patient under the influence of chloroform, having first obtained her consent to an operation if the taxis should fail. I then attempted the taxis when she was completely under the influence of the chloroform, and the parts were fairly relaxed by attention to position, but could not succeed. I accordingly proceeded to operate. I made an incision parallel to Poupart's ligament, and another perpendicularly downwards from it, over the tumour, so as to enable me to see the deep-seated parts at the neck of the swelling distinctly. After dividing the superficial fascia and some cellular tissue, the falciform process was exposed, the resisting fibres divided carefully; the probe-pointed bistoury guided on the finger was then insinuated gently between it and the hernial sac, and by turning the edge obliquely upwards and inwards a deeper constriction was divided. The sac and its contents were then drawn gently downwards, and a few fibres, apparently closely attached to the sac, and which seemed to constrict it, were scratched through with the bistoury. Holding the sac with one hand, I easily

returned its contents into the abdomen, and, feeling that it was empty and small, I invaginated it by pushing it upwards with my finger. I retained it within the ring till one of the gentlemen who assisted me in the operation closed the wound by sutures, the finger being withdrawn just before the last stitch was closed. A pad of sponge enveloped in lint was then placed over the opening, supported by a larger pad, and the whole secured by a spica bandage. An opiate was given after the effects of the chloroform had passed off. During the operation a little delay was caused by vomiting occurring while the patient was under chloroform, requiring her to be raised so as to prevent the fluid from passing down the air-passages. I saw her again in the evening; she seemed quite relieved; the vomiting and hiccup had ceased, and there was no tenderness of the abdomen. Next day she had a dose of castor-oil, which, however, did not act; two Colocynth pills were then given, and next morning I found the bowels had acted freely three times. The wound healed well; she recovered rapidly, and she was able to be up in about a fortnight after the operation.

CASE II.—Strangulated Femoral Hernia; Sac Opened; Recovery.

Margaret M., æt. 30, had been subject to femoral hernia for about eighteen months previous to my seeing her. The hernia used to come down occasionally, but was always easily returned, and she had never worn a truss regularly. About ten days previous to the attack for which I saw her, she had been delivered of a child, and was making a good recovery, when, having risen to make her bed, she felt the rupture come down. As it did not give her much uneasiness, she did not think of trying to reduce it till some hours afterwards, when she found it hard and painful, and then she could not succeed. As general pain and vomiting soon supervened, a medical man was sent for, enemata and other remedies were used, and the taxis tried unsuccessfully. I saw her in the afternoon, and found her suffering from very urgent symptoms. The abdomen was tense and acutely tender to the touch; constant vomiting, hiccough, and quick pulse. At that time, however, she would not submit to an operation, though the taxis had failed. She again sent for me in the evening, and I then operated. On dividing the tissues over the neck of the sac, I found I could not completely empty it, although it became more flaccid; it was therefore opened, and found to contain a portion of omentum, much congested, and slightly adherent. (The intestine had probably been returned before the sac was opened.) The omentum was returned, and the wound closed and dressed in the usual manner. Next day her bowels had been opened freely by a dose of castor-oil, but there still existed great abdominal tenderness and quick pulse. I therefore ordered hot fomentations to the abdomen, and directed her to take 2 grs. of calomel and $\frac{1}{2}$ gr. of opium every six hours.

Under this treatment the abdominal tenderness soon subsided, but the pulse still continued very rapid, and there was great nervous irritation; on inquiry I found she had suffered from flooding after her delivery, and lost a considerable quantity of blood. I therefore allowed her more nourishing diet and wine. Under this change of diet the pulse gradually came down to the natural standard, and she made a good recovery.

Remarks on Cases I. and II.—I have selected these two cases as, by contrast, illustrating certain precautions to be observed in reference to the extra-peritoneal operation for hernia. In both cases, strangulation had only existed for a comparatively short period, and both were therefore well suited for that plan of operation; and in both I proceeded to operate in accordance with it by dividing the stricture external to the sac.

In Mrs R.'s case, when this was done, I was able at once completely to empty the sac of its contents, and feeling it empty, to invaginate it within the femoral ring, satisfied that none of its contents could be then constricted.

In the woman M.'s case, although on dividing the constricting fibres external to the sac, part of its contents returned into the abdomen without pressure, I found that the greater part could not be returned from the sac; and though, from the hernial tumour being small, I daresay I could have reduced it *en masse* without much difficulty, such practice would in my opinion have been very *uncertain* and dangerous, because there might have been constriction of the irreducible portion by the neck of the sac itself; and therefore I considered it my duty to open the sac and satisfy myself as to the actual condition of the contents.

I hold that in all cases the complete reduction of its contents from the hernial sac ought to be made an invariable requirement in the extra-peritoneal operation, otherwise it will be apt to be brought into disrepute from reduction of small herniæ *en masse* with constriction still existing, and in all cases where the sac cannot be so emptied, the safest plan is to open it so as to make sure of the exact state of matters.

The case of M. is also useful as showing the necessity of constantly keeping in view the general state of the patient at or previous to the operation. The rapid pulse and irritable nervous state were evidently dependent upon her puerperal state and the loss of blood during parturition, and hence quickly disappeared under the use of opiates, nourishing diet, and wines; whilst an opposite treatment, from a dread of peritoneal inflammation, might have led to very serious results.

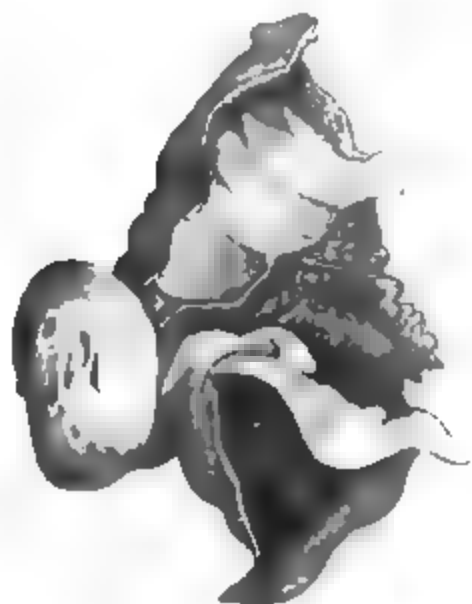
CASE III.—*Femoral Hernia Strangulated for Four Days; Operation, with Relief to Obstruction; Death from subsequent Perforation of the Bowel.*

Barbara L., a feeble woman, 76 years of age, from Fenton, East Lothian, was admitted into the Royal Infirmary August 25, 1852, on account of a femoral hernia, which had been strangulated for four days. Repeated attempts at the taxis had been made before her admission into the hospital; and the hernial swelling, which was about the size of a large walnut, was excessively painful even to the slightest touch.

I saw her about half an hour after her admission. From the state of the hernia, and the patient's general symptoms, I made no further attempt to reduce it, but proceeded at once to operate, after the patient had been put under chloroform. The integuments and superficial fascia were of the usual appearance, but the textures immediately over the sac were covered with recent lymph, and there was a small quantity of pus immediately external to the sac. On opening the sac, it was found to contain a small portion of bowel, very dark-coloured, congested, and granular, but not gangrenous. It was very slightly adherent to the sac at one point by effusion of recent lymph. It was replaced, and the wound dressed in the usual manner. The patient had an opiate after the operation.

August 26.—Patient much relieved; has slept well. Slight tenderness on pressure over abdomen; pulse 90. She had a warm water enema, and subsequently a dose of castor-oil, which moved the bowels slightly.

27.—Passed a good night.



Tenderness over abdomen increased; edges of wound inflamed, and no adhesion; enema repeated; bowels freely moved.

28.—Very restless; pain much increased; pulse 110, wiry. Calomel and opium were now given. The pain increased rapidly, with symptoms of sinking, and she died on the 29th of August.

Post-mortem Examination (thirty hours after Death).—The wound looked sloughy, and the peritoneal surfaces presented traces of acute inflammatory action. The portion of the bowel which was in the sac was not adherent at any point, but floated loose. It preserved an indented appearance, and was dark coloured. It had not given way by a large slough, but was perforated by a number of minute openings (as represented in the accompanying wood-cut), through which the serous portion of the feculent matter had exuded into the peritoneal cavity.

CASE IV.—Femoral Hernia Strangulated for Six Days; Operation; Recovery.

Mrs Robb, *ætat* 70, was seen for the first time by Mr Wallace, one of my pupils at the Royal Dispensary, on the evening of Monday, the 29th November 1853, who, finding that the hernia had been down from the preceding Tuesday, that the symptoms had commenced shortly after its descent, and that there was now feculent vomiting, requested me to see the case with him.

I found the patient a feeble woman, with anxious expression of countenance, quick small pulse, surface of the body cold, and her face covered with a clammy sweat. There was almost constant vomiting of thin feculent matter, and frequent hiccup. The abdomen was tense and painful; the hernial tumour was red, and tender to the touch. I was informed that, immediately after the rupture came down, she had passed a loose stool, but that since then she never had had any motion from the bowels, and the vomiting had continued almost from the commencement. As she was subject to biliary derangements, she had not applied for medical aid until the symptoms became excessively severe, and all medicine had failed to act on the bowels. Under these circumstances, I at once proposed the operation, as her only chance of safety. She readily consented; and Dr Gordon having administered chloroform, I divided the integuments by a T incision, so as to expose the neck of the sac and the falciform edge fully. Finding that there was no fluid in the sac, so as to enable me to pinch it up from the intestine, I insinuated a very thin probe-pointed bistoury between it and the falciform edge, and divided the resisting fibres. This enabled me to draw down the sac a little further, and to bring into view a portion of it which I was able to separate and pinch up. I now opened the sac carefully, and enlarged the opening with the probe-pointed bistoury sufficiently to allow me to introduce my finger. I found the bowel adherent to the interior of the sac by soft lymph; this I gently broke up, and, guiding the bistoury on my finger, I opened the sac fully, so as to expose its contents, which were found to consist of a small knuckle of intestine, of a dark purplish brown colour, granular, and presenting no glistening surface. The adhesions to the sac were quite soft, and easily broken up. The history of the case, and the appearance of the bowel, made me very doubtful of returning it; but as from the light I could not be quite certain of the shade of colour, and as there was no gangrenous smell, I replaced it gently, and only closed the wound partially, supporting the parts very lightly with a pad of sponge enveloped in lint, and retaining this by one or two turns of a bandage. An opiate was then ordered to be given in

about an hour after the operation. Next day, I found the patient had passed a good night; the pulse was about 98, the vomiting and hiccup had ceased, and there was less pain and distension of the abdomen. On removing the dressings, and finding no gangrenous smell, I reapplied the pad and bandage. As she was very thirsty, I desired her to keep a little ice in her mouth occasionally to allay the constant desire to drink water, which might have induced vomiting. On the following day she had a laxative enema and two Colocynth and Hyoscyamus pills, which procured two free evacuations from the bowels. This was followed by complete relief of abdominal pain and distension; the pulse gradually fell to its natural standard, and she made an excellent recovery, requiring little or no treatment, except regulation of diet, and the occasional exhibition of some gentle laxatives and tonics.

CASE V.—Scrotal Hernia—Strangulated seven hours; Operation; Recovery.

Mr ———, æt. 63, of spare habit of body, and suffering for many years from an aggravated form of dyspepsia and biliary derangement, had long been the subject of an inguinal hernia, for which he had worn a truss.

On the morning of the 29th May 1852, on getting out of bed the hernia came down suddenly, and to a larger extent than usual. He returned to bed, and attempted to reduce the rupture, but failed; and feeling the swelling larger, more painful than usual, and accompanied with sickness and other unpleasant symptoms, he sent for his medical attendant, Dr Burn, who came immediately and tried the taxis; but finding that steady and regular pressure did not succeed, he requested me to see the patient with him.

I accordingly visited the patient and found a pretty large scrotal hernia on the right side, tense and rather painful on pressure. I was told that the hernia was in general easily and completely reducible. As the protrusion had only existed about four hours, and as the swelling seemed to have suffered from the attempts of the patient to reduce it, I stated to Dr B. that before making any further efforts at reduction, it would be as well to apply cold to the tumour and to administer an enema. The patient was requested not to press the swelling at all, and it was arranged that we should see him again in about two hours.

Accordingly, at two P.M. we again visited the patient. The tumour was much more tolerant of pressure, and from this circumstance and a deceptive feeling of yielding, both I and my friend Dr Duncan, who kindly accompanied me, thought it would yield to steadily maintained pressure. We accordingly tried the taxis persistently for nearly half an hour. At the end of that time, finding that no progress had been made, and the patient being under chloroform, I proceeded to operate. After dividing the coverings of the tumour by an incision 3 inches long, and the textures at the deep ring external to the sac, I found the hernia still tightly constricted a little lower down. I opened the sac, and introducing my finger, divided the constriction from within in the usual manner. The bowel was now seen to be much congested and ecchymosed, and of a dark port-wine colour, but presenting a smooth glistening appearance; it was now easily reduced.

The wound was closed by suture, and a pad and bandage applied. An opiate was then given. I visited him in the evening and found him free from pain, with no vomiting, but suffering a little from flatulent distension. The opiate was ordered to be repeated at bed-time, and a dose of castor oil to be given in the morning.

At my visit next day, I found him still keeping free from any urgent symptom, but the bowels had not been acted on, and the pulse was about 120 and small. At my visit in the evening I found the bowels had acted twice freely, and the flatulent distension was much relieved. He had no sickness, abdominal tenderness, nor any bad symptom, except the quick pulse which seemed to depend on the use of the opiates, for on their gradual withdrawal the pulse began to fall and speedily attained its natural standard.

The part of the wound corresponding to the deep ring was slow of healing, thin pus being discharged from it for some weeks ; but in every other respect the cure went on favourably, and he was able to wear a truss and be out on the 12th of July.

CASE VI.—*Strangulated Oblique Inguinal Hernia ; Operation ; Recovery.*

On the 23d of May last, I received a message from Dr Cruickshank of North Berwick, to come and operate on a case of hernia in that neighbourhood. I reached the patient's residence about a quarter to ten A.M. Dr Cruickshank informed me that the patient, H——, an old man about 73, had for sometime been under treatment for a severe attack of pneumonia, from which he was slowly recovering. That on getting up the preceding night about eight P.M., the rupture, which had existed for many years previously, came down, but he thought nothing of it till he felt it painful and became sick, and he then attempted to return it, which he could usually do with great ease, but could not succeed though he made great efforts. Dr Cruickshank was then sent for, who saw him early in the morning, found the swelling hard and tense, and after giving an enema he tried reduction by the taxis for some time, and then feeling satisfied that an operation would be required, he sent a message for me in time to leave by the first train, so that I saw the patient within thirteen hours from the time the rupture first came down. From the urgent vomiting, abdominal pain and general state of the patient, and the tense and tender condition of the hernial tumour I felt it would be injudicious to try the taxis. I therefore having obtained the patient's consent to an operation, put him under chloroform and operated in the usual manner.

Finding that the stricture was in the neck of the sac, which appeared deeply indented, I opened it carefully at one point where I could pinch it up, and then enlarging the opening, I introduced my finger so as to guide the bistoury and slit open the sac more fully. This exposed the gut, which was a knuckle of small intestine about three inches long, of a dark purple colour, granular, and covered with flakes of adherent lymph. There was no fluid in the sac, merely a soft gelatinous-looking effusion. The constriction seemed deeply indented, and so tight that I could not get my nail within it. I accordingly made the bowel be drawn gently down, and with the probe-pointed bistoury, I notched the constricting edge sufficiently to enable me to introduce the point of a flat director between it and the gut, and then divided the deep stricture directly upwards. A large quantity of dark serum now escaped from the abdomen. I next drew down the bowel very gently to examine its condition at and above the constricted part. I found it looking sound above, but the knuckle which had been protruded seemed thin and covered with lymph, and retained its indented and constricted appearance after all constriction had been removed. Still, as it was not gangrenous, I replaced it gently within the abdomen, closed the wound and bandaged the patient. I did not see this patient again, but I learned from Dr Cruickshank that he made a very good recovery.

CASE VII.—*Strangulated Inguinal Hernia ; Operation on the fourth day ; Recovery.*

G. R——y, æt. 39, coach-painter, was admitted into the hospital, September 27th, 1852. He states, that ten years ago he first felt a swelling in right inguinal region. The rupture came down sometimes as often as once a fortnight ; at other times, not for a period of six months. Four years ago he felt a similar, but smaller lump, come down occasionally in the same region on the left side.

On the night of the 23d September, or very early on the morning of the 24th, the hernia came down on the right side. This was an unusual occurrence, as it formerly had generally come down during the day. He felt very

severe pain in the part, and found he was not able to reduce the bowel, as he had been before accustomed to do. He went to his work, however, on the Friday morning, but feeling ill, he returned to his lodging, but did not go to bed. All the food he took was vomited. He continued in this state till Monday the 27th, when he applied to Mr Lizars. Mr L. was unsuccessful in his efforts to reduce the hernia, and recommended him to apply for relief at the hospital, which he did in the evening at half-past seven P.M., when he was admitted.

On admission ; he has evidently been drinking recently, and his manner is excited. He says he only vomits when he takes food. The tumour is excessively tense, and the size of a small orange. He was put into a warm bath, and the taxis carefully employed, but without avail. At half-past nine P.M. Mr Spence operated for the relief of the hernia. On opening the sac, a large quantity of yellowish, but clear serum, exuded from the wound. The bowel was dark-coloured, and had a granular appearance. There was a small spot resembling a clot of blood sticking to the gut. The stricture having been divided, the bowel was returned, and the external wound brought together by three points of suture. A compress of sponge, with a folded towel, was applied to the wound. This was kept in accurate apposition to the walls of the abdomen by a spica bandage. After awaking from his chloroform sleep, he said that he felt quite easy. 50 drops of sol. morph. were shortly after administered, and a second 50 drops had, after an hour's interval, the effect of sending him to sleep.

Sept. 28th—Continues to feel easy : had an injection of warm water and castor oil at eleven P.M., which opened his bowels freely, after which he felt more comfortable ; no tenderness of abdomen on percussion.

Sept. 29th—Bandage changed by Mr Spence ; suture removed ; wound looks well.

Sept. 30th—The bandage having become loose was reapplied : the remaining sutures were removed, on which some discharge exuded from the wound. Patient feels quite well, and even expressed a wish to sit up. He is allowed low diet for the present.

Oct. 9th—Has continued to improve since last report. A few days ago he caused the upper part of the wound to gape by getting out of bed contrary to orders. He is quite comfortable, and feels quite well.

Oct. 13th—A very small portion of the wound remains unhealed ; in all other respects he is quite well.

Oct. 29th—Is quite well, and wears a double truss, as there is a decided tendency to hernia on the opposite side.

Nov. 1st—Dismissed cured.

CASE VIII.—*Strangulated Umbilical Hernia ; Operation ; Recovery.*

On the evening of the 24th November 1851, I was requested by Dr M'Cowan to visit Mrs M'G——, æt. 65, who was labouring under symptoms of strangulated umbilical hernia. It appeared that she had been subject to the protrusion of the hernia from childhood, but was always able to press it back. For some years past, however, she had suffered from attacks of bronchitis, which, she thought, had caused it to increase in size ; and she also stated that for some time past there had been always a little solid swelling left after the rupture was replaced. About eight days before I saw her the swelling protruded whilst she was coughing more suddenly, and of a larger size than usual, and she could not put it back. She did not, however, apply to any one at the time, but as she felt sick she took some aperient medicine, but without any effect. She repeated the medicine, but this time she vomited it almost immediately, and now feeling the tumour painful and hard, and the vomiting and obstructed state of the bowels continuing, she applied for relief.

Dr M'Cowan tried the taxis, but without success, and on consideration of the length of time which had elapsed, and the urgency of the symptoms which were present when he visited her, he requested me to come and operate.

I found the patient suffering from the symptoms of strangulated hernia, anxious and restless, with quick small pulse, and abdominal tenderness. The hernial tumour was about the size of a large orange, very tense and painful, and generally elastic except at one part where it felt solid, as if the textures were matted together. I made an incision about $2\frac{1}{2}$ inches long over the tumour parallel to the linea alba, and a shorter transverse incision, and carefully exposed the surface of the sac. On opening the sac, I found the contents to consist of a considerable portion of the small intestine much congested, and a portion of omentum. After division of the constricting edge directly upwards, the bowel was readily reduced, but on attempting to return the omentum it was found adherent at one point, and on tracing it towards the surface I discovered that the sac at that part was quite obliterated, and that the omentum was adherent to the superficial fascia and integuments. I, therefore, divided the narrowest part of the protruded omentum, and placed ligatures on some vessels, and then removed the adherent portion. The wound was then closed, and a pad and broad bandage were applied, and an opiate was directed to be given.

Next day the patient was easier, the vomiting had ceased, and there was very little abdominal tenderness. The bowels had not acted; an enema was, therefore, ordered to be administered, and a dose of castor oil to be given in the evening. Her general symptoms improved, but as the bowels continued to resist the action of the milder purgatives, on the third day after the operation two compound Colocynth pills were given, followed by a saline purgative, which acted freely on the bowels, and relieved the tympanitic distension of the belly. Indeed, from this time she improved very rapidly. The ligatures which had been placed on the omentum did not separate for fully six weeks; this rendered the healing of the wound tedious, but did not prevent her wearing a truss, and going about her usual avocations. When the ligatures came away, the wound healed firmly in a few days.

Remarks on Cases III. IV. V. VI. VII. and VIII.—This group has interest as exhibiting the different effects of the constriction in hernia upon the included intestine, and the difficulty as to the prognosis, judging from the apparent amount of alteration in structure. In some cases there is no room for doubt; when a portion of gut is ash-coloured or black, and has a sloughy appearance and gangrenous odour, there can be little hesitation as to the practice to be adopted, viz., to relieve the constriction and lay open the mortified portion of bowel, or at least to forbear returning it into the abdomen, and to apply warm water dressing to the wound, so as to afford a chance of cure by artificial anus. But the cases where the practitioner is sometimes in doubt as to the propriety of returning the bowel are those where the constriction has been very tight or long continued, and where the bowel, though much altered in structure and its vitality impaired, is not positively gangrenous. In such cases where the stricture is divided, with careful after-treatment the gut may recover its vitality and tone. On the other hand, notwithstanding every precaution, the diseased action may go on and lead to fatal results from perforation. The cases of L., and R—b, show this difficulty of judging from the mere appearance of the strangulated bowel; in Mrs L.'s case, the gut, though dark, granular, and tightly constricted, had by no means so bad an appearance as that in Mrs R—b's case, yet perforation took place in the former, whilst the latter recovered without a single

bad symptom. The period after the operation at which the symptoms of perforation supervened in Mrs L.'s case, the portion of the intestine floating loose, instead of adhering to the surrounding structures, together with the examination of the perforated portion, and the character of the minute perforations show that ulceration had proceeded from the mucous surface outwards. Hence the symptoms were at first favourable, and this shows how guarded we should be in our prognosis in cases where the strangulation has existed for so long a period, particularly in old feeble people in whom the reparative powers are small. In such cases we should especially avoid exciting the action of the bowels too soon, and where there is much doubt as to the state of the intestine, I would recommend the plan of dressing the wound which I adopted in Mrs R—b's case, so that if sloughing of the bowel should occur, the feculent matters have the chance of escape by the wound. In the case of Mr — the very dark, congested, and ecchymosed appearance of the intestine, was, I think, principally due to the prolonged attempt at reduction by taxis as well as to the tightness of the constriction, for the serous coat of the bowel had not lost its natural smooth glistening look, nor did it present any appearance of effused lymph on its surface. In H., on the contrary, though, as in Mr — the strangulation had only been of very short duration prior to the operation. The state of the bowel was apparently quite as bad as in Mrs L.'s case. It was "dark purple, granular, covered with flakes of recent lymph, and retained the indented appearance even after the constriction was divided;" and though from the appearance of the intestine and the feeble state of the patient, I formed a very bad opinion of the case, I was agreeably disappointed to find my prognosis was not fulfilled, for he made an excellent recovery. The cases of R—y and Mrs M'G. afford examples of the symptoms existing for some days, gradually increasing in intensity, and inducing great alteration in the constricted portion of the intestine, yet contrary to what might have been expected and what is generally found under such unfavourable circumstances, both recovered without the supervision of peritonitis or other bad symptoms. Mrs M'G.'s case possesses some interest also, as umbilical herniæ are not very frequently the subjects of operation, and the obliteration of the peritoneal sac at one part, and the adhesion of the omentum there to the deep surface of the superficial fascia, is also a point of practical importance worthy of notice.

CASE IX.—*Scrotal Hernia, apparently reduced by Taxis. . Small Hernial Tumour felt at upper part of Canal; Operation; Death.*

Mr C., æt. 85, was seized with the symptoms of strangulated hernia on the 23d December 1853. He was seen by Dr Cruickshank of North Berwick, who found a large scrotal hernia which the patient stated he had had for upwards of twenty years, but that it had never troubled him as he could always reduce it. Dr C. returned the tumour apparently by the taxis, but the symptoms still continuing, Dr C. asked me to meet him to examine the

case. I visited him on the afternoon of the 27th, and found him restless and suffering from hiccough. I was informed that the vomiting had not been so bad for some hours, and that he had had a slight motion of the bowels after an enema. The abdomen was tense and painful to the touch; there was, however, no distinct appearance of hernial swelling but merely a general fulness from great development of fat on both inguinal regions. On examining the left side where the large scrotal hernia had existed, by passing my finger from below upwards along the cord, I thought I could detect a small swelling at the upper part of the canal. After again examining him, and feeling more satisfied as to the existence of the small swelling, I explained to his friends the necessity of an operation to afford him the only chance of relief, although from his exhausted state and advanced age the case was very unfavourable.

He had previously suffered from acute rheumatism and asthma, and on trying to administer chloroform it so depressed his pulse that its use could not be continued.

An incision was made beginning well over the position of the deep ring and carried down for about $2\frac{1}{2}$ or 3 inches. A very great depth of fat required to be divided in order to reach the tendon of the external oblique. I then slit up the tendon from the external ring so as to expose the canal, and when this was done I brought into view a small hernial tumour, about the size of a walnut, constricted at the deep ring. The sac was cautiously opened as there was no fluid in it. The bowel was found very red and granular, but not very dark, and it presented no appearance of gangrene. The constriction was then divided directly upwards. I next gently drew down the gut to examine it immediately above the stricture, when I found a small firm band of lymph constricting it within the sac. This I also divided, then readily returned the bowel, and dressed and bandaged the wound in the usual manner.

I did not see the patient again, but I learned from Dr Cruickshank that after a temporary relief, the symptoms of vomiting and hiccough returned with abdominal tenderness, and the patient gradually sank and died on the 30th of December.

CASE X.—Strangulated Femoral Hernia; Operation; Recovery.

I visited this patient at the request of Dr Maine of Gorebridge, and as I only saw her on the occasion of my performing the operation, I am indebted to Dr Maine for the rest of the report of the case.

Mrs W., æt. 62, previously stout and healthy, was rather suddenly seized with pain in the right hypochondrium, at twelve o'clock noon on the 10th March, followed by sickness and vomiting. Was first seen at twelve p.m. Pain severe but intermittent, and shooting along the course of transverse colon. Pulse, skin, and tongue natural. Bowels have been confined for two days. A calomel and Colocynth pill, with one grain of opium was given, and warm fomentations directed to be applied to abdomen.

11.—No Better. Pain and vomiting at intervals; rather anxious expression of countenance. Bowels not moved. On examining the lower part of left side of abdomen a tumour, the size of a large walnut, was found in the inguino-femoral region rather hard and somewhat nodulated. She complained of no pain in the part, even on pressure, but some uneasiness in the bowels above it. Dr Maine tried to reduce it, but could not; he directed a simple enema to be given, and warm fomentations to the abdomen on the return of pain. Dr Thomson, Dalkeith, saw her with Dr Maine in the evening, when we found her much easier. Had one or two attacks of pain during the day, but not of long continuance. Pulse good; enema had brought away a hard stool. Applied taxis again, but without success. An opiate to be given at bed-time.

12.—Twelve p.m. Has remained nearly free from pain and vomiting all day. Repeat the opiate.

13.—I saw this patient about half-past five p.m. of the 13th March, along with Drs Maine and Thomson. The abdomen was tense and generally tender

to the touch, her features were sharp and anxious, and bedewed with cold perspiration. The pulse was 96 and jerking; tongue rather dry and white in centre. She stated that the vomiting had not been so frequent as yesterday, but that whenever she takes food or liquid the vomiting returns. The bowels were obstinately constipated.

On examination of the left inguino-femoral region I found a hard firm swelling about the size of a walnut, somewhat flattened and movable. It lay outward over the vessels, and no distinct neck could be traced from it towards the direction of the femoral ring, and there was not the slightest impulse on coughing. Still the symptoms and history of the case, combined with the presence of a tumour in this region, seemed to me sufficient warrant to operate. I accordingly, after obtaining the patient's consent, did so. I made a T incision over the swelling, but from the very great amount of fat, I had to convert it into a crucial incision to gain room. After dissecting through a great depth of fat, I exposed a small hernia. The sac was found vascular with masses of fat developed in it, so as very closely to resemble omentum, but by tracing it upwards to the narrow neck of the tumour, its true nature was easily recognised. I next opened the sac, which required great care, as it contained no fluid. The bowel was dark and granular, but not adherent.

On dividing the constriction, which was very tight, a large quantity of reddish serum escaped from the abdomen.

The bowel was then gently reduced, and the wound dressed and bandaged.

14.—Passed a comfortable night, but complains of considerable pain over abdomen generally—has vomited this morning—considerable thirst present. Pulse 110, good. Ordered a grain of opium and two of calomel every three hours. Twelve p.m., greatly relieved. Pulse 100. Pills to be given every four hours.

15.—Much in the same state as last night. 3vj. of castor oil to be given at once, and a grain of opium every six or eight hours.

16.—Feels more comfortable, little or no pain, tenderness slight. Wound looking well—removed some stitches. Opiates to be withdrawn, and the oil repeated. Twelve p.m., bowels moved—feels comfortable. Opiate to be given if necessary to procure sleep.

20.—Has continued to improve till this evening, when, probably owing to having been moved in bed, the pain in abdomen returned with vomiting, and some amount of tympanitis. Ordered the calomel and opium pills again every three or four hours, and a sinapism to the abdomen to be repeated in a few hours.

21.—Pain relieved somewhat, but vomiting continues. Tenderness and tympanitis increased. Face cold, with anxious and sunk expression; pulse small, 120. Covered abdomen with tela vesicatoria, gave a grain and a half of opium to be repeated in three hours. Twelve p.m., greatly better—nearly free from pain—vomiting ceased—skin warm and comfortable. Pulse 112, good. Ordered a poultice to abdomen, and an opiate every six hours.

22.—Passed a good night, and feels better. Opium withdrawn, except at bed-time.

23.—Still improving. Bowels moved by castor oil. Wound sloughing to small extent. Dressed with red lotion. A more generous diet allowed.

From this time she improved rapidly without an unpleasant symptom.

Remarks on Cases IX. and X.—These two cases illustrate two of the difficulties as to the question of operating, arising from the doubtful nature of the case. In both these cases, there was one feature in common, which added to the other difficulties of diagnosis, viz. a very great development of fat in the inguinal region, but, in other respects, the cases were very different. In Mr C.'s case, the doubt as to the existence of any portion of hernia being down,

together with a deceptive remission of the symptoms, rendered it much more difficult to decide as to operating. The hernia, originally a large scrotal one, had been reduced by Dr Cruickshank, with a distinct gurgling sound, of which the patient was sensible. There was also for a day, some remission of the symptoms, and a slight stool. These circumstances taken in conjunction with the absence of any apparent swelling in the inguinal region, when compared with that of the opposite side, or by examining the surface of the abdomen with the fingers, seemed to prove that the hernia had been reduced, and from its large size, it could not have been reduced "en bloc." On the other hand, though there had been some remission of vomiting, still there was not that relief which follows complete reduction by taxis. There were the quick pulse, hiccup, clammy sweats, abdominal tenderness, tympanitis, and occasional vomiting on taking liquids; and the mere remission of the vomiting is by no means uncommon in cases of strangulated hernia, as I have had occasion to remark before in this Journal. The most deceptive symptom to my view was the character of the stool, it was scanty but liquid and bilious looking, as if from the upper part of the intestinal canal; and this, in conjunction with the statement that several distending enemata had been given, which came away without any feculent matter, whilst the thin stool had been passed after the patient had taken some medicine by the mouth, I confess somewhat staggered my decision at first, but after several careful examinations of the dilated inguinal canal by my finger introduced along the chord, I was sure I felt a small swelling very deep, and too tense to be merely a part of the sac, and, therefore, after explaining the necessity of giving the patient a chance of relief, I operated, and as it proved, the state of matters justified my decision. In a younger person, there would have been less room for hesitation, because in such a case the mere incision, even supposing no hernia had been found, would not have been dangerous. In the case of Mrs W., though there was doubt as to the nature of the hard small swelling, still I felt no hesitation as to the practice to be pursued, for the existence of a tumour near the ordinary position of hernia, combined with the symptoms present in her case, is a sufficient warrant in my view, to cut down upon, and expose it. For we not unfrequently find small femoral herniæ lying under glands, adherent to the surface of the sac, and even if no hernia be found, no great harm is done, or rather the real nature of the case is rendered clearer by the process of exclusion; and the practitioner may use more powerful remedies than he could venture to do, so long as there was any probability of strangulated hernia being present. On the contrary, if the surgeon for fear of not finding a hernia, delays too long in such a case, then he most certainly puts his patient's life in great jeopardy.

Besides the difficulties of diagnosis, there is another point of

interest in Mr C——'s case, the peculiar cause of strangulation of the small knuckle of bowel. When the great mass of the hernial contents were reduced by taxis, What prevented the return of this small portion? Before operating, I thought it might depend on a double constriction in the sac, similar to that in the case of P—— B——, recorded in the series of hernia cases, in this Journal for August 1845. But on opening the sac, the true cause was explained by the presence of a band of lymph, constricting the loop of bowel, in fact, an internal strangulation within the hernial sac, keeping the constricted portion distended, leading to its further strangulation by the neck of the sac at the deep ring, and so preventing reduction. The occurrence of such cases has sometimes been argued against the extra-peritoneal operation for hernia, as it has been said that we cannot be certain in any case that such an internal constriction may not be present. But if we pay attention to the rule to make sure of being able to return the contents of the sac without forcible pressure, and take care not to push the sac and its contents back *en masse*, we may feel satisfied there is no such constriction. For although in this case not only all constriction external to the sac was divided, but even the neck of the sac itself, the knuckle of bowel still remained tense, and could only have been reduced by forcible pressure, if at all, and therefore I believe that the objection is groundless, inasmuch as division of the textures external to the sac would not permit of reduction, and thus the surgeon would be necessitated to open the sac, and so recognise the true nature of the case.

Part Second.

REVIEWS.

The Pathology and Treatment of Stricture of the Urethra. By HENRY THOMPSON, F.R.C.S., M.B., London. 8vo. 1854. Pp. 424.

(Continued from page 347.)

IN regard to the diagnosis of stricture, Mr Thompson after pointing out the insufficiency of symptoms, however apparently well marked, to lead us to a correct diagnosis in cases of stricture, and stating the necessity for physical diagnosis by means of instruments, proceeds to point out the form of sounds and catheters which he considers best adapted for passing readily and with least chance of obstruction along the urethra, and gives the figure of one constructed on the natural curve of the canal. The curve and shape of that which he

recommends, is certainly well adapted for passing easily in a great majority of cases, and is almost similar to what is now generally used. We entirely agree with Mr T. in regard to the disadvantage of having the instrument very long in the point beyond the curve, and especially when the point is allowed to droop from the curve; and as to there being a certain determined relation between the axes of the shaft and the point of the catheter, which Mr T. insists upon, we think the value of that suggestion is very obvious. As he remarks, however, it is very certain that many surgeons attain great facility in introducing instruments of very different forms, and some of them not well adapted to the natural curve of the urethra; but this is easily explicable, because in reality it is by no means absolutely necessary that the instrument should be of the exact curve of the canal. A perfectly straight instrument can be passed easily enough if there be no prostatic enlargement; so can also the rectangular staff, though at first sight an awkward looking tool, and few instruments are passed more easily than the short-beaked and suddenly curved lithotomy sound. The great point we think to be attended to in the choice of an instrument, is to have one of a moderate curve and rather short in the point, of a form in fact similar to the catheter represented at p. 163 of Mr T.'s work; but the education of the hand by frequent practice both on the dead and living body is the great essential, and the rules laid down by the author as to the method of introducing the catheter, are in our opinion very judicious.

In describing the plan of proceeding in the physical diagnosis of stricture, Mr T. very clearly points out the benefit of commencing with a moderate sized sound or catheter, so as to avoid sources of error which might arise from the use of small instruments. Even where from the history of the case, the minute stream, etc., we have little doubt, still the introduction of a No. 7 or No. 8 is of use in enabling us to ascertain where the constriction positively begins. In reference to the diagnosis of the exact position and number of the strictures, we observe that Mr Thompson recommends small sounds with "bulbous or olive-shaped extremities about two sizes larger than the stem," and from the figures he gives of them they resemble somewhat, and are on the same principle as the ball-probes recommended by Sir Charles Bell. For the last ten or twelve years we have been in the habit of using and recommending for the same purpose, small-sized sounds from No. 1 and two sizes smaller with probe points, in fact, probes similar in appearance to the pocket-case probe, but of the length of an ordinary sound, and with a flat handle. We have often found them useful in the diagnosis and treatment of very tight strictures, and if used gently and lightly they will often be found to pass along a stricture where the ordinary form of sound of the same sizes would be dangerous; and when passed, the slightly bulbous form of the extremity affords on withdrawal an indication of the position of the stricture. The instruments figured by Mr T.

seem to us objectionable from the sudden swell of the bulbous extremity, which would require an opening of considerable diameter to allow it to pass. The plan of ascertaining the position and form of strictures, by means of soft wax model bougies which take a mould of the constricted part, has been very frequently recommended; and our author, whilst he thinks that it should not be altogether neglected, expresses his opinion that "there is less to be learned in this way than some books appear to teach," an opinion in which we concur, for though we have often made the experiment, we have never been able to succeed in getting such a model of the stricture as books describe. In fact if the bougie be made sufficiently soft to take the mould, it is almost impossible to direct it, and unless it be very soft indeed, it scarcely takes the slightest impression even from a dense firm stricture. As to the advantages to be derived from the "bougies tortillées," recommended by Dr Leroy D'Etoilles, either in the diagnosis or treatment of stricture, we confess we feel at a loss to imagine what these may be. Indeed we are inclined to consider the preliminary bending or twisting of soft gum bougies as rather a work of supererogation, as from what we have seen of the practice of those who use such instruments in stricture, the bougies seem to become bent and twisted enough in attempts at introduction without any such previous preparation.

Mr Thompson, after stating that the two great indications of treatment in cases of permanent strictures, are, 1st, to restore the natural calibre of the canal at least so far as shall be consistent with the safety and comfort of the patient; and, 2dly, to maintain the adequate patency of the canal afterwards, adverts to the endless variety of plans and modifications of treatment proposed from time to time to fulfil these indications, and states that all these plans of treatment may be resolved into three classes, viz. :—Dilatation, the use of Chemical Agents, and Division by cutting instruments. In regard to the treatment by dilatation, our author well observes, that as it is the mildest, so it is the most desirable treatment to employ whenever the case admits of it; that it is the most generally applicable, and the best adapted to cure the great majority of cases of stricture, and that the records of surgery show that it has borne the test of experience longer than any other: opinions in which we think few surgeons will differ from him. He next proceeds to state the method to be pursued in dilatation, by taking two imaginary cases, one mild, the other more difficult, and detailing the treatment local and general, applicable to each, viz., by the introduction of solid metallic bougies or sounds through the stricture, gradually and cautiously increasing the sizes until the canal is fully dilated, rapid or forcible dilatation being avoided, and by carefully attending to the state of the patient's health, and especially to the condition of the urinary secretion. In regard to the general plan of the treatment and most of its details, we cordially agree with Mr Thompson, but we cannot coincide with him as to the advantages of the "con-

cal sound" in narrow strictures, on the grounds that "its wedge-like form enables it to be insinuated with greater ease than the ordinary sound, and permits the dilatation to be effected by the use of a single instrument, instead of two or three." First, we cannot see why a sound of equal size throughout, and of the same diameter as the point of the conical sound, should not be as readily insinuated into the stricture as the conical one. But if it be meant that the advantage is to be gained by the wedge-like action as well as form of the conical bougie, then the practice is most objectionable; it is in fact an insidious form of forcible dilatation, a plan always fraught with danger, and in most cases leading to very distressing results. When a stricture is dilated in this way, or by the almost equally objectionable one of passing over a size, as is sometimes done with the larger sizes, it will generally be found that the patient suffers considerable irritation, there is sanious discharge; and not unfrequently rigors and other disagreeable symptoms present themselves, and many a patient can trace back his sufferings from irritable intractable stricture to some such attempt at rapid cure. Several years ago on proceeding to dissect the perineum in a subject, we had introduced an old-fashioned conical sound, which was lying in the dissecting-rooms, and were somewhat astonished to be requested immediately afterwards to preserve the urethra, as the man had had a very obstinate stricture. True, the instrument had been rather tightly held in passing, but we felt sure that it was in the right passage. On examining the urethra we found that the small point of the instrument had been insinuated through a tight, firm, annular stricture, and that the thicker portion following the point had not dilated, but fairly split it up, and that the mucous membrane and submucous tissue were fissured in a longitudinal direction for some little distance on each side of the contraction. From having our attention thus directed to this point, and from what we have learned of the histories of patients who have since come under our care with irritable stricture, we believe that a fissured condition of the urethral mucous membrane, analogous to what we find to be the case in the similar condition of the rectum, depending upon some form or other of rapid dilatation, is a very probable cause of that intractable form of the disease.

The plan of dilatation by catheters introduced through the stricture, and left in for twenty-four hours, or by larger sizes being introduced as the others become loose, "by the only safe plan of vital dilatation," as Mr Liston used to say—though not objectionable on the same grounds as the plans we have just adverted to, is still attended with many risks, and the contraction is usually found to recur more quickly than after more gradual dilatation. It is a plan, however, which may be adopted with advantage in cases of very tight strictures, or where retention has taken place, so as to gain ground with the smaller sizes; but whenever we reach a No. 3 or 4 it should give place to the ordinary plan

of gradual dilatation. In speaking of the "vital dilatation" as conducted on the plan recommended by Baron Dupuytren, viz., pressure with the point of a sound or catheter against the face of a stricture, Mr T. animadverts on the opinion expressed by Mr Liston against the practice; but whilst we admit the high names of Dupuytren and Guthrie as advocates for this method, we cannot allow that any authority, however high, can suffice to shield it from the objections to which it is, in our opinion, obnoxious. First, what guarantee is there that the point of the instrument is pressing against the obstruction, and not in a false passage? we have known more than one case where false passages have undergone this process of vital dilatation, and the imagined progress of cure been noted from time to time. Again, nothing forms a greater contrast than the irritation which is caused by the unsuccessful attempts to pass a bougie, and the amelioration of symptoms which follows the introduction of even the smallest sized instrument fairly through the constriction into the bladder. This fact has been often noticed by surgeons; and so disproportionate is the relief to the size of the instrument passed, that some have, not without reason, referred the effect to the influence of certain sympathetic actions of different portions of the canal. Thus, the late Mr Vincent, of St Bartholomew's,—

"I find, if I can once get into the bladder the finest instrument of the catgut kind, I have never been baffled in setting the tube to rights; but this confidence rests upon the fact that the bougie should pass absolutely through the whole urethra, and clearly into the bladder. This accomplished, the patient passes a slender stream, with less straining and more comfort than he did before. On the contrary, I have generally found that when I have passed an instrument only just into the stricture, and even forced it through as far as possible without injuring the membrane, but not into the bladder, that so far from the patient passing his urine better after the operation, he has had much more difficulty, and sometimes altogether a stoppage."—*Vincent's Surgical Observations*, p. 179.

As to the laudation by our author of the "Wakley Dilators" as "an ingenious and useful contrivance," we are utterly at a loss to reconcile it with his views of the structure of the urethra, the pathology of permanent stricture, or the principles of treatment he inculcates; and viewing the plan alluded to as simply absurd, we can only attribute the laudatory remarks to his excessive good nature. Finally, as regards the important subject of treatment by dilatation, we believe that if we would only keep constantly in mind that the cure of a stricture is not to be effected by a mere mechanical stretching, but by a vital process, viz., the gradual disintegration and absorption of the organised material which had constricted the canal; and if we proceeded gradually and at moderate intervals from size to size of our dilating instruments, paying attention to the conditions of the urinary organs and other associated functions deranged in consequence of the urethral disease, we would generally be successful in attaining a cure by that method, and but seldom have to resort to other plans of treatment. Under the head of chemical agents in the

treatment of stricture, Mr T. confines himself to the consideration of the action of nitrate of silver and potassa fusa as the two agents most generally used. After reviewing the methods and the results of these plans of treatment, as practised by Hunter, Home, and Whately, and stating some experiments of his own to ascertain the action of each form of caustic when applied to a mucous surface, he comes to the following conclusions :—

“ That these agents are never to be employed for the sake of their escharotic or caustic powers, properly speaking.

“ That the nitrate of silver, lightly applied, exerts a salutary action on the diseased surface of the urethra, relieving inordinate irritability, and checking undue vascularity and disposition to hæmorrhage, as it does in similar conditions of the skin and mucous membrane in other parts of the body, and thus it becomes a useful adjunct to dilatation.

“ That the potassa fusa, as a caustic, is considerably more active than the preceding, and is therefore more dangerous of application. If used at all, it should be only in *very minute quantities*, such as fractional parts of a grain, inasmuch as it is exceedingly difficult to limit the action of so powerful an escharotic. It may, perhaps, aid dilatation in the reduction of some strictures, probably by facilitating the solution of some of their component tissues, when care is taken to employ it in obedience to the condition just named.”—P. 220.

From what we know of the action of nitrate of silver in destroying or allaying the morbid irritability of other mucous surfaces, and as applied to the other chronic affections of the mucous surface of the urethra and neck of the bladder, our author's conclusion seems a fair one ; but practically, when we consider that it is most uncertain in its action, and not unfrequently gives rise to hæmorrhage and abscess *in perineo*, we think that, even used in the limited way recommended by Mr Phillips and our author, as an adjunct to dilatation in irritable stricture, the risks attending its use more than counterbalance the advantages. Further, when we look to the history of cases treated by these chemical agents, we find that they have been very generally used in such a way as to produce an escharotic effect on the stricture, causing ulceration, which, though it may for the time obtain a speedy enlargement of the canal, yet leads ultimately, by its cicatrization, to the most indurated and obstinate form of contraction, and from what we have seen of cases so treated we consider the practice most objectionable. We now come to the third plan of treatment, that of division of the contraction by cutting instruments. Our author treats of this under two great general methods. 1st, Section by incisions from within—internal incision ; and 2dly, Section of the stricture from without—external incision. After fully describing the history and principles of the former plan, and the various modifications of the cutting instruments used to perform it, and the dangers which attend the operation, Mr T. states that he feels bound unhesitatingly to discountenance the use of all curved instruments used for the purpose of perforating or dividing the contraction when situated at the posterior part of the canal, and considers that internal section “ should be limited to that part of the

urethra which is quite movable, and where the direction of the lancetted stillette can be controlled somewhat by the assistance of the hand not employed in directing the instrument." He concludes with the following quotation as to the class of cases to which he considers the intra-urethral section applicable.

"1st.—When dilatation has been sufficiently tried and does not afford relief, or that which is temporary only in its duration (and the stricture is not of an irritable character, in which case, as we have seen, a trial of the caustic may first be made).

"2d.—And the stricture is known to consist of a mere fold of membrane, or, at all events, is a short one, i.e., of small extent from before backwards.

"3d.—When this is situated in the anterior three or four inches of the urethra, the case is one which most probably will be successfully treated by division, according to the modes already recommended, the choice of which should be determined by rules already given."—P. 233.

In regard to these conclusions, we confess, that after a careful consideration of our author's very excellent summary of this plan of operating, we cannot agree with them; or rather we think that most surgeons, on perusing this chapter, would be led to see that internal section, while it possesses no advantage in any case over the external incision, is surrounded by so many dangers, that it should be now entirely dismissed from surgical practice. Mr T. does, indeed, make out a sort of case in favour of its use in preference to external incision when the stricture is anterior to the scrotum, as he states that incisions made in the pendant part of the urethra are apt to be tedious in healing, and frequently become permanently fistulous; but he at the same time admits, that when the patient is kept in the horizontal position, and proper care taken, no greater obstacle will be encountered here than in other parts of the canal. When we consider the risk of hæmorrhage, the almost certain occurrence of more or less infiltration into the textures around the stricture [the urine having no external opening to escape by, especially if such plans as the extensive incisions proposed by M. Keybard be adopted], we again repeat, that in cases requiring division with cutting instruments, the intra-urethral section is not for a moment to be compared in point of safety to division of the stricture from without, when the latter operation is properly performed; and that for such reasons we hold it ought to be abandoned, as at once uncertain, most dangerous, and unnecessary.

The chapter on the treatment of stricture by external incision seems to us the best history and appreciation of the various plans of perineal section and of Mr Syme's operation of urethrotomy which we have yet seen, and we would recommend it to the perusal of all who desire to obtain a clear summary of these plans of treatment. Having described at some length the methods of operating by external incision of the older surgeons, and the modifications of perineal section, as recommended and practised by Messrs Arnot and Guthrie, Mr T. points out that they have hitherto been had recourse to only as operations of the last resort in aggravated cases where no

instrument could be passed along the urethra through the stricture, and hence attended with so much danger and uncertainty that no surgeon has ever adopted them except where all other means had failed; and that these methods are not to be confounded with Mr Syme's operation of external urethrotomy, considered as a method of treating certain kinds of stricture. After drawing a distinction between obliterated urethra in consequence of injury and cases where the urine is passed entirely by fistulous apertures in perineo, and stating that wherever urine can pass from the bladder along the urethra, the canal must be permeable to instruments perseveringly and cautiously used, he proceeds to state the principles of Mr Syme's operation thus:—

“Proceeding then upon the assumption that there are no impermeable strictures, and using the term to embrace an extent of signification which has been just explained, Mr Syme proposes to reverse the maxim which was stated a few pages back to be an axiom accepted by the profession hitherto, viz.:—

“A stricture being permeable to instruments, external division is contra-indicated;”

“And to make permeability an indispensable pre-requisite to the performance of external division.”

“This he does on the ground of the danger, uncertainty, and difficulty, which he asserts must attend incisions made in the perineum in search of the urethra without a guide.”—P. 252.

He next describes the operation and the class of cases to which he considers it applicable, viz., cases in which dilatation fully and carefully tried has failed to afford permanent relief, or where it cannot be carried out owing chiefly to one of the two following causes,—1st. “When the contracted canal is so extremely irritable that the introduction of instruments instead of alleviating the symptoms exposes the patient to various dangers from the local and general disturbance thus excited.” 2d. “In the other the peculiarity consists in a contractile tendency so strong as quickly to counteract the effect of dilatation, and thus render it useless.” In forming his appreciation of this method, Mr T. gives a list of the results of 113 cases in which the operation had been performed at the time he wrote, and taking them as data he proceeds to inquire, first, What are the dangers of the operation? secondly, How far is it entitled to be considered as a cure? As regards the former of these questions, he shows that the number of fatal cases in the 113 above mentioned was only four, in all of which the symptoms were those of pyemia—a condition to which every operation, however simple, is occasionally obnoxious, and which sometimes follows even the use of the catheter in cases of stricture. As to hemorrhage, there has been no fatal case of its occurrence, and primary hemorrhage has never been so great as to cause uneasiness to the surgeon in any case. In one of the cases secondary hemorrhage occurred to some extent on the 14th day, but was easily arrested. As to the probabilities of hemorrhage which have been argued from the anatomy of the parts, we refer our readers to

the former part of this article. From the part of the urethra requiring division being situated in front of the deep fascia, and the presence of the external incision, the risk of urinary infiltration is shown to be very slight. There remains a train of symptoms which occasionally follows the operation, and which Mr Syme recognises and designates as "nervous," which "consists of rigours, bilious vomiting, and delirium." Mr Syme's experience, Mr T. says, leads him unhesitatingly to state, that these symptoms are not dangerous in themselves, and do not produce dangerous results; and, to use Mr Syme's words, "they require no cordial or other treatment, except a strong assurance on the part of the surgeon that all is right." We freely confess, that on this point we could desiderate some more satisfactory explanation, both as to the causes of these symptoms, and also as to the diagnosis between them and similar symptoms which are usually premonitory of dangerous constitutional disturbance. Otherwise, we think, the surgeon himself would require no small amount of "assurance" to look upon such ugly "nervous" symptoms as "all right." As to the second question, viz., the curative value of the method, Mr Thompson, after examining the results of the cases operated on, as classified by him under three heads, 1st. Where it has failed to afford relief; 2dly. Where it has been followed by relapse; and 3dly. Where it has effected a permanent cure; says,

"There is therefore no alternative remaining than to admit the existence of a strong probability, that a large proportion of cases must be assigned to the third category, viz., those for whom the proceeding has effected a permanent cure."—P. 267.

We have said enough, we think, to show how fully and dispassionately our author has discussed this controverted plan of treatment, and we most heartily agree with him in his remarks on, and appreciation of, it. We believe with him, that much of the opposition it has encountered has arisen from its being confounded with old, difficult, and dangerous plans of perineal section in cases of obliterated urethra, described by Mr T. at the beginning of this chapter, and we think he has done good service in so fully drawing the distinction between them. Possibly, however, some of the opposition to the operation may have also been evoked by a cause which leads to opposition in the case of most discoveries, viz., a very natural tendency to urge its claims too strongly and indiscriminately, so as to lead to the idea that it was proposed as a general method in lieu of dilatation, and not, as Mr Syme really meant it to be, an exceptional method, adapted for a particular class of cases. Restricted to the cases already indicated, we hold Mr Syme's method of urethrotomy to be established, as a simple and usually efficacious remedy in a hitherto very troublesome class of strictures, and as free from danger as any operation on the urinary organs can well be; and we doubt not, that when time shall have softened down the asperities

of controversy, and experience shall have dispelled prejudice, it will be acknowledged by all as a most valuable addition to surgical science.

The work closes with remarks on some of the consequences of stricture, such as abscess and fistula in *perineo*, retention of urine and stricture of the female urethra, but our already exhausted limits warn us to conclude. There is, however, one subject of some importance to which we would briefly advert in reference to fistula in *perineo*. After showing that in cases of urinary fistulæ, our first treatment must be directed to restore the calibre and healthy state of the urethra, and stating that generally, when this is done, but little treatment is required to heal the fistulæ, Mr T. alludes to cases which do not yield to this treatment, and directs the use of the heated wire or nitrate of silver to destroy the pseudo-mucous membrane, and excite absorption of the consolidated lymph, which he seems to regard as the chief causes of the patency of the fistulæ. Doubtless, the treatment recommended is the proper one in the cases referred to by him, but we are surprised that Mr T. does not allude to those urinary fistulæ, where the difficulty or rather the impossibility of cure depends upon great organic changes of structure, such as the fistula communicating with a chronic abscess of the prostate, the gland in some cases being completely atrophied, and merely represented by its capsule, filled with foetid pus and urine, and communicating with the urethra and the perineal fistula. Again, where there has been extensive loss of substance both of the urethra and the soft parts of the perineum from sloughing, as after infiltration of urine, the condition of the parts often preclude us having hopes of curing the fistula permanently by any measures, whether by the heated wire, potential cauteries, or plastic operations; for though in such cases we may cause closure of the fistula temporarily, abscesses speedily form, and other fistulæ are established.

In commencing this review, we stated that we considered that a thorough and dispassionate investigation of the whole subject of the pathology and treatment of stricture of the urethra, was very much wanted at the present time; and in concluding our notice of Mr Thompson's work, after a very careful perusal and study of it, we think we may safely say that he has fulfilled that requirement most ably. In reviewing the different portions of the work, while agreeing with our author as to the great general principles, we have occasionally differed from him as to details, or in regard to his appreciation of certain plans of treatment, and have not in such instances hesitated to state our own opinions; yet taking the work as a whole, we have little hesitation in saying that, as regards sound views of the pathology of stricture, clearness in enunciating general principles of treatment, or in describing operative procedures, and as, in fact, exhausting all that could be said of our present knowledge of the subject, we know of no modern monograph on stricture which equals it. A perusal of the valuable matter contained in the appendix

alone, will serve to show somewhat of the amount of research, labour, and expense, Mr Thompson must have incurred in gaining information from every available source to render the work complete, and in obtaining correct data as to disputed points by his own personal observation. And when, moreover, we reflect on the war of conflicting opinions and statements, and the acrimony of the controversy in reference to certain plans of treatment which existed at the time this book was written, we think Mr Thompson deserves no small credit for the dispassionate manner in which he has executed his task, and for the calm discriminating judgment displayed in appreciating the various methods of treatment. Whilst, therefore, we congratulate him on his hard-earned and well-deserved success in having obtained the Jacksonian prize, we would congratulate him still more on the execution of the book itself, which is calculated to be most useful to the profession, and to reflect great credit on Mr Thompson both as an author and as a practical surgeon.

Class-Book of Botany; being an Introduction to the Study of the Vegetable Kingdom. By J. H. BALFOUR, M.D., F.R.S.E., Regius Keeper of the Royal Botanic Garden, Professor of Medicine and Botany in the University of Edinburgh, etc. Edinburgh. 8vo. 1854. Pp. 1114.

THE want of a full and complete text-book for students of botany has long been felt by teachers and learners of this delightful science in Britain. There are not a few good treatises and manuals devoted to separate sections of the science, but none of sufficient amplitude, embracing all its divisions in one comprehensive view. On the Continent there are many works of this kind, and twenty years ago, translations of continental manuals, that are now far behind the state of research, served, though imperfectly, to fulfil the office of student's guide. By their use, and by occasional study of the then excellent, and still valuable, introductions of Professor Lindley, the diligent young botanist attempted to master his subject. But, until comparatively lately, there was a great difficulty felt by the teacher when called upon to recommend a *single* work for the guidance of his pupils. This desideratum has been amply and ably provided by Dr Balfour, who is assuredly at this moment the most successful teacher of botany in Europe.

Dr Balfour's Class-book contains an epitome of the leading facts and principles of botanical science, in all its ramifications, as recognised at the present day. Commencing with vegetable organography, under which head the author includes the consideration of the tissues and their combinations, he then takes up the physiology of plants. His third part contains a condensed analysis of systematic

botany; and in the fourth and fifth sections of his book, the distribution of plants in space and time is summarily discussed. An appendix is appropriately devoted to a glossary, and to directions for the practical pursuit of botanical researches. Throughout, the work is richly and profusely illustrated with well selected wood-cuts, serving at once to adorn the text, and to render it clearer to the reader, without distracting his attention by obliging him to turn to separate plates.

It is difficult to find fault with so good a book as this, nor are we inclined to do so, believing it to be the most complete work of its kind. Were we to make an objection, or to offer a suggestion for future consideration, it would be to urge upon the author the propriety of getting rid of the superfluous terminology that he has adopted from ultra-analytic foreign authors, in his first or organographical part. Too many distinct names for things that are very slight modifications of each other, become burthens on the student's memory, and serve rather to perplex than to help him. The remaining parts are excellent throughout, and are brought up to the latest published original researches. Occasionally, when discussing opposing theories or statements, Professor Balfour leaves us in some doubt respecting the conclusion at which he has himself arrived. This modesty can, however, be easily rectified in the class-room, though, for readers at a distance, it will be felt as a defect, since they will naturally be anxious to know the decision of so eminent and experienced a botanist.

There is one feature in the book, almost a peculiar one, so far as similar works are concerned, that deserves especial commendation, viz., the brief and excellent analytic summary introduced at the close of discussion of each distinct subject. To both learner and master these summaries or recapitulations will prove of the greatest service. They contain the pith and substance of the text, and are drawn up with precision and skill. At the end of the account of each natural order also, there is appended a series of notes on the products and properties of those of its genera and species as have been applied to economical, medical, or ornamental purposes. Occasional memoranda direct attention to peculiar botanical features presented by some particular species. The number of useful facts thus accumulated in few words is prodigious, and bears sure testimony to the learning and extensive researches of the recorder of them. The work thus acquires a value far beyond that of its ability as a class-book, and becomes adapted for a portable cyclopædia of reference in botanical matters, most convenient for the restricted library of the traveller as well as for that of the resident in distant regions.

Part Third.

PERISCOPE.

PRACTICE OF PHYSIC.

REPORT REGARDING THE CASES OF HYDROPHOBIA WHICH OCCURRED IN FRANCE DURING THE YEAR 1852. BY M. AMBROISE TARDIEU.

(In the year 1850, the minister of agriculture and commerce, on the recommendation of the committee of public health, sent a circular to every prefect in France, requesting him to give information regarding any cases of hydrophobia which might occur in his department. A number of reports were in consequence sent in, but as these were in some respects incomplete, a fresh circular was issued detailing more particularly the manner in which the cases should be recorded. From the information so obtained, M. Tardieu drew up a report regarding the cases which occurred in the years 1850-51, as well as in 1852. As the report for the year 1852 is much more complete than the others, we subjoin an abstract of it).

1. The number of cases of hydrophobia which occurred in France during the year 1852 was 48. These were observed in 14 departments: the department in which the greatest number occurred was that of the Hautes Alpes (in the south-east of France, latitude between 44° and 45°); while the department of Lozère (also in the south, and having the same latitude as the other) came next.

2. With regard to the sex; 36 of the 48 cases were males, 12 females; the proportion in the two preceding years was almost the same.

3. The following table exhibits the ages of the subjects affected with hydrophobia:—

Below 5 years, in 1852,	3	in two former years,	4 = 7
From 5 to 15	16	...	14 = 30
... 15 to 20	4	...	11 = 15
... 20 to 30	3	...	9 = 12
... 30 to 60	17	...	37 = 54
... 60 to 70	1	...	7 = 8
Above 70	0	...	6 = 6
Not mentioned	4	...	0 = 4
	48		88 = 136

This table shows the incorrectness of the opinion which ascribes the disease to the effects of terror, for it shows that 7 children under five years of age have been attacked.

4. All the cases which occurred in 1852 originated in the bites of dogs, except one, where the bite of a cat was the cause of the disease.

5. The situation of the wounds inflicted by the rabid animals was as follows in 48 cases:—On the face 13 times; on the upper extremities 15; on the lower extremities 12; not mentioned 8. In two of the cases the disease was communicated by pet dogs which were accustomed to lick their master's faces, and where excoriated lips were the seat of the inoculation.

6. In 40 out of the 48 cases the date of the inoculation has been observed. It occurred in March, April, and May in 10 cases; in June, July, and August 16; in September, October, and November 4; in December, January, and February 10.

7. It seems a considerable number of individuals who are bitten by rabid

animals escape the disease. During 1852 some observations were made on this point, and it appeared that out of 44 persons bitten about the same time 23 only were attacked.

8. The period of incubation of the disease was exactly noted in 20 cases. It was as follows :—Less than a month in 8 cases ; from 1 to 3 months 10 ; from 3 to 6 months 1 ; 11 months 1.

9. The duration of the disease in 20 cases was 2 days in 6 cases ; 3 days in 8 ; 4 days in 5 ; 6 days in 1.

10. The termination of *confirmed* cases of hydrophobia was constantly fatal. Of the 48 cases, it appears that only 27 came under this category, in the others the effect was merely local. In 12 of these 27 cases no precaution was taken, in 4 no mention is made of this circumstance. In 8 of the remaining 11 cases cauterization was resorted to immediately, in 3 at a late period. Of the 21 individuals who escaped (see ¶ 7) cauterization was energetically performed in 12 cases ; the details of the other 9 have been omitted.

11. As to the mode of cauterization employed, the actual cautery was used in all the cases but 5, and these were treated by protonitrate of mercury, nitric acid, ammonia or butter of antimony. In Germany it has been proposed to excise the bitten parts and then to wash the wounds with a solution of caustic potash.—*Annales d'Hygiène, etc.*, January 1854.

BAILLAYER, SPECIAL VARIETY OF INSANITY, PRESENTING REGULAR ALTERNATIONS OF EXCITEMENT AND DEPRESSION.

This species of insanity, to which the author gives the name of “*folie à double forme*,” is characterised by two regular periods, the one of excitement the other of depression, occurring alternately. The disease presents generally an intermittent form, and the duration of the attacks is exceedingly variable. Some cases pass through both periods in eight, six, or even two days ; sometimes the paroxysm lasts two, three, or four months. The most numerous cases are those which last about a year, the patient spending six months in a state of melancholic depression and six months in maniacal excitement. In the short attacks, the transition from one period to another is abrupt and sudden, and generally takes place during sleep. But in the attacks which last a year, the change takes place very slowly, and by insensible degrees ; there occurs even a momentary equilibrium in the intellectual functions, and the patient appears to be recovering ; but a fortnight or a month afterwards the second period breaks forth. In a first attack, this state of transition has been more than once mistaken for real convalescence ; and patients dismissed from asylums in this condition have been brought back almost immediately in a state of violent maniacal agitation. The duration of the intermittance, also, is very variable—from several days to several months ; some patients have had only seven or eight attacks in ten or twelve years. Occasionally there is no intermittance, the attacks succeeding without interruption ; the patients present regular alternations of excitement and depression, of sadness and gaiety, without ever stopping at the condition of equilibrium which constitutes health. In the attacks, there is a distinct relation in the intensity and duration of the two periods, which at once distinguishes this variety of insanity from the irregular agitations which are observed occasionally in melancholic at its different stages. Thus the maniacal excitement is more violent in proportion as the previous melancholic depression was more profound. In very short attacks, the duration of each period is equal, and the same relation exists, with a slight difference, in attacks prolonged for several months or even a year. The symptoms during the maniacal period consist more frequently in instinctive impulses than in raving delusions ; we observe nymphomania, satyriasis, dypsomania, and continual tendency to do mischief. With regard to treatment, the author quotes a case in which bloodletting, practised for several months in succession at the middle of the menstrual interval, caused the cessation of the maniacal

period in a young girl, but the period of melancholy still persisted, and returns in paroxysms.—*Gaz. Med.* Feb. 4, 1854.

M. Falret has also described, under the name of "folie circulaire," a form of insanity similar to that just described, and characterised by the regular and successive reproduction of the maniacal state, the melancholic state, and a lucid interval of long or short duration.—*Gaz. Med.* Feb. 18, 1854.

BARTHEZ ON THE INFLUENCE EXERTED ON VARIOLA BY VACCINATION PRACTISED DURING THE INCUBATION OF THE ERUPTIVE FEVER.

In this inquiry the important practical question is raised, if vaccination ought to be performed when an attack of small-pox is imminent? The general opinion prevails that variola coming on during the course of the cow-pox eruption is modified and mitigated by it; but M.M. Barthez and Rilliet have shown that, under certain circumstances, the variola is aggravated by the vaccination; while M. Bousquet has maintained that vaccination has no influence whatever, and that the modifications of the variola which follows it are mere coincidences. From a general review of the facts known, however, it cannot be doubted that the variola is modified favourably by vaccination in the great majority of cases, so that all its stages are shortened and lessened in severity; in fact, the variola is transformed into a varioloid. And this modification will take place even when the vaccination is done during the incubation of the eruptive fever, provided the vaccinal eruption precedes the eruption of small-pox. But in some exceptional instances, which are far from rare, variola supervening on vaccination has been found unusually dangerous and fatal. In these cases, according to M. Barthez, the small-pox is not fatal from its own intensity or from its usual complications, but it is modified unfavourably by the vaccination, and presents various anomalies. The eruption is generally small, unequal, irregularly scattered, pale, without areola, without local or general reaction; sometimes there is more activity in the beginning of the eruption, which is accompanied by fever, but the disease soon puts on the characters of the hemorrhagic small-pox. These anomalies have been observed only in very young children, of weak constitution or reduced by previous illness; and in them the concurrence of vaccination and variola has invariably proved fatal. On the other hand, very young, weak, and sickly children have sometimes recovered from the variola alone; so that it appears that the vaccination aggravates instead of lessening the danger under these circumstances, and induces an anomalous and fatal form of small-pox. Accordingly, the author lays down the following rules:—

1. If we vaccinate children during the incubation of small-pox, so that the vaccinal precedes the variolous eruption, the latter will be almost always modified.

2. That in the great majority of cases the modification will be favourable, and the variola will be changed to a varioloid.

3. That if the eruptive fever, in being modified, assumes a grave character, this must be attributed especially to the previous state of health along with the very young age of the child.

4. That in the latter case, the vaccination being a third disease superadded to the two already existing, has no favourable influence on the small-pox, but only forms a complication, which is of the greatest danger from the early age and feebleness of the child.—*Revue Med. Chir. de Paris*, January 1854.

DR GUNSBURG ON GANGRENE OF THE BRAIN AFTER DECUBITAL MORTIFICATION.

Henlé is of opinion that pus, cancer-juice, etc., are not propagated by the lymphatic vessels. Dr Günsburg doubts the truth of this opinion, and relates the following case to prove its fallacy:—

R—, a criminal lunatic, who had been for years in a state of general paralysis, became affected by decubital mortification, which caused external necrosis of a large portion of the sacrum, and died, after some weeks, labouring

under all the symptoms of chronic hydrocephalus. The body was much emaciated, the cranial bones were thickened, their diploe was obliterated, the dura mater was anæmic, the longitudinal sinus was enlarged, the pia mater was thickened and partially disintegrated. The whole base of the brain was in a state of complete ramolissement, and the crura cerebri and optic thalami were of a greenish-grey or blackish colour. Granular masses were found here, in the neighbourhood of the destroyed vessels, formed from elementary corpuscles. The optic thalami were atrophied, and the lateral ventricles contained about ʒij. of clear serum. Both lungs were loosely adherent, and here and there were slightly altered granulations. The bronchial tubes contained some reddish serum; the pulmonary texture was pale coloured; and the lower lobe of the right lung was swollen, dark-brown coloured, and contained neither blood nor air. The pericardium contained some serum. The spleen was somewhat enlarged, and the liver pale brown and fatty. The mucous membrane of the alimentary canal was thickened and yellowish, in the cæcum. The omentum was adherent in the right iliac region. (Partial omentitis is common among the insane.) The splenic, left hypogastric, left external iliac, and both vertebral arteries contained masses of elementary granules, resembling pigment cells, and also spindle-shaped corpuscles, which formed little grumous masses, analagous to those found in the softening at the base of the cerebrum.

The author considers that the presence of these masses in the skin, spleen, and brain on the one side, and in the arteries on the other side, almost indubitably shows them to have been conveyed by the arteries, from the diseased external parts to the various organs affected. The author adds that cerebral gangrene only occurs in the decubital mortification following long-continued illness, and never in the cases of that affection so common in typhus.

NIEPCE. HYDROPHOBIA OCCURRING IN A CRETIN FROM THE BITE OF A MAD DOG—REMARKABLE DEVELOPMENT OF THE INTELLIGENCE DURING THE PAROXYSMS.

Antoine Chauvet had been a cretin from birth, and at the age of 17½ years presented, in a marked degree, all the physical and mental characters of cretinism. He could only articulate a few words imperfectly. He had not sufficient intelligence to learn reading or writing, nor to understand the catechism. His affections were little developed: he had some liking for his mother, but showed none for his brother. On the 10th May last he was bitten by a mad dog; the wound was slightly cauterised with some drops of ammonia by a druggist, about an hour after the accident. Nothing was observed till the 27th July following, about eleven o'clock, when Chauvet refused to eat or drink; and two hours afterwards all the symptoms of hydrophobia made their appearance. From the commencement of this disease, to the great astonishment of every one, Chauvet spoke with much greater facility than he had ever done before, addressing those around him, and relating the sufferings which he felt. In the intervals of the paroxysms, he called his mother and brother, showing his affection for them by the most tender caresses, and entreating them not to leave him alone. He caused the priest to be sent for, and on his arrival expressed with tears his bitter regret that he had never been able to learn the catechism. During the remainder of his illness, his intelligence became always lucid during the paroxysms of suffering, when he would put questions to those around him and give directions to them; but as soon as calm or depression ensued, the natural state of his intellect returned. On the 1st August, acute delirium came on, during which he spoke frequently and with volubility, citing facts which had happened several years before, and to which he had never seemed to pay attention. The delirium lasted till night, when it was succeeded by deep coma. He died at five o'clock on the following morning.—*Gaz. des Hôp.*, Aug. 27.

ON THE CURE OF GALL STONES. BY M. BARTH.

M. Barth read a paper entitled "Anatomico-pathological researches on the
NEW SERIES.—NO. LIH. MAY 1854.

mechanism of the spontaneous cure of gall-stones, and on the practical consequences which may be deduced."

The nucleus of gall stones consists in general of a mass of inspissated bile, and many calculi are composed entirely of this substance, hence the utility of strict attention to diet, both as a prophylactic and curative agent, of the abundant use of diluent drinks, and the frequent employment of laxatives to prevent the unnatural thickening of the bile.

The predominance of cholesterine, or fatty matter, in the composition of a great many calculi, indicates the advantage of a vegetable diet and of the use of alkalies and alkaline baths, care being taken to refrain as much as possible from all fatty matters. The solubility of the greater number of calculi in ether and in oil of turpentine, encourages us to make use either of ether alone or of a mixture of ether and oil of turpentine, both internally and externally. There is no doubt an immense difference between the effect of ether on a calculus plunged into it, and its action when introduced by the stomach; still, however minute this action, it may be sufficient to diminish the size of the gall stone a little, and so to allow it to pass through the biliary canals.

In addition to these means, it will be well to employ frictions of belladonna, with a view to facilitate the dilatation of the duodenal extremity of the common duct, which is the point which offers most resistance.

It appears that some calculi may be broken up within the gall bladder, and hence the utility of applying firm pressure and percussion to the right hypochondrium.

M. Barth adds, that whatever may be thought of the reasoning on which these practical deductions are founded, frequent success, both in hospital and private practice, has proved to him the utility of the combination of these modes of treatment.—*Bulletin l'Academie Imperiale de Médecine*, March 7, 1854.

[In a paper on the same subject, published in the "*Journal de Médecine et de Chirurgie Pratiques*," for February last, Professor Trousseau recommends a mode of treatment almost identical with the above. He does not, however, believe that ether can have any effect in dissolving calculi already formed, but he considers that it acts as an anesthetic on the nervous system of the stomach and duodenum, and is hence very useful in cases of hepatic colic. With this view, he recommends it to be prescribed either in capsules, or in a mixture of syrup and orange-flower water, of which a table-spoonful (containing about ten drops of ether) is to be taken every half-hour; attacks of hepatic colic rarely fail to be relieved by this means. To cure the tendency to the formation of calculi, *M. Trousseau* recommends the use of alkalies and alkaline baths, a diet consisting of lean meat, fish, and vegetables, and this combined with plenty of exercise.]

SURGERY.

VIRCHOW ON THE GROWTH OF HEALTHY AND RACHITIC BONES.

(Continued from page 369.)

The next process which occurs is *the formation of the medullary substance from cartilage cells*. Virchow observed that in some places, on the same level with the ossification, the double-contoured capsules of the cartilage cells became thickened, granular, and not easily collapsed by the action of water. Their nuclei were multiplied, (so that two, three, four, or more lay within one cell), and they became rounded and slightly granular. Their capsules, and also the intercellular matrix, lost by degrees their firm, clear, homogeneous appearance, and became of a dim and faint yellow colour, indistinctly striated, and finally they merged into the true medullary substance. This substance consists chiefly of dim non-granular cells of various sizes, containing one or more nuclei; and, here and there, specks of cartilage are found enclosed in it. The medullary spaces here are as large as those found in the cancellated texture of the bone. In some places, Virchow observed at their circumferences, cartilage cells which had again become partially enveloped by thick capsules; but one side of the capsule was generally found amalgamated with the medullary substance lying next it. He could not discover the origin of the spindle-shaped bodies found in perfect medulla;

but he saw bodies resembling them in the stripes of matrix lying between the groups of the cartilage cells, which seemed to him to extend into the cell-cavities. After much careful examination and comparison, Virchow has come to the conclusion that the medulla of the bone and that of the cartilage are exactly identical, with this single exception, that the large cells, containing many nuclei (the "*plaques à plusieurs noyaux*" of Robin), which are occasionally found at this age, in the marrow of the bone, are wanting in that of the cartilage.

The characteristics of this stage of rachitis, according to Virchow, are *tardiness of ossification*; *unusually great breadth of cartilage* in consequence of this; and *abnormality in the advance of medullary-space formation*, which often extends beyond the line of ossification. In fact, *the rachitic process principally consists in ossification being nearly arrested, while the preparatory growth of cartilage, and the consecutive formation in it of cancellated medullary texture, continue to proceed uninterruptedly.*

These peculiarities of rickets are seen most distinctly in its advanced stages. The costal cartilages then become very much swollen, especially on their internal surfaces, where the enlargement is often three-fourths of an inch broad, and from two to three lines raised above the level of the cartilage. This enlarged portion presents two distinct structures, viz., one which is white and regularly formed, lying at the sternal extremity of the cartilage, and constituting one-fourth of its whole extent; and another, characterising the remaining portion, which has a reddish, speckled, and often ecchymosed appearance. On making a longitudinal section of the part we find that the lesser segment corresponds to the bluish transparent growing cartilage, and the reddish portion to the vascular layer in which medullary formation occurs. Ossification generally commences at the circumference, especially at the inner side, where it is farthest advanced; and large anastomosing blood-vessels—extending in the long axis of the bone—are found traversing the interior of the solid cartilage. The bulb-like extremity of the newly formed bone, at this part, lies in the cup-shaped hollow of the swollen cartilage, like an acorn in its cup. With the naked eye we can perceive, scattered the vascular medullary layer, bluish cancelli (*balken*) and portions, which are immediately connected with the layer of external growth, and are analogous to it in structure. These are the remains of earlier formed texture, arrested in their development by the extension of the medullary excavations, which have become pliable unresisting structures. Often, especially in the femoral and tibial cartilages of the knee joint, the blue cancelli of the endogenous layer, and the red or yellow ones of the medullary spaces, are arranged in alternate rows, their projections interlacing, like the fingers of one hand thrust within those of the other.

A microscopic examination of the part shews the transformation of the cartilage cells into bodies resembling the lacunæ of bone. Sometimes this occurs only in isolated patches, but at other times the process advances uniformly and regularly. Virchow observed the cartilaginous elements at the circumference, ranging themselves round large ramifying medullary spaces, in regular radial lines, among which were interspersed fine striae and sometimes delicate canaliculation, with fine fibre-cells. In some parts of the cartilage the transformation process begins by thickening of the cell wall, which renders the intercellular space narrow and serrated at its margins. Projections extend from the capsules, with fine and narrow pores lying between them. The cavity of the cartilage cells at this stage presents a granular appearance; and muriatic acid displays in it a nucleus, surrounded by some fat cells, and by granular matter which Virchow distinctly traced into the porous canals. He also observed, at the circumference of the large medullary spaces, fine but distinct prolongations, like oblong hollows, extending into the intercellular matrix.

The metamorphosis sometimes occurs simultaneously in *all* the cell-groups, giving to the cartilage a true osteoid character. The thickened capsules become coarsely fibrous, while the surrounding intercellular substance remains wholly unchanged. The intercapsular cavities become narrower and notched. In the thickened capsule there are formed coarse and firm yellowish fissures, which extend all in one direction; and, finally, there is only a spot (*insel*) of striped fibrous

substance having numerous stellate cavities. This appearance is rendered more distinct by the action of muriatic acid.

It happens less frequently that this transformation occurs while the substance remains clear, shining, and fibrous. The bony character is then distinctly seen, and we can trace, in the homogeneous matrix, the ramifications of every fine canaliculi, which anastomose with one another, and exactly resemble lacunae and canaliculi. In such cases it would seem that the homogeneous intercellular substance is formed by the amalgamation, (*verschmelzung*) of the capsular envelopes.

In more advanced stages of rachitis, medullary spaces are formed in these places, which are of a fine fibrous character, containing few cells, and but scantily supplied with blood-vessels. When viewed with the naked eye, these appear dim and whitish-yellow, like those found in young growing periosteum, instead of red and soft like those of the true cancellated tissue.

Ossification occurs in the usual manner in the osteoid cartilage; in the simple capsules, as well as in the homogeneous masses with serrated cavities, but especially in the striped fibrous texture. It generally advances in the hard osteoid bars which intersect the medullary spaces; but in exceptional cases it occurs in the thickened capsules, forming rounded, elongated, or narrow calcareous bodies, having central serrated hollows. All these changes are visible to the naked eye, in the articular extremities of the long bones.

Rachitis, according to the phenomena we have described, may be divided into the following three stages:—

“1st, *Arrested advance of the line of ossification, and proportionately great development of the preparatory cartilage.*

“2d, *Advance of the medullary space formation into or beyond the line of ossification; without interruption of the growth of the cartilage.*

“3d, *Formation of fibrous medullary spaces; and osteoid transformation of the parts surrounding them, as well as of those more distant, without the occurrence of any calcareous deposition.*”

(To be continued.)

LOCAL ANESTHESIA.

The production of local anesthesia in disease by the direct application of the vapour of chloroform, was first brought into notice by Dr Hardy of Dublin, and has lately excited much attention.—(*Dublin Quart. Jour.*, Nov. 1853). The instrument, which he invented for the purpose, consists of a small metallic cylinder holding a sponge: a pipe is fixed at one end, and to the other end a gum elastic bottle is attached, provided with a valve for the admission of air. When the sponge is moistened with chloroform, pressure on the elastic bottle forces the vapour through the pipe, and the jet of vapour can be applied directly to the affected part. The first effect produced by the application of the vapour is a sensation of heat, which may even cause uneasiness; but this is quickly succeeded by the subsidence and complete removal of pain. The relief is said to be as speedy and perfect as it could be by inhalation, and is so permanent that the pain does not return again for several hours, and then only in a mitigated degree; and there are no unpleasant subsequent effects, such as occur after inhalation, or after the use of narcotics. Dr Hardy relates several cases, mostly of uterine disease, and generally of a very painful and distressing kind, in all of which immediate and complete relief was afforded by the anesthetic douche. His first case was one of cancer of the uterus, where the excessive suffering was completely removed in a few minutes by the chloroform vapour; and, when the pain afterwards returned, the same means were always resorted to, with a like effect. In his second, third, fourth, and fifth cases, which were examples of uterine disorders, attended by great pain of the back and womb, the douche applied to the uterus caused the cessation of the pain, first in the back, and afterwards in the womb. In a case of great irritability of the nipples, in various other forms of local irritation, and particularly in a case of pruritus pudendi, the anesthetic application was equally successful.

From this experience of its action, Dr Hardy considered the local application of chloroform to be preferable to its inhalation.

Thus strongly recommended, the practice was repeated lately in Paris pretty extensively, but, unfortunately, the same success has not attended it. At first, indeed, M. Nélaton, after applying the douche, incised an abscess on the foot without the patient showing any symptoms of pain; and M. Dubois produced insensibility in a painful abscess of the axilla and wound of the back of the hand. But, with these and a few other exceptions, the douche has generally failed to produce anesthesia in surgical operations. Velpian used it, without success, in a case of abscess and of cancer; Giraldes and Nélaton in subsequent trials; Gosselin and Ricord found no effect produced by it; and Roger found it completely ineffectual in producing anesthesia on the sound skin. Langier, however, states that he has found this method of service; and, in a case of painful stump, after amputation of the thigh under M. Larrey, the patient felt great relief from his sufferings after the chloroform application. With the view of increasing the effect, several modifications of the original apparatus have been devised. M. Rickard used a much larger reservoir of chloroform, and expelled the vapour by means of bellows. In a case of fistula in ano, in which he employed this apparatus, however, no other effect was produced except a sensation of cold; the incisions were as painful as they generally are. M. Maisonnenué having to perform amputation of the toe, enveloped the foot in an india-rubber bag, in which chloroform was poured, so as to keep the part in an anesthetic vapour bath; but, instead of insensibility, a painful feeling of burning, and so much local hyperesthesia were produced, that the operation had to be put off till next day. In short, the method has so completely disappointed expectation, that the surgeons have latterly entirely abandoned it. M. Velpian, indeed, thinks that ether and chloroform cannot produce anesthesia unless they are absorbed by the lungs; and he looks upon Dr Hardy's cases as not at all conclusive, in consequence of the irregular duration of uterine pains, and their frequent spontaneous disappearance. Yet, even in uterine cases, M. Dubois has renounced its use as quite ineffectual. In consequence of the failure of Dr Hardy's plan, attention is now being directed to other local anesthetics. Velpian still uses in many cases the production of cold by a freezing mixture of ice and common salt; and the cold produced by the volatilization of ether and chloroform has lately been tried, as likely to be more efficacious than the douche. M. Ricket has employed this local anesthesia by refrigeration in two cases, with partial success. The chloroform or ether was dropped upon the part, and its volatilization hastened by a current of air from a ventilating apparatus which M. Mathieu has invented. The incisions which were made in the part caused so little pain, that there could be no doubt that the sensibility was deadened by this means.¹

[However deficient in success hitherto, local anesthesia is of such great practical importance, that it well deserves to be investigated and carried out, with all the appliances and resources of modern ingenuity. We understand that Professors Syme and Simpson have made trial of the anesthetic douche in cases of abscess, but although a slight and superficial impairment of the sensibility was noticed, no diminution of the feeling of pain took place. Dr James Arnott's plan, of freezing the skin and subjacent parts, seems to us well adapted for minor operations in surgery, and we feel surprise that it has not been more extensively tried in this country. M. Velpeau has pronounced strongly in its favour. M. Richat has published some cases of operations performed without pain, by means of an ingenious apparatus, constructed by M. Mathieu (figured *Gaz. des Hop.* April 1st). By this instrument a fair amount of ether and of air is simultaneously directed to the part. Dr Liégard (de Caen) has pointed out that the peasants in Lower Normandy have long been in the habit of preventing pain by squeezing strongly the fore-arm or leg, when operations are performed on the hands or feet.]

¹ *Gaz. de Hopitaux.*

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION XII.

OVARIAN DISEASE.

RUPTURE OF OVARIAN CYST DURING PREGNANCY—RECOVERY.—The following particulars of a case of *accidental rupture of an ovarian cyst occurring during pregnancy, and followed by spontaneous cure*, was read from *Dr Gibson of Dundee* :—

Mrs L.—, a lady of very delicate constitution, and the mother of two children, but who, previous to my becoming her medical attendant, had miscarried repeatedly, and undergone at different times a long course of treatment by an eminent practitioner in one of our metropolitan cities, on account of chronic inflammatory ulceration of the uterine neck, aborted again on the 27th July 1849, at the ninth or tenth week of pregnancy, and had flooded profusely before I could reach her residence some miles off in the country.

I found reason for believing this abortion to have been the result, like the others, of a renewal of the same chronic inflammatory mischief as formerly, and had accordingly to subject her to the appropriate treatment for some length of time ere I had the satisfaction of seeing her restored to a fair measure of health and exemption from suffering. One very profuse menstruation which meanwhile occurred at the end of seven weeks from the time she miscarried, having appeared very materially to contribute to the relief of her uterine state, and to hasten on her amendment. When, in the month of October, and just at the period when the catamenia should again have made their appearance, she was suddenly seized, after a walk that fatigued her, with violent pain in the lower part of the back, affecting chiefly the left side at first, but soon stretching round to both hips, and attended with intense sickness at stomach and harassing frequency of calls to make water. After three or four days of this suffering, she found the abdomen had suddenly become enlarged at its lower part, presenting a tolerably firm round tumour of about the size of an orange, situated towards the left iliac region, and sufficiently movable to admit of its being displaced and pushed about to a certain extent.

On the 30th of October, that is, fifteen or sixteen days after all this had commenced, I was requested by her husband for the first time to see her, the pain, nausea, and irritability of stomach having been meanwhile incessant. I then found that the tumour had increased in volume to about the size of a fist, was perfectly smooth on its surface, and of a well-defined globular form, still inclined towards the left iliac region, but readily movable; tender on pressure, which at the same time aggravated the pain and nausea, and attended with a constant distressing sense of internal stretching, distension, and weight.

Altogether it appeared to me to be an ovarian enlargement of one kind or another, though the suddenness of its origin, and the symptoms attending it seemed at the moment opposed to that view; while my patient's own conviction was strong of her pregnancy, in spite of the entire novelty of her feelings and sufferings, and whatever the tumour might be.

Nor did a careful vaginal examination at this period throw much light on the matter, though I found the body of the uterus somewhat enlarged, the cervix elongated and somewhat voluminous, and the os so open as readily to admit the point of the finger, while they were all very tender on pressure.

Only the fear of her pregnancy, to which the absence of the catamenia imparted some likelihood, deterred me from using the uterine sound.

By the end of November the tumour had attained, at first very rapidly, latterly more slowly, at least the size of two fists; was round as a melon, firm but somewhat elastic to the feel, and still perfectly movable from side to side, though it generally occupied exactly the mesial line as she lay on her back, and ascended about an inch beyond the umbilicus above; while below, it admitted of the fingers being thrust deep into the abdomen between it and the pubis.

The uterus also by this time had itself undergone a very notable increase in volume, its firm, round fundus being now easily discovered to have risen to a level with the brim of the pelvis; while on a vaginal examination, the cervix and os were found situated considerably higher than formerly, and the body of the organ was felt to have expanded so much as nearly to fill up the cavity.

On the whole, the hitherto perplexing ambiguities of the case seemed to be now clearing up. The fact of my patient's pregnancy was becoming more and more probable; while it was scarcely possible to suppose the tumour to be any other than an ovarian cyst, which the symptoms gave too much reason to fear had inflamed and might be undergoing a process of slow suppuration.

Meanwhile, the patient herself was now miserably dejected in spirits, got almost no sleep, was tormented with thirst night and day, and an increasingly obstinate state of the bowels; while the pulse was generally small, soft, and feeble, and she was for the most part very chilly, with occasional very transient flushes of heat. But as she resolutely declined being farther subjected to treatment of one kind or another, and expressed her settled desire just to let things take their course, I now for some time in a great measure discontinued my professional visits, though I heard of her frequently, and, I confess, was under constant apprehension of the cyst sooner or later giving way into the abdominal cavity, and thus, as a matter of course, almost speedily causing her death.

Matters, nevertheless, continued essentially in the same state as before, till the beginning of February 1850, when all doubt of her pregnancy having, meanwhile, in due time been settled by the occurrence of quickening, my patient, in the act of suddenly starting up from the sofa, felt conscious of violently straining herself. The immediate effect of this was a great aggravation of all her previous suffering, which in a few hours reached a pitch of absolute agony, accompanied by extreme faintness and sickness at stomach. The following morning, however, her intense suffering abated, and that rather suddenly, when, from sheer exhaustion, she fell into a prolonged and sound sleep, from which she awoke much recruited, and in all respects better. It was now found that the tumour had quite disappeared, and that a great diminution had taken place in the size of the belly. Far, too, from any of the evils arising which I had dreaded so long, her health from this time improved very speedily, and she went on in comparative comfort till the 27th of June, when I was summoned to attend her accouchement. Her labour proved perfectly easy and natural, and she gave birth to a very fine boy of the average size; not a trace being to be afterwards found of the tumour, nor a single untoward symptom occurring, while she had also so completely recruited her strength as to be able to continue nursing for the usual period.

Thus terminated what appears to me a case of very singular interest; spontaneous cure and recovery taking place under circumstances so apparently desperate as the rupture of a large ovarian cyst into the abdominal cavity, and that during pregnancy. For while it will not be doubted, I think, that it was by sudden rupture that the tumour in this case disappeared—the only way of accounting, in fact, for what occurred in the beginning of February. I satisfied myself by the entire absence at the time or afterwards of any outward discharge, that it was into the sac of the peritoneum it had emptied

itself, and not by venting its contents into any of the natural outlets with which the walls of the cyst might have contracted adhesions.

RUPTURE OF INFLAMED OVARIAN CYST AFTER DELIVERY—DEATH.—*Dr J. A. Sidey* reported the following case:—

Mrs Niven, aged 36, mother of five children, had always enjoyed good health, but was of spare habit, was struck or kicked by her husband on the right side a little above the pelvis, on the 28th of October. She was then seven months pregnant. At the time of the injury she felt sick and faintish, but was sufficiently well to give evidence in court against her husband, who was sent to jail for fourteen days. Sickness and fainting recurred three or four times a day until the 1st of November, when I was sent for. I then found her labouring under symptoms of shock, she was pale, cold, and almost pulseless, and fainting on the slightest exertion. She also complained of severe pain in the right iliac region, which bore the marks of a severe contusion. She required the free use of stimulants before she was able to bear the application of leeches and a blister, which so relieved her that she was enabled to go about her usual duties in a week.

On December 29th she was delivered of a son, being attended by Mr D. Stewart. The labour was natural, and nothing abnormal discovered at the time. She progressed favourably up to Friday, 3d of January 1853, when she was seized with shivering and slight symptoms, accompanied with pains in lower part of abdomen. These were relieved next day by the use of turpentine poultices and Dover's powder.

She, however, remained in bed until Saturday 10th, night, when she rose. After sitting about ten minutes, she felt sick, and went to bed, when she was seized with very profuse hemorrhage from the uterus. She fainted several times, and became quite blanched. The hemorrhage was subdued by the use of Gallic acid and ergot, and the employment of cold cloths to pudenda. On Tuesday night it was completely restrained. She gradually improved until Friday 16th, night, when she rose and made her bed herself, walking bare-footed over a stone floor. She had scarcely got to bed when she was again seized with shivering, followed by a profuse cold sweating. Next morning when I saw her, in company with Dr G. Weir, she complained of pain in left side, and over the region of the uterus. The pain was much increased on pressure, especially on touching the upper part of a tumour of about the size of the fist, and which was situated directly behind the symphysis, rising about an inch above it, and supposed to be the uterus. On a vaginal examination, the os was slightly open, and felt velvety. On the right side of the os there was a hard tense swelling like the end of an egg. Tongue white, furred, and moist. Bowels had been moved. No dysuria. Pulse 90.

She was ordered a blister, to be applied over the tumour, and a diaphoretic mixture. In the evening she was again examined, when a swelling similar to that on the right was felt on the left side of the os, and also another joining the two lateral swellings passing in front of the cervix. She was then ordered Mas. Pil. Hydrarg. iij., Pulv. Opii. gr. i., every three hours.

18th.—In the morning she was much in the same state, the pain continuing, but not so severe. She was then ordered to have eight leeches applied to the anus. In the evening there was a tendency to tympanitis and dysuria; in other respects, the same. She was ordered a turpentine and oil enema.

19th, *Morning*.—Has had rather a restless night, bowels have not been freely moved, still a good deal of tympanitis, and also frequent desire to make water. Ordered to continue the pills, and to have an assafoetida injection. In the evening she complained of sudden and severe pain in pubic region, increased on pressure, and which was followed by symptoms of peritonitis. The pulse rose to 120—small. She had an anxious look. There was a great deal of tympanitis, and slight vomiting. She was then ordered Cal. and Opii pills every two hours, and a blister over the pained part.

20th.—Tuesday and Wednesday she continued much in the same state;

the bowels had never been freely moved, and the tympanitis and vomiting still continued. On Wednesday evening the pain suddenly left the pubic region, and removed upwards to the stomach. She complained of no pain except in that part. The tympanitis had disappeared in a great measure, but the vomiting had become stercoraceous. Dr Alison saw her, and she was then ordered *ij. gr. Opii* every two hours.

Thursday.—Passed a very restless night, and has had a good deal of same kind of vomiting, but without effort, the ejecta rising into her mouth. She is cold, and has a very anxious and sunken aspect. Pulse small, and scarcely to be felt. No pain except at stomach.

Died about half-past four.

Post-mortem forty-six hours after death.—Present Drs Alison, Keiller, Pinkerton, and Sidey.

On opening the abdomen, all the intestines were found glued together by an effusion of recent lymph, the greatest quantity being about the stomach. In the pubic region, the upper parts of a dark gangrenous mass presented, partially covered with lymph. This tumour, which rose about an inch above the pubis, on examination was found to be the right ovary, which was impacted at the time, and occupied the whole of the pelvis, lying in front and above the uterus, and also pressing upon the bladder. On lifting up the ovary, which was slightly adherent, it was discovered to be twisted, so that it presented its posterior surface as the anterior surface of the tumour.

A quantity of thick bloody fluid escaped through a rugged and ulcerated opening situated on the posterior aspect and left side, as it lay in situ.

On removal, the tumour proved to be a multilocular ovary with the ruptured opening on its outer surface, two smaller openings were seen also. On cutting into one of the largest cysts, a clot of blood about the size of a walnut presented. The intermediate walls of the other cysts showed small points of ulceration. The uterus and left ovary presented a healthy appearance.

No other parts were examined.

Remarks.—The chief points of interest in this case are three in number, viz., 1st, The strong resemblance in symptoms and signs to a case of pelvic cellulitis, as manifested by the protracted convalescence after delivery, the paleness of the surface, the frequent rigors, followed by a feeling of bearing down, difficulty of defecation, and dysuria, and on examination, the presence of a tense painful tumour, with slight fluctuation, situated on both sides of the os uteri, and also crossing in front of it. Also a distinct tumour felt projecting slightly above the pubis, and tender to the touch. These symptoms were such as fully to justify an opinion that the case was one of pelvic cellulitis. 2d, The position the ovary occupied in the pelvis and median line. The 3d, I think requires a good deal of consideration, and is interesting in regard to the medico-legal aspects of the case. But I shall merely resolve it into two questions. Did the blow cause the hemorrhage, followed by a subacute inflammation, and subsequent ulceration into the peritoneum? Or did the hemorrhage and inflammation take place spontaneously, as is often the case in ovarian tumours, results which might be promoted by the pressure necessarily exerted on such a large body by the contraction of the uterus and abdominal muscles during delivery?

Dr J. A. Sidey also stated the following particulars of two cases of rupture of ovarian cysts, occurring in his father's practice :—

RUPTURE OF INFLAMED OVARIAN CYST.—DEATH.

Miss M., æt. 36, in the year 1840, for the first time observed a tumour in the right iliac region, which on examination proved to be ovarian. The tumour gradually increased, and she had several attacks of pelvis inflammation, followed by phlegmasia dolens of the right leg. In 1841 the tumour became very large and the breathing oppressed. Diuretics and blisters were

extensively used, without any marked effect. On the 20th of June she was tapped, and about five gallons of a viscid fluid drawn off. No untoward symptoms were manifested, and her health became much improved. But the fluid soon accumulated, and in about a month afterwards she was again tapped, and two and a half gallons drawn off. Within twenty-four hours a violent rigor followed, accompanied with pain in abdomen; pulse 120. There was also great tension of the abdomen and vomiting. These symptoms continued for several days, but were at length removed. The sac filled again, and in three weeks after the second operation, the opening ulcerated and burst, and about three gallons of a limpid fluid was discharged, and also about ten ounces of pure pus. During August she improved very much, but on the 6th of September both orifices made by the trocar burst, and about the same quantity of pus, of a slightly blue colour, was discharged. At this time she became very weak, and tongue and fauces red and apthous. She, however, gradually improved with the free use of stimulants, and was able to sit up in bed. On the 22d there was again a discharge of pus through the wounds, amounting to about one-gallon and a half. On the 25th of October there was another discharge of pus, and continued to discharge for several months about ten or twelve ounces daily, generally of pus, but sometimes of a clear fluid. Her health improved notwithstanding, so that on January 1842, she was able to sit up for several hours and sew—the tumour at the time being about the size of a small cocoa-nut. On the 7th of February, after eating a hearty dinner, she was seized with intense abdominal pain and vomiting, and died in a few hours.

On examination it was found that the contents of the multilocular ovary, in a state of inflammation and ulceration, had been discharged into the abdomen.

RUPTURE OF OVARIAN CYST.—RECOVERY.

A lady, æt. 55, with the exception of dyspeptic symptoms, always enjoyed good health.

In September 1848, she experienced a sensation of weight in the left iliac region, and then, for the first time, observed a fulness there. Upon examination it was found to be the left ovary enlarged to about the size of a hen's egg; the limb on the same side varicose.

The tumour rapidly increased in size, passing over to the right side, and at length ascending rather above the umbilicus.

Shortly after it had attained this size, while driving in a carriage over a rough road, she suddenly became faint and sick, and a feeling of coldness over the whole body followed. The ovary had evidently burst, as the tumour had disappeared, and the lumbar regions, as she lay on her back, were found on percussion to be filled with fluid. Violent abdominal symptoms, and a tympanitic state of the bowels came on; but these were subdued by hot turpentine poultices, and the fluid afterwards carried off by smart purging with pulv. jalap. co. The varicose and swollen condition of the limb also disappeared. The enlarged state of the abdomen gradually subsided, and allowed the ovarian sac easily to be felt in the left iliac region. Health returned, and she was able to go about as usual. In December the ovary began to enlarge again; and in March 1849, it had attained the same size as before. Upon stooping down for the purpose of tying her boots, she, on a sudden felt very uncomfortable, as if "something had given way within her," and was immediately seized with violent pain in the back, and tympanitic state of the abdomen. Violent abdominal symptoms soon came on, and upon examination the tumour was gone. The treatment consisted in the turpentine fomentations, and jalap purges kept up for some time, and afterwards she was put under alterative doses of blue pill.

On the 1st of April, indications of the tumour enlarging again, became manifest; several blisters were applied, and various diuretics were used, without any marked benefit. About the beginning of May it had become as large as before, and burst the third time with a noise similar to a person emptying a

bottle. This was followed by great depression and rigors ; the breathing much oppressed, and countenance almost livid, the pulse very weak, requiring the use of stimulants. Abdominal symptoms then came on, which were again subdued with turpentine fomentations, large quantities of dark liquid stools followed the free use of purgatives. After the violence of the symptoms had been overcome, a strong bandage and thick compress were applied over the situation of the ovary, and was continued for several months. She again regained her health, and on examination seventeen months afterwards, only a very slight fulness in left side could be detected. She lately died from disease of the lungs, but on no occasion was there any greater fulness of the ovary detected. No post-mortem examination permitted.

The relating of these cases of ovarian disease, led to a lengthened conversation on the character of ovarian tumours, and on the various modes of treatment proposed by authors.

Dr Weir referred to a case of ruptured ovarian cyst without bad consequences that he had treated by large drastic purgatives. After the rupture, the cyst again enlarged and became the seat of acute inflammation, which resulted in a complete cure of the dropsy.

Drs Simpson and Moir also referred to cases of rupture of ovarian cyst occurring under their care.

Dr Keiller directed the attention of the Society to a case of parturition in a patient who had previously undergone the operation of ovariectomy.

ON THE TREATMENT OF OVARIAN DROPSY BY INJECTIONS OF IODINE INTO THE CYSTS. BY PROFESSOR SIMPSON.

It has been often proposed to treat dropsy of the ovary upon the same principles as hydrocele or dropsy of the tunica vaginalis. In accordance with this view, *Drs Hamilton, Scudamore, and others*, have in former times injected ovarian cysts with irritating solutions of sulphate of zinc, etc. ; but the results have in general proved so unfortunate and disastrous as to prevent a repetition of the practice.

In 1832, *Mr Martin* first recommended the use of tincture of iodine as the surest and safest injection for the cure of hydrocele ; and this drug seems now almost universally adopted by surgeons in the obliterative treatment of this variety of local dropsy in the male subject.

Latterly, various surgeons, particularly *Velpeau, Boinet, Belluerimi, etc.*, have extended the practice of iodine injections to the treatment of other local dropsies and cysts ; to chronic abscesses ; diseases of the joints, etc. And the past experience of surgeons on the subject would certainly seem to show that while the local and direct application of iodine to morbid secreting surfaces has a great power of modifying, altering, and arresting even the secretory action of these surfaces, and often changes suppurative into adhesive inflammation, it shows at the same time wonderfully little aptitude to excite any excess of local irritation and pain. Hence naturally arose the question whether it could be safely and successfully injected into such large cysts as those of the common form of dropsical ovary.

In 1846, *Dr Alison, of Indiana*, recorded the history of a chronic case of ovarian dropsy that had been repeatedly tapped, and which he injected at last with a solution of iodine. Severe symptoms followed, but the ultimate result seems to have been favourable. In 1851, *Dr Simpson* assisted *Mr Syme* in injecting a cyst in the neighbourhood of the ovaries, but not a common cystic form ovary. The symptoms which ensued were those of considerable excitement ; but the original cyst apparently became obliterated. Another one in its vicinity has lately shown itself in this patient.

Within the last year, *Dr Simpson* has, subsequently to tapping, injected into dropsical ovarian cysts the tincture of iodine in seven or eight cases. For this purpose he has employed the common tincture of iodine of the Edinburgh Pharmacopoeia, undiluted. He has usually thrown into the cyst two or three

ounces of the tincture. In some cases he has allowed a portion of the injected fluid to re-escape; in others has retained the whole of it in the sac of the cyst that was tapped. From these cases he drew the following conclusions:—

1. In none of the cases of ovarian dropsy treated with iodine injections after tapping has he yet seen any considerable amount of local pain follow the injection, with one exception; in most instances no pain at all is felt; and in none has constitutional irritation or fever ensued. In the one exceptional case considerable local irritation followed; and the pulse rose to 110; but the same phenomena occurred in the same patient after previous tapplings without iodine being used.

2. While the practice seems thus so far perfectly safe in itself, it has by no means proved always as successful, as in hydrocele in preventing a reaccumulation of the dropsical fluid; for in several instances the effusion into the sac seems to have gone on as rapidly as after a simple tapping without iodine injection.

3. But, in two or three of the cases, the iodine injection appears to have quite arrested, for the time being, the progress of the disease, and to have produced obliteration of the tapped cyst, as there is no sign whatever of any reaccumulation, though several months have now elapsed since the date of the operation.

Lastly. Accumulated experience will be required to point out more precisely the special varieties of ovarian dropsy most likely to benefit from iodine injections, the proper times of operating, the quantities of the tincture to be injected, and other correlative points. Perhaps the want of success in some cases has arisen from an insufficient quantity of iodine being used, and from the whole interior of the cyst not being touched by it. The greatest advantage would of course be expected from it in the rare form of unilocular ovarian cysts. In the common compound cyst the largest or most preponderating cyst is usually alone opened in paracentesis; and though it were obliterated, it would not necessarily prevent some of the other smaller cysts from afterwards enlarging and developing into the usual aggravated form of the disease.

NUMEROUS FISSURES IN THE PERITONEAL COVERING OF THE UTERUS.—*Death 6½ Hours after Labour.*—Dr C. Skae related the following case:—About eleven o'clock A.M. on Saturday the 8th of May 1852, I was requested to visit Mrs S. in her seventh confinement, symptoms of labour having come on two hours previously, attended with flooding. On examination, the os uteri was found scarcely at all dilated, and the hemorrhage had almost ceased. The pilul plumbi opiat was administered, and cold applications directed to be applied externally, in case of any return of flooding. On again visiting, at about half-past three P.M., the os was found dilated to the extent of a crown-piece, pains brisk, with a mere show of coloured discharge. After rupturing the membranes, a dose of the liquor secal was given, and increased uterine action speedily followed. Labour went on steadily till half-past seven P.M., when a dead foetus was expelled, and the placenta was thrown off without any extractive interference, or any amount of coagula beyond what is usually met with. About half an hour afterwards, while conversing with her husband in his garden, the nurse sent to say that Mrs S. was flooding. On undoing the binder, the uterus was found somewhat relaxed, and some eight ounces of coagula were found to have been expelled. The uterus was kept steadily compressed, a dose of the ergot of rye given, and an ounce or two of brandy, as the circulation was feeble, and the countenance indicated considerable prostration of the vital powers. From this time there was no return of hemorrhage, and the uterus continued well contracted; still, the countenance and circulation indicated an increasing loss of strength, notwithstanding the repeated exhibition of stimuli at short intervals. About half-past nine P.M., I became apprehensive of her safety, and speedily had the assistance of Drs Zeigler. An opiate was administered, and other means used calculated to obviate the rapid ten-

dency to sinking, which now became painfully evident, but with scarcely any benefit. About eleven P.M., Dr Simpson, with the assistance of Mr Carmichael, transfused from the arm of a stout maid-servant some five or six ounces of blood, but with scarcely any appreciable effect. The circulation steadily declined, and she sunk at two o'clock A.M. on Sunday morning, six hours and a half after delivery.

On examining the placenta, it was found that a segment of it had been separated from the uterus during labour, which was sufficient to account for the accidental hemorrhage, the total amount of which, from first to last, could not have exceeded fifteen to eighteen ounces.

The urine was examined, and found slightly albuminous.

Autopsy.—Some four or five ounces of extravasated blood were found in the peritoneal cavity, and both broad ligaments had an ecchymosed appearance. The anterior surface of the uterus exhibited some twenty-five or thirty fissures of an oblique irregular appearance, extending merely through its peritoneal investment, varying from three to six or eight lines in length. No other notable morbid appearances were found, with the exception of slight fatty degeneration of the kidneys.

A conversation ensued on cases of death *post-partum*, from hemorrhage, placenta-previa, etc.

NEW VACCINATOR.—*Dr J. Struthers* exhibited a vaccine scarificator which he had received from Dr Marcus, a surgeon in the Danish navy, and which had been invented by Professor Dryer of Copenhagen. In form it resembles the iron pen, with two legs and screw for regulating their distance, commonly contained in cases of philosophical instruments; but the extremities of the legs, instead of being pointed, are broad and sharp, similar to those of the double knife of Valentin. The advantages of this instrument are—1st, That the scarification and vaccination are effected by one application of it. The point, previously dipped in lymph, being simply drawn over the skin, scratching it to the required depth; and 2d, The rapidity with which a number of individuals can be vaccinated at a time; the quantity of lymph capable of being held between the blades, being sufficient for 20 or 30 applications. This instrument is extensively used in Denmark, where the practice of revaccination is general, and is almost the only one employed by the army and navy surgeons, who consider it superior to those in use in this country. Dr Marcus also informed Dr S., that in Denmark the practice of preserving the vaccine lymph, in capillary tubes, has been general for several years past.

UNINTERRUPTED SERIES OF PRETERNATURAL LABOUR IN THE SAME WOMAN.—*Dr Pattison* related the particulars of an uninterrupted series of preternatural labour occurring in the same female. The woman had given birth to five children, four of which presented by the feet, and one by the breech. Nothing remarkable in her conformation was observed, nor were there any other circumstances known that could account for her children always presenting by their pelvic extremity.

Dr P.'s case led to a conversation on the position of the child in utero, and the probable causes of malpresentation. The opinions of Nægale, and others, as to the effect of early irregular uterine contraction, in determining the presentation, were referred to. Dr Simpson stated that, whatever be the true explanation of preternatural presentation, he believed that the child always adopted that position in utero, which was freest from irritation, and that peculiar shapes of the uterus, from congenital conformation, or otherwise, might thus act in producing corresponding irregularities in the position of the child. In this way, he believed that the malpresentations which are sometimes observed to occur in members of the same family, may be explained, peculiar shapes of the uterus being probably influenced and determined by family predispositions, as much as peculiar shapes of other organs are.—Cases of malpresentations occurring in members of the same family were related by

various members, which tended to establish predisposition as a probable occasional cause of preternatural labour.

Illustrative cases were cited by Dr Keiller, Dr M'Cowan, Dr Malcolm, and Dr Simpson.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIII., 1853-54.

March 1, 1854.—J. Y. SIMPSON, M.D., President, in the Chair.

ON EXCISION OF THE KNEE-JOINT. BY R. J. MACKENZIE, F.R.C.S.—*Mr Mackenzie* said that he had now performed this operation in three cases. The first would be immediately presented to the Society, and would speak for itself. Recovery in the second case had, from various causes, proved more tedious than it had at first promised to be. The patient, however, progressed satisfactorily, though slowly; and Mr Mackenzie had no fear as to the ultimate result. The limb was becoming rigid at the knee, and was straight, shapely, and but moderately shortened. He hoped at a future meeting to present this patient, with a sound and serviceable limb. In the third (a peculiarly unfavourable case for the operation), the result had been fatal. The case was one of acute ulceration of the cartilages, accompanied by suppuration; the joint being distended with pus. The patient, a lad of 18, was in an advanced stage of hectic, emaciated, and much exhausted by continued suffering. He was most reluctant to submit to amputation; and, having seen one of the patients walking about, on whom the operation of excision of the knee-joint had been performed, he expressed an anxious wish that an attempt should be made to save the limb by the performance of this operation. It was a matter of regret that this request had been complied with; for, although amputation held out but a small chance of recovery, the case was little suited to test the merits of the operation of excision. Immediate and great relief followed the operation; and, as far as the limb was concerned, everything had progressed as favourably as could have been wished. Diarrhoea, however, which had been threatened before the performance of the operation, set in; the tongue and lips became covered with aphthous crusts; he suffered from cough and night sweats; and sank, in the course of a month after the operation. The wound was in great part healed, and no difficulty had been experienced in keeping the limb in excellent position. Permission could not be obtained to examine the body; but Mr Mackenzie had little doubt that tubercular disease of internal organs would have been found.

Mr Mackenzie stated that, since the subject had been last brought before the attention of the Society, the operation had been performed by several able surgeons in England, Scotland, and Ireland. He had written to four of these gentlemen, requesting them to furnish him with the result of their experience; and he had received from all very kind and full replies to his inquiries. He read some extracts from the letters which he had received from Mr Page of Carlisle, Mr Gore of Bath, Dr Keith of Aberdeen, and Mr Butcher of Dublin, all of which gave favourable accounts of the cases under their care, and expressed themselves as being advocates of the operation.

A patient (a man between forty and fifty years of age), was then exhibited, in whom Mr Mackenzie had performed the operation of excision of the knee-joint a year previously. He appeared to be in robust health, and traversed the hall, in various directions, with ease and celerity. In walking, the toes were pointed downwards, but, his body yielding slightly, he planted the foot firmly on the ground, and rested his entire weight on the limb without uneasiness. In the horizontal posture, he elevated and depressed it with the greatest nicety. On examination, the limb was found slightly curved outwards, firmly ankylosed at the knee, three quarters of an inch shorter than its fellow, and equal in muscular development. The heel of the shoe was raised inside three-fourths

of an inch, to allow for the shortening. He stated that he had been walking for six months.

Mr Syme said, that the question was not as to the possibility of saving the limb, but how far excision of the knee-joint could be substituted for amputation of the thigh, and an useful limb preserved. Now, all subsequent experience went to show that the operation was not a good one, and that it could not with advantage be substituted for amputation, even admitting the possibility of keeping on the limb. He questioned the usefulness of the limb in the case shown to the Society. The future must be looked to ; and much depended on the avocations of the patient. Should they be rough and laborious, the limb would speedily be affected with swelling or ulcerations, and amputation would become necessary. Again, how many times have the limbs been lost, or unsatisfactory results obtained ? The question was not *Mr Mackenzie's* skill as an operator ; but how far we could regard this operation as a boon to the human race.

Mr Mackenzie was sorry to hear the statements of *Mr Syme*, which he could not but regard as the results of mere theory, and not deduced from a fair consideration of the facts which had been brought forward. He would only refer to the opinions of some of the best surgeons in the country, whose recent experience of the operation bore ample testimony to its value, as shown in the letters which he had read to the Society. *Mr Syme* had spoken of the great loss of life following the operation. In this *Mr Syme* was mistaken ; of the last fifteen cases in which the operation had been performed, three cases only had terminated fatally ; a proportion which would surely bear comparison with the operation which *Mr Syme* recommended, amputation of the thigh. *Mr Syme*, too, spoke of the useless limbs which were preserved. He would refer to the patient whom they had just seen, and would leave the Society to judge whether his limb were useless ; he would refer to such cases as that of *Mr Page*, where the patient was standing or moving about at his employment the whole day, sometimes walked six or seven miles to the country for his amusement, and could walk with any of his companions. He would refer, indeed, to the gratifying results in this respect of nearly every case in which the operation had been performed of late years. With scarcely an exception, the operation had been the means of saving a truly serviceable limb.

Mr Syme did not expect to change *Mr Mackenzie's* opinions. The object of his remarks was to state his own ; and a little time would show which were correct. He might be wrong in the conclusions to which he had come ; but he begged to adhere to them.

ON VELO-SYNTHESIS, OR THE OPERATION FOR CLEFT PALATE. BY JAMES SYME, Esq.

Mr Spence differed from *Mr Syme* as to *Mr Fergusson's* plan of operation being impracticable. Some years ago he had assisted a friend in performing the operation according to that plan, and no difficulty was experienced in keeping the parts tense during the section of the muscular fibres. The knife used on that occasion was one made after the pattern represented in *Mr Fergusson's* work. From what he saw at that time, he felt doubtful whether the levator palati could be divided by a knife so short in the blade ; and he had subsequently made several experiments and dissections to satisfy himself on that point. He found that though with the knife in question the muscle could only be partially divided, complete section could be accomplished with ease and certainty by a knife a little longer in the blade. The palato-glossus and palatopharyngeus, he believed, could scarcely be completely divided with safety or certainty. The most doubtful point to him was, whether the horizontal division or splitting of the soft palate, necessary to effect section of the levator palati, might not sometimes interfere with the vitality of the part, and so prevent union.

Mr Mackenzie could entertain no doubt as to the possibility of entirely dividing the levator palati, which was the principal desideratum in *Mr*

Fergusson's operation. As he understood Mr Fergusson's method, it was not generally necessary to divide more than the anterior fibres of the palatopharyngeus, whilst the palato-glossus rarely required to be interfered with at all. With regard to the results of Mr Fergusson's operation, which had been described by Mr Syme as such a useless and improper proceeding, he (Mr Mackenzie) had lately learnt on inquiry, that Mr Fergusson had performed the operation in thirty-three cases, and that with one or two exceptions, the results had been nearly perfect.

Mr Syme objected to the interpretation Mr Mackenzie had put upon what he had said. His (Mr Syme's) remarks only went to show that the palatal muscles need not be divided to ensure good results in the operation; but he had no wish to throw any doubt on the amount of success claimed by Mr Fergusson.

Dr James Johnston, when in Canada, had assisted Dr Home, an army surgeon, in performing the operation for velo-synthesis, as recommended by Mr Syme. No muscles had been divided; and a successful result followed.

ON THE WEIGHT AND SPECIFIC GRAVITY OF THE BRAIN IN INSANITY. BY DAVID SKAE, M.D.—This paper will be published at length in a future number.

On the President asking for any medical news—

CARDIAC DISEASE.—*Dr Bennett* said that there was in the medical clinical ward of the Royal Infirmary, a case in which all the signs of disease of the valves of the pulmonary artery, hypothetically laid down by the late Dr Hope (who had, however, never met with a case), were present.

MORTALITY FROM MEASLES.—*The President* said that during the present epidemic of measles, it had been remarked that no deaths had taken place at its commencement, but that latterly some fatal cases had occurred. The Society would be glad to hear from any member what the patients had died of. He stated that he had himself seen three or four of the fatal cases. In one death was caused by sloughing of the throat, resembling noma, and such as had been described by Dr West of London. In another there were pneumonia and pleurisy and peritonitis, with indeed general inflammation of almost every internal organ. Of other two cases the first had not been recovering satisfactorily, when symptoms presented themselves which were attributed to hydrocephalus, but rapidly became tetanic, and she died of tetanus next night, having been ill about forty hours. On *post mortem* examination, one large and several miliary tubercles were found in the brain. The second case, which occurred shortly after the former, and what was remarkable, in the practice of the same gentleman, had passed through the measles so easily as to require nothing in the shape of drugs to be prescribed. Tetanic convulsions suddenly supervened; they were the most severe that Dr Skae had ever witnessed. The patient died in thirty hours. On examination after death no lesion could be found. There had been ischuria from the commencement of the attack of convulsions, and no urine could be obtained for examination. The bladder was found empty.

HYSTERICAL SPASMODIC COUGH.—*Dr Andrew Wood* said that about a fortnight ago, in a public institution for the education of girls, one of the inmates was seized with a peculiar spasmodic cough, coming on in paroxysms every half hour or twenty minutes, and for some time he was at a loss; but others of the girls becoming affected, Dr Wood recognized an old friend, which had come under his notice in a kindred establishment about two years ago. About a dozen of the inmates were then affected, and on coughing made a sound resembling the cry of a turkey-cock. No remedy seemed to check the complaint, except removing those affected to their homes. In one case, where from the violence of the symptom the larynx was feared for, chloroform, then but newly introduced, had an admirable effect. In the present instance only three or four cases have occurred, those affected having been removed to separate houses,—an opiate usually completing the cure.

April 5, 1854.—J. Y. SIMPSON, M.D., President, in the chair.

CASE OF HYSTERIA AND SPURIOUS PREGNANCY. BY DR KEILLER.

THIS case, an abstract of which appeared in the Journal for 1849-50, in the proceedings of the Obstetric Society of Edinburgh for March 1850, was given in great detail. The patient, an hysterical young woman, was supposed by her friends to be in the family way, from the large size of the abdomen, and the occurrence of pain, which elicited such loud cries that the whole neighbourhood was disturbed. Her supposed labour continued for many weeks, and Cæsarean section was proposed, as a last resource, by an irregular practitioner. When Dr Keiller saw her, and made a careful examination, he attempted but in vain to disabuse of the minds of her relations that she was pregnant. A variety of treatment was instituted, but without any marked benefit; indeed, change of air was the only thing that seemed to do her any good. After one of these removals, she was able to return for a short time to her usual employment at a mill, though her abdomen always remained of a large size. Dr Keiller lost sight of her till eight years after she presented herself in Edinburgh, with a child that she had had in the interval, complaining that her old symptoms were reappearing. Her abdomen was very large and tympanitic. She was sent into the Maternity, where, with the advice of Dr Simpson, she was put under the influence of chloroform. The abdomen immediately became flat, and the uterus was felt to be of its normal size. When the effects of the chloroform passed off, however, the abdomen again enlarged in size, and resumed its former bulk. During her stay in hospital, she introduced foreign matters into her urine, which on one occasion presented a dark colour, as if from the admixture of ink, for, on the catheter being passed, limpid urine was drawn off. She left the Maternity, and, when last heard of, was still suffering from her obscure complaint. Dr Keiller stated that a somewhat similar case, though not complicated with the symptoms of pregnancy, was at present under his care in the Hospital, in which chloroform had the same effect in removing the distension, and galvanism was similarly serviceable.

Dr W. T. Gairdner had perused an essay lately published by Mr More O'Ferral of Dublin, on *Abdominal Tympany*, in which a similar observation on the use of chloroform in the diagnosis of such cases was made, but with no mention of the previous investigations of Dr Simpson. It was interesting, however, to find a collateral and independent testimony to the value of the agent.

The *President* said, that his own observations on the use of chloroform in the diagnosis of such cases would be found in the same journal, at least three or four years before the appearance of Mr O'Ferral's paper. He believed that cases of spurious pregnancy were often met with in practice; for many a married lady would acknowledge, when questioned, that she had once or twice thought herself pregnant, when, as the result showed, she was not. He had, in his own practice, seen several cases in which ladies had removed into town with their entire establishments, in the full belief of their approaching confinement, and where he had had the disagreeable duty of informing them that they were not in the family way. These were instances in which patients progressed onwards to near the full term of pregnancy, suffering all the usual symptoms and discomforts of that state. Sometimes phenomena exactly like those of labour came on at the full term. This seemed to have occurred in Dr Keiller's patient. Dr Simpson had seen several analogous instances of spurious pregnancy terminating in spurious parturition. Besides the cases of pseudo-pregnancy, in which the patient went on with the usual symptoms of pregnancy till near or up to the full period of labour, there were other varieties of this curious morbid state. In some, for example, the affection lasted only for a few months; in others, they occasionally continued far beyond nine months, and became, as it were, chronic in their character. As to the symptoms themselves, they consisted of the presence of more or fewer of all the

usual sympathetic symptoms of pregnancy, as swelling of the abdomen, nausea, and sickness, a feeling of quickening and motion of the child, etc. Dr Keiller had stated that the mammary signs were not well marked in his patient. Sometimes, however, they were ; and he (Dr Simpson) had sketches illustrative of this fact, executed by the patient herself during a state of spurious pregnancy, and in whom, in a subsequent veritable pregnancy, her first, the areolæ did not present a deeper tint than they had done during her pseudo-pregnancy. As to the sensations which had been described in such cases, they were very frequent, and sometimes there were true motory contractions in the abdominal walls. Dr Simpson was inclined to believe that the malady was connected with the ovary. In one aggravated case which had fallen under his observation, there was marked ovaritis, and the ovary subsequently suppurated. Again, it was observed that, although menstruation did occur in the cases which had been noticed, yet it was much scantier than usual, and sometimes the catamenia were wanting for several months. It was well known that the complaint was not peculiar to the human female. Harvey had long ago remarked that, in hounds who were well fed, many of the phenomena of pregnancy, such as swelling of the abdomen, and the presence of milk in the mammæ, occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, but during which there was no intercourse with the male : kittens and other young animals were frequently stolen by the animal, to make the semblance of a litter. The curious observation had also been made by Harvey, that the animals were liable to the diseases of bitches which had recently been delivered. He (Dr Simpson) believed that, when some of the cases of the so-called hysteria in unmarried females were inquired into, they would be found, as he thought he had repeatedly seen, to be really symptoms only of spurious pregnancy. Dr Keiller had alluded to the retraction of the limb as having been a well marked symptom in his patient. He (Dr Simpson) had seen a case many years before the introduction of chloroform, in which the symptom was a prominent one. The lady had been under treatment in Paris, and, on her return to Scotland, a surgeon examined her, and recognized, as he supposed, the presence of a large ovarian tumour—not an uncommon mistake, as, in the records of ovariectomy, six cases are detailed in which no tumour could be found after the abdomen was opened. Dr Simpson was consulted, and, on percussing, he found the abdomen quite tympanitic, and of course negatived the tapping, and the presence of any ovarian tumour. The leg in this case was much drawn up, and he was informed that, while in Paris, the heel was for a time closely applied to the back of the neck. With regard to the nature of the abdominal swelling in spurious pregnancy he had tried various experiments to ascertain its cause, but in vain ; and as yet he could come to no decided conclusion on the subject. It had been suggested that, while the patient was deeply under the influence of chloroform the contained air escaped unobserved ; but in one very marked case in the hospital, he had a tube passed *per anum*, its nozzle being kept under water ; but not a bubble of air escaped. The diaphragm he was inclined to suspect to be a chief agent in the production of the swelling. The chloroform, he believed acted by relieving the muscles, diaphragmatic and abdominal, from the influence of reflex action, and permitting their relaxation. Dr Keiller had classed the interesting case which he had communicated under the head of hysteria ; but some of the cases to which he (Dr Simpson) had alluded, could scarcely be properly included under that designation. The phenomena were common to the females of our domestic quadrupeds, and in them would not be designated hysteria. Dr Simpson suggested that a series of experiments should be made on the bitch during the occurrence of the anomalous pregnancy, to ascertain, by examination of the state of the ovaries and uterus, the true nature of the cause.

Dr Matthews Duncan had lately seen a case where the explanation of abdominal swelling was completely made out. A young lady had been long in

bad health ; her chief symptoms were referrible to the left hypogastric region, which was very tender, dull on percussion, and extremely distended. Called in consultation, he recommended the production of deep anæsthesia as a diagnostic aid. Long before the girl was insensible, he remarked the great anterior arching of the lumbar vertebræ, which, as insensibility came on, disappeared ; the spine coming to touching the mattress, from which it was before far removed. At the same time (no doubt from relaxation of the diaphragm and anterior abdominal muscles) the abdominal distension disappeared and also the left iliac fulness, to the great relief of her anxious friends. He thought it very important to distinguish cases of spurious pregnancy from cases of false pregnancy. Real cases of the former disease were common in the lower animals, but, he thought, not very common in women. From these, where not only was there no deception, but the real existence of many of the signs of pregnancy, should be carefully separated cases of false pregnancy. These last were of various kinds. In some, the female simply wished to deceive, in others, she was herself simply deceived, easily undeceived, and the phenomena found were explicable without supposing spurious pregnancy. In a third class of false pregnancies, we had a good example of the so-called electro-biological state. The female's mind was impressed with the unshakeable conviction that she was pregnant, and under these circumstances some of the symptoms of pregnancy were induced.

The *President* had made some experiments on the subject in the wards of the hospital, and had satisfactorily ascertained that in the cases he had examined, the prominence of the abdomen did not depend upon any arching of the spine.

Dr Keiller concurred with *Dr Simpson* with reference to the agency of the spine. In the case at present under his care in the hospital, and to which he had alluded, particular attention had been paid to the position of the spine ; and he could not detect any connection between it and the abdominal swelling. He believed, with *Dr Simpson*, that the peculiar appearance was mainly due to the action of the diaphragm, and he had observed that patients labouring under the disease could voluntarily increase the size of their abdomen by a little effort. *Dr Montgomery* of Dublin, in a note which he (*Dr K.*) had lately received, stated that he had seen a number of cases of the same kind, and that one somewhat similar to the one just read to the Society, was recorded in his book, *On the Signs, etc., of Pregnancy*, p. 173. *Dr Montgomery* went on to say, that the "whole subject was full of wonders, almost justifying Harvey's strange idea that the conceptions of the brain and uterus were to a certain extent identical and might change places" !

CASE OF RUPTURE OF THE ILEUM. BY F. THOMPSON, M.D., OF PERTH.

The patient was admitted into the Perth Infirmary with the symptoms of acute peritonitis, the effects of a blow which he had received on the groin about two hours before, from a piece of wood which was being cut by a circular saw, and which had started back. He gradually sank forty hours after the receipt of the injury. The pulse never rose above 70. On examination, the ileum was found ruptured in two places, the contents of the bowels had been extravasated, and the peritoneum presented the usual morbid appearances of acute inflammation. Very faint marks of violence were observed on the site of the blow, but extensive ecchymosis existed over the ribs of the same side, where no injury could be ascertained to have been received.

Dr W. T. Gairdner said that the case was important in a medico-legal point of view, as the question might be raised, Whether the openings in the bowel were the result of violence or of disease? It, however, admitted of a satisfactory answer. In apertures, the result of external violence, there was invariably eversion of the mucous coat of the intestine—a fact which was explained by the greater copiousness of that structure, and its lax connection with the serous layer. In apertures, however, which were produced by disease, though both layers were

involved, the mucous membrane was destroyed to a much greater extent, and was not found everted. In the specimen which had just been handed round, there could be no doubt, from the appearance of the openings, that they had been produced by external violence.

Dr Matthews Duncan observed that, at first sight, it appeared difficult to understand why external violence should produce a circular opening, as if a piece had been punched clean out of the bowel. This was aptly explained by observing the effect of a lineal incision in the uterine wall, in performing Cæsarean section. The effect of the incision was to make a circular opening, as had been remarked by Sir Charles Bell. In the same way, the opening of the bowel was by the contractions of the variously directed muscular fibres quickly changed from a lineal or irregular wound to a circular one; and thus the curious phenomenon might be explained.

The *President* asked *Dr W. T. Gairdner* if any explanation had as yet been given of the presence of two circular ulcers occasionally met with at the pyloric end of the stomach. He (*Dr Simpson*) had seen them occupying positions exactly opposite to each other, and in the line of the axis, which, according to physiologists, the bolus of food assumed at that portion of the stomach. These ulcers were generally met with in servant girls who had come into town from the country, and were subjected to a change of diet in the shape of a greater quantity of solid food. Indeed, one of the first items in their treatment was the substitution of liquid nourishment.

Dr W. T. Gairdner said, the difficulty was to explain the destruction of the mucous membrane; but if we could suppose that it had suffered violence from something which had been swallowed, then, the mucous coat once through, he believed that the gastric juice would keep up a sufficient irritation to prevent the healing of the ulcer. Similar ulcers were sometimes, though rarely, found in the duodenum, but, as far as his observation went, never lower down.

April 19, 1854.—J. Y. SIMPSON, M.D., President, in the Chair.

CASE OF CATALEPSY. BY JOHN COLDSTREAM, M.D.

THE case was detailed at great length; the following is a short abstract of its history. The patient was a lady, aged 29, of sanguine temperament. She had laboured some years before under lateral curvature of the spine, but her health continued good till 1852, when she was seized, while in the country, with the symptoms of simple fever, during the convalescence from which, she betrayed certain aberrations of intellect. She was brought into town for change of scene, and for some time she continued excited, capricious in her temper, and shunning society. In the course of the summer these symptoms disappeared, and in August she was in her usual state of health. Again, however, in September, she began to shun her friends, averting her eyes from persons who addressed her. During five months from October, she passed through many phases of mental complaints, and appeared disinclined to use her mental faculties. Prior to menstruation it was noticed that her head was hot and face flushed—symptoms, however, which were relieved by the flow. At this time she refused to speak, at times, for hours together: her silence was evidently voluntary. Her symptoms varied from time to time, but mental aberration was absent, though she was still suspicious of her strength of mind. In March 1853, she began to be obstinate and complained of illness; she appeared depressed, and her extremities were observed to be cold. A few days after she threw herself from bed on the floor, and on her physician being announced, declared that she would never speak to him again. She kept her word, but she was subsequently dumb to every one. She immediately became cataleptic. In April, the last phase of her disease made its appearance, and lasted till her death, which took place in the following September. There was catalepsy, extreme quiescence and rigidity, anæsthesia, and unwillingness to eat. She lay on her back, and seemed to be destitute of feeling and consciousness, except for one hour in the morning, when she busied herself at her toilette, but if any

one entered she instantly became cataleptic, and remained so as long as the visitor was in the room. When any attempt was made to move her, her muscles instantly became rigid. Even the eyes and the auditory canal appeared to be in a state of anæsthesia. She never spoke but twice: once during the application of galvanism, and again a short time before death. No words addressed to her seemed to affect her, except on one or two occasions. Music had no effect upon her. Her attendants firmly believed, however, that she noticed all that passed around her. She slept regularly, muttered occasionally in her sleep, but there was no somnambulism. Her resistance to take food dated from the middle of April. The approach of the spoon to the mouth produced violent contractions of the muscles of the lips and jaws, the face becoming flushed during the struggle. For the sake of brevity, as the case was very protracted, Dr C. classed the description of the symptoms under several distinct heads. And, 1, As to the cataleptic symptoms. They were always readily manifested when she was awake; a rude touch (a slight one was ineffectual), or any attempts at passive motion sufficed to produce them. They could be overcome, however, by exertion on the part of the attendants. She could balance her body on one limb, and maintain the posture for several minutes. A weight of 10 lbs. could be suspended on the extended arm from the wrist without deflection; a little more added to the weight, however, sufficed to pull the arm down. (In the case recorded by Heberden, 7 lbs. attached to the hand proved sufficient to bend the arm). While in the horizontal position, she bore a weight equivalent to 12 lbs. on the lower limbs, when these were raised a short distance from the sofa. Uneasiness was only expressed by her on being made to assume for some time a half sitting posture, with the limbs placed horizontally. The eyelids were generally closed: on being touched, contraction of the orbicularis followed, but on force being used, the elevators became cataleptic. The eyes were directed upwards, and had a vacant expression; the irides being more sluggish than natural. The symptoms were more pronounced in the after part of the day. The experiments that were made evidently induced fatigue, and on force being employed there were paleness of the face and quickening of the pulse. 2. As to the Quiescence. As already stated, for one hour only in the day did she move about, at other times not a muscle moved. Indeed, her appearance of absolute stillness produced a feeling of awe in the beholder, and almost induced the belief that we were looking upon a corpse. 3. The Anæsthesia. It was difficult to ascertain its extent correctly, as no response was elicited by any stimulus, except when galvanism was applied, and food was attempted to be introduced into the mouth. The most tender parts of the body were pressed, etc., etc., were touched by flies, or pricked with pins, and no shrinking was produced; yet whenever an attempt to give food was made, violent movements took place. Noises, or nauseous substances in the mouth had no effect upon her, but she winced under a bright light. The resistance to food appeared to have no connection with the kind presented to her; the gustatory nerves seemed as much affected as any other. The motions following the touch of the spoon showed that the lips were not anæsthetic; yet they, along with the face, were frequently seen covered with flies without the slightest motion resulting. The application of hartshorn to the nose only produced a flow of tears. 4. Speechlessness; at first only the will was lost, and it might be attributed to hysteria, and afterwards she was ashamed of her taciturnity; but the silence that came on subsequently with the catalepsy began with her determination not to speak to her medical man, though she extended it to every one. During sleep she occasionally spoke; once during the action of the galvanic machine she called out "stop, stop," and when near death she faintly said "the sofa." 5. As to the resistance to the introduction of food. It was similar to what is observed in many cases of melancholia and mania. Even after emaciation had long existed, the attempt to feed her was attended with flushing of the face and a flow of tears, but the

hands were never raised, or the head bent back in resistance. Food when once introduced into the mouth was seldom rejected, though it was swallowed slowly. Digestion seemed normal. The pulse became more quick as the atrophy progressed. There was no bruit at the heart, and the respiration was soft and slow, as if she were asleep. The urine was healthy, it was generally passed during the struggles at her meals. The catamenia appeared shortly after the first appearance of catalepsy, but not afterwards. Her temperature was low, and the extremities generally cold. Death took place by exhaustion six months after the first occurrence of the catalepsy: the muscles preserving their rigidity to the last. No post-mortem examination could be obtained.

AS TO THE CAUSES.—The predisposing were present in her active, sensitive, and by no means robust frame. She had sustained a severe shock from the suicide of a young friend, about a year before her illness, but recovered from it sooner than had been anticipated. The friends could see no connection between this and her subsequent illness, but the author thought it was not improbable that the shock sustained at that time was the remote cause. As to the treatment; he at first feared melancholia or mania, and hysteric symptoms were also present, but he felt hopeful as to the issue of the case, as the patient was originally of a strong intellect, and of a cheerful disposition of mind, and from time to time there were signs of improvement. After the catalepsy set in, the head especially was attended to, and blisters, followed by turpentine enemata, valerian, valerianate of zinc, aloes, musk, cod-liver oil, etc., and, subsequently, the inunction of olive oil were had recourse to. Electricity, gradually increased in strength, was tried, and after a time one wire was placed over the epigastrium, while the other was applied below the knee. Strong contractions of the abdominal muscles followed, the face was flushed, and the head raised from the pillow, while she cried out "Stop, stop." He was of opinion that she was rather injured than otherwise by the use of the galvanism, and the morning activity certainly lessened. Chloroform was administered on one or two occasions; she slept after it, with her mouth more open than usual.

In the present case he was inclined to believe that there was slight organic disease of the brain, the result of the nervous shock which had been sustained, and which in another individual might probably have produced mono-mania. A remarkable circumstance was the absence of any lengthened remissions, which made the case differ from the other cases on record. He could not regard the malady as feigned, as no motive was discoverable for the production of the symptoms.

The *President* said that he had seen two examples of the disease; one in his student-days in the wards of the Royal Infirmary, under the care of Dr Duncan, jun., which came on with mental excitement; and the other in the City Lunatic Asylum, a few years ago, in which the symptoms were not so well marked. With regard to the interesting case which had just been read, he could corroborate what Dr C. had said as to the feeling of awe produced by the appearance of the poor patient. She was clothed in white, and as she lay on the sofa, in her great emaciation, with the stillness of a statue, she contrasted remarkably with her former self, as she was depicted in glowing colours in a picture which was suspended exactly opposite to her. This, with the stillness of the room, and the subdued manner of the attendants, all tended to form one of the most curious sights he had ever seen. He would be glad to hear from the members any new pathological views which might be current with regard to this strange malady. It had been proved by experiments with brucines and strychnine that symptoms resembling tetanus might be produced, but as far as he knew the peculiar symptoms of catalepsy could not be so obtained.

At the request of the President, *Professor Macdougall*, while quite unprepared, as he said, for the unexpected call, made a few observations. He had listened with great pleasure to the very interesting case which had just

been read to the society, and coincided entirely with the remarks made by Dr Coldstream. He had paid some attention to the subject, and, along with Dr C., regretted to find that there was not a sufficiently minute and extensive record of these most remarkable cases to warrant any very confident and distinct conclusions. What struck him as particularly remarkable in the present case was the resistance, evidently dependant on mental apprehension, manifested by the patient to the introduction of food into the mouth. The very slightest touch of the spoon, it appeared, was sufficient to excite violent muscular motion, and proved clearly enough the existence of sensation in and around the lips; while yet, as Dr Coldstream stated, flies might traverse the lips and face in all directions without seemingly giving any annoyance, or, at any rate, eliciting any movement. Now, this seemed very analogous to certain other phenomena of more ordinary occurrence, and referable to a general law of consciousness, to which he would advert immediately. He could grope after an explanation of the anomalous circumstance in this state by supposing it most likely to be furnished by the general and striking fact that the mind may be perfectly alive and sensitive for an instant, and an instant only, to any impression *ab extra*; while yet a patient may possibly acquire by habit and settled determination of will the power of so instantly and entirely withdrawing attention as to be unable, almost the next moment perhaps, to remember that any sensation had been felt at all. The present case, however, was evidently very complicated. We had sensibility to bodily impressions, and, at the same time, such a power of mental abstraction, or some other condition of equivalent effect, that these impressions ceased to be followed by their usual physical consequences, which he was strongly inclined to suppose referable, in part at least, to a transient exercise of voluntary power. The law to which he had already referred was that of latent mental modifications. Leibnitz and others held that changes were continually going on in the mind beneath the level of consciousness, and which only in mass, or in their results, came up above the surface, so to speak, and revealed themselves to the mind's own observation. The doctrine in this form was not certainly without its difficulties; but none such appeared to attach to the very simple view which had been stated a moment ago. And just as we found in the case of the external senses, that a certain strength and duration of physical impression was indispensable for the production of sensation, so with regard to all mental modifications a law not very dissimilar might well be conceived to hold, namely, that a certain strength and duration of each was necessary, not, indeed, in order to its being felt at all, and at the very instant, but in order to its being capable of being recalled by the memory and being reflected upon, or being *known* to have ever existed. In both cases alike, attention or voluntary concentration of mental regards was of power, within certain limits, to do the work of greater time and intensity, while voluntary and energetic abstraction of the attention had the opposite effect of proportionally reducing and weakening. He was disposed to believe that this law had much to do with the explanation of at least part of the apparent insensibility in this case to particular physical impressions. Of course, the difficulty was still unexplained of the total absence of even *reflex* action in the muscles of the lips and face from the irritation of flies. If such action was really and purely reflex alone, and in no degree sensational, he could only confess his ignorance both of the precise state of body indicated thereby, and of the mode in which wish or purpose on the part of the patient could operate to produce it. On the other hand, so far as there was any reason to believe the action dependant upon sensation, the considerations adverted to might be not unimportant towards furnishing the explanation.

The *President* did not think that the peculiar symptoms which had been alluded to were connected with mind at all. In his opinion, they were rather of a reflex character; they were very like a spinal action, and did not at all resemble a cerebral one. Similar observations had been made on persons suffering from tetanus. The cases on record of fetuses being born without brains,

and yet in whom the action of sucking was performed on the mouth being touched, as well as the experiments of Grainger and others, in his opinion, conclusively showed that such movements as had been observed in the case related by Dr C. were not necessarily cerebral. It was difficult however, he allowed, to separate such reflex acts from mental ones. He thought it probable that had experiments been instituted in this case while the patient was either asleep or under the influence of chloroform, it might have been determined satisfactorily to which of the classes the movements belonged. It might be asked, if the acts were reflex, why the contact of the fly did not excite them as well as the spoon, the only reason he could give was the greater minuteness of the impression.

CASES OF DISEASE OF THE HEART GIVING RISE TO SYSTOLIC MURMUR AT THE APEX, WITHOUT DEFORMITY OF THE MITRAL VALVE ; WITH REMARKS ON THE PROBABLE MECHANISM OF THE MURMUR IN SUCH CASES. BY W. T. GAIRDNER, M.D.

Dr G. communicated a number of details with reference to the occurrence of systolic murmurs at the apex of the heart in cases in which no valvular deformity existed, and where it was presumed that regurgitation through the auriculo-ventricular orifices must have been caused by dilatation of the ventricles rendering the valves incompetent. Dr G. remarked that cases of this kind had been recorded by various observers, but that in most of them the regurgitation was ascribed to a dilated condition of the orifice, and considered as a rather rare and exceptional condition. He believed it to be, on the contrary, one of the usual consequences of a certain amount of ventricular dilatation; but reserved for a future occasion the discussion of the mechanism of the auriculo-ventricular valves on which it depended. The views of Lænnec, Hope, Stokes, and other authors, as to the systolic apex-murmurs were adverted to, and the importance of the entire subject with regard to cardiac pathology, diagnosis, and treatment was pointed out.

The *President* suggested that the murmurs which Dr Gairdner brought under the notice of the society might be due to vibrations in the blood in its passage through the auricle.

After a few remarks from Drs W. Begbie and Gairdner, the society adjourned.

MR BLACKLOCK ON THE CHOLERA IN DUMFRIES.

(*To the Editor of the Monthly Journal of Medical Science.*)

SIR,—In reply to Dr Grieve's letter to you of the 14th ult., stating that he had perused with much interest my observations on the origin of cholera in Dumfries, and regretting that I had confined my remarks to the origin of cholera only, and had not favoured you with my sentiments on the nature and treatment of it, I beg leave to say that the sole object I had in view when I addressed you on the 13th Feb. last, was to rectify the mistake which Dr Grieve unfortunately committed when he declared that the next or early cases of cholera, did not appear here in the autumn of 1832, in the same locality in which the first case appeared. This being quite at variance not only with the records of our local Board of Health, but also with my own private notes and memoranda, and distinct remembrance of almost every circumstance of any importance connected with the outbreak of the disease on that occasion, I considered it my duty to endeavour, with as little delay as possible, to remove all misunderstanding on the subject, more especially as I was anxious, and always have been, that our statistics of cholera should be carefully preserved, and that nothing hastily or inconsiderately asserted, should be permitted to remain uncontradicted, if it have the slightest tendency to mar or defeat the great object we must all have in view, the prevention of the pestilence. Dr Grieve, however, I am glad to find, now admits that the *first five or six cases of cholera did occur in English Street*. The remarks in my former letter were chiefly confined to the progress of the cholera during the first seven or eight days of the epidemic here in 1832; and I still adhere to what I then stated, with the trifling

exception that the case mentioned by me as having occurred in the Globe Inn Close, did not occur there but in Herries' Close ; although this, to say the most of it, is a distinction without a difference, for these Closes run parallel to, and communicate with each other ; Herries' Close being a few paces nearer English Street than the Globe Inn Close. Dr Grieve says that Callan, the patient, lodged in Queensberry Street, and was known to be labouring under choleraic diarrhoea, when he went thither in the morning. This last circumstance is not unlikely, for the most of the attacks of cholera at that time were preceded by diarrhoea, although the man told me, in the presence of Dr Charles Bell, after being removed to the cholera hospital, that he was quite well when he left home in the morning ; and on being questioned as to what he had been eating and drinking, he replied that he had eaten little or nothing, but that he had taken, as he was in the habit of doing, one glass of whisky on his way to the workshop. That part of Queensberry Street, however, in which Callan lodged, communicates by three filthy lanes (in one of which his abode was situated) with Flesh Market Street, near to where it terminates in that part of English Street, in which the first five cases of cholera appeared, so that his place of abode was really much nearer the origin of the disease than either the Globe Inn Close or Herries' Close. But, as Dr Grieve thinks that my information cannot be implicitly relied upon, as it is derived from the minutes of our local Boards of Health in 1832 and 1848, I must take this opportunity to state, that in 1832, the sederunt or minute-book of the local board of health, of which the late Mr M'Lauchlan and myself were the medical members, was carefully kept by our active and zealous secretary, Mr Gemmill, and few or no mistakes of any importance were committed. Indeed, during the *first week* of our visitation of cholera in 1832, to which my former letter referred, I am perfectly satisfied that the only case not recorded was Barnaghan's, for at that time, there was neither excitement nor panic in this place, as alleged by Dr Grieve, and the number of resident medical practitioners was more than sufficient for the calls that were made upon them.¹ There were thirteen of us, all of whom, with the exception of the late Dr Maxwell, who was far advanced in life, were both willing and able to take charge of any case of cholera that might appear ; and, as not more than nine cases did appear in the course of the first week, I think you will agree with me that, up to the end of the seventh day, none of us were over worked. Mr M'Lauchlan and myself, being members of the Board, were almost constantly acting together at that time, acquiring information, and making the necessary arrangements for the treatment of the sick poor ; and I am convinced that no case of cholera occurred during the first week of the epidemic, without our being apprized of it. It was not until nearly the end of the second week of the outbreak of the disease that the inhabitants became alarmed. On Wednesday, 26th September, one of our great annual fairs was held, and although the influx of strangers was not so large as on former occasions, yet there was a considerable attendance of people from the surrounding country ; and during that night and the following day a very sudden and unexpected increase of attacks took place ; and then, indeed, but not till then, we were running to and fro, and supplicating aid from a distance. As to the list of cholera patients kept by the Parochial Board here during the epidemic of 1848—for at that time we had no local Board of Health—I perfectly agree with Dr Grieve that it is a document of little or no value ; and, for that reason, I refrained from making any further use of it than to copy the dates and locality of the first two cases of cholera which appeared on that occasion. But, when he states that no consistent plan of superintendence was organised until the arrival of Dr Sutherland, the Government inspector, he is altogether wrong ;

¹ Since writing this letter Mr Gemmill has kindly shown me that Barnaghan's case, and also Robert Hume's recovery, referred to by Dr Grieve, although not mentioned in the minute-book, are both fully recorded in the Board's list of cholera attacks, so that there is every reason to believe that no mistake whatever occurred during the first week.

and to satisfy you as to this, I beg to call your attention to the enclosed printed copy of a letter, which was addressed to Dr Sutherland immediately on his arriving here, to which Dr Grieve's own name, and the names of six other resident medical practitioners, are affixed. By that letter you will perceive that this town had not only been divided into nine districts, previous to the arrival of Dr Sutherland, but that Dr Grieve himself took charge of one of them from the 24th till the 30th November, when the whole arrangement was cancelled by the sapient Board. Dr Sutherland, however, so soon as he was made acquainted with our previous arrangements, and the conduct of the Board, and with my plan of house to house visitation in each district, by qualified medical practitioners, known to the people, instead of by laymen, or medical students, as had been attempted in some places—for the purpose of ferretting out, and treating cases of diarrhoea, and of cholera in the earliest stages—he lost no time in intimating to the Board, that “the state of the town was most alarming, and that, if the original arrangement had been adhered to with the medical men, he had no doubt, from the skill with which their duty had been performed, that the disease would have been manageable,” and added, “that he thought it requisite that the original division of the town into nine districts should be resorted to; and that a medical man of experience in cholera, and, if possible, acquainted with the localities, should be appointed to each, with an assistant.” Of course, whatever Dr Sutherland *thought requisite* was a law to the Board; for they were well aware that he had power to enforce what he advised; consequently, all was speedily again in efficient order, and the result was most gratifying. I have not been thus prolix merely for the purpose of convincing Dr Grieve, or any other person who may doubt the fact, that not more than nine cases of cholera appeared in this town during the first week of the fearful epidemic in 1832; for it certainly rests with those who entertain any doubt on the subject, to prove the opposite, by mentioning the names and abodes of the persons attacked; nor to prevent any one from being led to believe that no arrangement was made previous to the arrival of Dr Sutherland, for the treatment of the sick in 1848; but rather as a preparatory step to some observations on the origin and prevention of cholera in this country, which I may, perhaps, at no distant period, humbly submit to the profession.

Dr Grieve, like many others, strives to maintain the generally received opinion—which was also for many years my own opinion—that the malignant, or Asiatic cholera, is not, under any circumstances, contagious, or in any way communicable from person to person; for, when he tells us that during both our epidemic visitations some of the inhabitants of this town, who fled to Annan with the disease upon them, did not communicate it to any person there, he strangely forgets to mention what happened at Clarencefield; a fine, clean, well-aired, healthy village, delightfully situated, about nine miles from Dumfries, on the old Annan road; that while the cholera prevailed here in the beginning of winter, 1848, a postman, or runner, as he was usually called, who resided in that village, and visited Dumfries three times a-week, was seized by the disease, and sent for Mr Smith, a medical gentleman in that neighbourhood, who obeyed the summons, and carefully attended him until life was extinct. A woman, who acted as nurse on the occasion, and afterwards washed the postman's clothes, was then attacked, and soon died. Mr Smith also attended her; and she was the last patient of any kind that he ever visited, for previous to leaving her he was himself attacked with cholera, and had to be conveyed home; and so rapidly did the disease run its course, that in three hours from the time he left his dying patient at Clarencefield, he was himself a corpse. The nurse had not been near Dumfries, nor any other infected place; and Mr Smith had not been within six miles of us. He had shortly before, however, attended a fisherman at Caerlaverock, six miles from this town, and likewise a man about a mile from Clarencefield, both of whom were attacked with cholera, and died, soon after returning from Dumfries.

Now, it may be asked, were Mr Smith and the nurse infected by the cholera-poison issuing from the postman's clothes, after they had been exposed upon his person to a pure uninfected atmosphere, at least three hours, while he walked from Dumfries to Clarencefield? or were Mr Smith and the nurse infected by a poison generated in, and emitted from, the postman's body? If they were infected in the first way, no person, it is plain, should be permitted to come from an infected locality, unless he come in as naked a state as he came into the world! Medical men, and all others, who visit cholera patients in the infected localities, should likewise be obliged to change their clothes before visiting patients of any other description, in healthy localities. Notwithstanding, however, this Clarencefield event, and many similar occurrences, I am well aware that some more powerful influence than human emanations is concerned in the propagation of cholera; but what that influence, or primary cause is, we have yet to learn. We should not shut our eyes, however, to the well-established fact, that several diseases spread both by an epidemic influence and by human infection, and are often introduced in the latter way to localities where an epidemic influence does not exist. What reason, therefore, have we to suppose that the Asiatic cholera cannot also be communicated, or conveyed, in both ways? Indeed, the many well-authenticated facts which have been published since the first outbreak of cholera in the United Kingdom, place this beyond a doubt.¹ And who can now pretend to say, when cholera prevails in a populous district, how much the spread of the disease is caused by emanations from the sick, and how much by the epidemic influence; or how far a combination of both hurries on the destructive march of the pestilence? Opinions, it has been well observed, are of two kinds, those founded on facts and reasoning; and those hazarded without a knowledge of either; and that rational opinions must undergo modifications with the facts from which they flow.—I am, etc. etc.

ARCHD. BLACKLOCK.

CANCEROUS ULCER OF THE THIGH.

(To the Editor of the *Monthly Journal of Medical Science*.)

SIR,—My attention has just been called to a notice in the British and Foreign Quarterly Review for January 1854, page 281, in which a case is quoted from the *Monthly Journal* of Nov. 1853, p. 413, which is described "as a very satisfactory amputation of the thigh" for "a malignant ulcer," and it goes on to state, that with the exception of a slight tendency of the posterior flap to retract, reported on the seventh day, nothing occurred to interrupt the cure, which was *complete* in about nine weeks.

As the patient has since died from a return of malignant disease in the stump, I am induced to give the sequel of the case to correct what would otherwise prove a statistical error, should this case find its way into a table of successful operations for the cure of malignant disease.

I saw the patient a few days after his return from Scotland, when I found the wound healed with the exception of a small surface about the size of a four-penny piece in the line of the union of the flaps. This wound gradually increased, considerable tumefaction of the stump followed, and he complained of great pain. There was a very considerable amount of purulent discharge from the wound, and in a week or two the ulcer had increased to about half its original size, and the ulcer put on precisely the same characteristic appearance as the original disease.

The discharge continued to increase, and the patient gradually sunk, and

¹ I beg to refer you to a letter from the late amiable and highly-gifted Mr Moir of Musselburgh, dated March 2, 1849, and addressed to my respected friend, Mr Scot of Craigmuir, late Secretary of the Medical Board of Madras, contained in the Introductory Remarks to the abridged edition of his Report on the Epidemic Cholera, as it appeared in the territories subject to the Presidency of Fort St George, drawn up by order of Government, under the superintendence of the Medical Board. Published by Blackwood and Sons, Edinburgh, and Murray, London. 1849.

ultimately died on the 24th January 1854, just six months after the operation. For a short time it was doubtful whether the pain, tumefaction, and discharge might not have resulted from the irritation of carious bone in the stump, but afterwards the appearance and characters of the sore left no doubt on my mind that it was a return of the original disease. The patient was under my care for some months previous to his visit to Edinburgh, and was seen by Mr Bickerteth and his son. We did not advise amputation. Indeed I scarcely think the idea occurred to any of us. It certainly was not mentioned in consultation, and the result of the case has not been such as to make me regret the view we took ; at the same time, if the patient chose to take the alternative, it was, perhaps, just worth while to give him the chance. As it has turned out, I question very much whether life was at all prolonged, or his sufferings diminished, by the temporary removal of the disease, for he did not, while under my care, suffer very much from pain, nor did the ulceration proceed rapidly. I was unable to obtain permission to examine the parts after death, but I suspect the interior of the stump was extensively occupied by malignant disease.—Your obedient Servant,

A. B. STEELE, M.R.C.S.

LIVERPOOL, April 17, 1854.

MORE ENGLISH LAW.—THE FORGED DIPLOMA CASE.

THE extraordinary decisions which the English courts of law so frequently present us with, when the subject of them in any way relates to the medical profession, is one of the greatest anomalies of the age. In most civilised nations a medical practitioner is obliged to hold a diploma, which indicates that he has been educated for the profession, or that his competency has been tested by an examination, and he who falsely assumes the title of doctor or surgeon is visited with summary punishment. Again, in most codes of law, culpable homicide is a crime ; but what is culpable homicide, if it be not destroying life by administering powerful drugs, when unacquainted with their properties ? Yet such is the absurdity of English law, that it has been proved over and over again, that if, for the purpose of gain, a deadly poison be so administered as to cause death, and the individual prescribing it plead ignorance of medicine, both judge and jury consider there is no harm done. But if a regularly educated medical practitioner commit a mistake, heavy damages are awarded, and should malpraxis be followed by death, it is with the utmost difficulty he can escape the penalty of manslaughter. The result to be deduced from this state of things is obviously that it is safer in the eye of English law to be ignorant than skilful, to impose on the public than serve them faithfully, to practise the grossest quackery than exercise one's profession honourably. Need we be surprised that Charlatanism under such circumstances should flourish, and that the public should at length look with indulgence on crimes which are not only not repressed, but are positively encouraged by the State.

Another singular illustration of this state of things has lately occurred. An individual named Lewis Bossy represents himself as an M.D. of the University of Edinburgh, and hangs up a forged diploma in his surgery, to impose upon all and sundry. In this capacity, among other things, he does not hesitate to sign schedules of individuals applying to insurance companies, whose lives are insured on the faith of his representations. The validity of these insurances is, we believe, to be made the subject of another trial, and the mischief he may have occasioned in this way is yet to be ascertained. But this impostor, on the 8th ultimo, was tried at the Central Criminal Court in London for a misdemeanour, he "having uttered as true a false diploma of Edinburgh, entitling him to practise and assume the title of 'Doctor of Medicine.'" The offence was distinctly proved ; and "the jury, after a little hesitation, returned a verdict of *Guilty*, but with a strong recommendation to the merciful consideration of the Court" ! Whereupon the Judge (Mr Baron Martin) gives utterance to

the following sage and indisputable remarks :—" It was undoubtedly an offence to utter a diploma like this, and persons should understand that if such a document was hung up in their houses where patients might see it, it was an uttering in law, and rendered them liable to the consequences. He could conceive cases where very great mischief might arise from an individual holding himself out falsely to be a physician. No man ought to trade upon a false bottom ; and, undoubtedly, the fact of a person professing to have a diploma from the University of Edinburgh, where it was known that a very severe examination had to be undergone, was calculated to give a man a position, and create a confidence, which no man ought to possess unless he was really entitled to it."

The learned Judge is then reported to have passed sentence upon this man who he could conceive might, by his gross fraud, commit great mischief, in the following terms :—" It was his intention on the present occasion merely to pass a nominal sentence, which was one of two days' imprisonment ; and the defendant under that sentence would be entitled to his immediate discharge ; but he hoped that the present proceeding would have the effect of putting a stop to such practices in future."

Whether such a heavy sentence will produce the effect the learned Judge anticipates may be much doubted ; for, were not the whole proceeding a melancholy truth, it might well be regarded as a preposterous burlesque.

In another case, where a man placed the letters M.R.C.S. after his name, without a diploma, he defended himself by saying that he was a Member of the Reformed College of Surgeons, of New York !

MEDICAL REFORM. THE ASSOCIATION BILL.

THE time is now approaching when every medical man who truly loves his profession, ought seriously to consider the question of medical reform. On the 10th ultimo, Lord Palmerston said, in the House of Commons, that he had received so many complaints and objections against Mr Brady's bill, that he should consider it to be his duty to resist its further progress. He added, that the question of registration would naturally form part of a general measure, and that he had under consideration the bill of the Provincial Medical Association. This, then, is the only measure which we are now called upon to criticise, and we trust it has already been conclusively shown, that for one of the great ends for which a medical reform is required, namely, equality of privilege throughout the kingdom, it is worse than useless. A conference, we are informed, was held by delegates from all the medical bodies on Tuesday the 25th ultimo, in the hall of the London College of Physicians, the result of which had not reached us on going to press.

In addition to the utter absence of provisions for securing equality of privilege throughout the kingdom, which must secure the failure of the Association Bill as we now have it before us, it is further objectionable from the unnecessary complexity of its plan. There are to be three councils, an English, Scotch, and Irish one. Why not one council, if uniformity be sincerely desired ? And if the idea of centralization in the metropolis be objected to, let such council meet in London, Edinburgh, and Dublin in turn. Great care should be taken that the council be sufficiently large, and formed on such a liberal basis as to place it above the suspicion of interested views. According to this Association Bill, each council is to appoint a board of examiners, to license for practice, the result of which will be, that such licence will be considered, as a matter of course, the truly essential one. Indeed, this is also secured by the bill, as no one is to be allowed to register without it, and in consequence, all the present boards will be rendered useless, and many institutions ruined. Yet there are no provisions for securing, that an examining board, so mischievous in its effects, will be in any degree superior to those which already exist.

We hope, however, that the Association Bill may be considerably changed as a result of the conference, and we forbear from making further comments on the matter at present. But should a modified bill reach us sufficiently

early, we shall dedicate an article to an exposition of the political and social state of the profession in our next number.

PROFESSOR JAMESON.

THIS distinguished individual expired on the 19th ultimo, aged 81 years. He occupied the Chair of Natural History in this University for half a century, and may be called the father of modern natural history in Scotland. The science had flourished under Sir Robert Sibbald and his contemporaries, but had sunk to the lowest ebb when it received new vigour from the labours of Mr Jameson. The magnificent museum which now fills our halls and galleries to overflowing, is mainly indebted to him for its existence and present excellence. As a teacher, he especially possessed the art of exciting enthusiasm in his pupils; and those who attended his practical demonstrations and excursions in the country, will not easily forget the interest and indelible impression they produced on the mind. As a writer and journalist, he may be regarded as the principal means by which the mineralogy and geology of Scotland have been made known during the last half century.

We have found it impossible, however, to do justice to the life, character, and scientific eminence of Professor Jameson in the short space which has intervened between his death and the period of our going to press. We propose giving a biographical sketch of him in our next number, and shall feel obliged to any of our readers who can furnish us with available facts and information for its compilation.

VARIETIES.

WAR AND PESTILENCE.—Some interesting tables have issued from the Health Office, comparing the loss of life by war and pestilence. It appears that in twenty-two years of war there were 19,796 killed, and 79,709 wounded, giving an annual average of 899 killed and 3623 wounded. In 1848-49, there were no fewer than 72,180 persons killed by cholera and diarrhoea in England and Wales, and 144,360 attacked; 34,397 of the killed were able-bodied persons capable of getting their own living! Besides these deaths from the great epidemic, 115,000 die annually, on an average, of preventible diseases; while 11,419 die by violence. Comparing the killed in nine great battles, including Waterloo—4740—with the number killed by cholera in London in 1848-49—14,139—we find the difference of 9399 in favour of war. In cholera visitations, 12 per cent., sometimes 20 per cent. of the medical men employed, died. The London missionaries die as fast as those in foreign countries, and there are some districts in London which make the Mission Society ask themselves whether they have a right to send men into them. From the returns of twelve Unions it is found that 3567 widows and orphans are chargeable to the cholera of 1848-49, entailing an expenditure of L.121,000 in four years only.—*Edinburgh Advertiser*.

THE WAR AGAINST THE LANCET.—The Council of the Medico-Chirurgical Society has decided on retaining this Journal, although a memorial in its favour was supported by only 104 members, whilst the opposition one had appended to it 155 names. Hence the *Lancet* has gained the first battle with inferior numbers, but tarnished its victory by immediately afterwards proceeding to slaughter the character of the enemy's General. This has given rise to loud murmurs, and greatly exasperated the rebel army. The conspirators have lately been reinforced by the Reading Pathological Society, and are actively engaged in re-organizing their ranks with a view to another battle. They loudly assert that the advantage in the last action was obtained more by stratagem and false pretences than by fair fighting. The *Lancet*, on the other hand, strongly entrenched among the Council, and ably supported by its powerful allies, President Copland and Sir Benjamin Brodie, makes from time to time murderous sallies into the hostile camp. Both parties feel confident of ultimate success, and we look with the most intense anxiety for the next intelligence from the seat of war.

DEVELOPMENT OF TAPEWORMS.—M. Von Bunsen has written to the *Académie des Sciences*, that a pig who swallowed at the end of last October some eggs of the *Tænia solium*, passed by a woman, was lately killed, and a large number of cysticerci were found in its muscles.—*Gazette des Hosp.*, 13 *Avril*. This may have been a coincidence, as it is not mentioned that the muscles of the animal had been examined before the commencement of the experiment.

THE MEDICAL COMMISSION AT BUCHAREST.—Dr Cuillat informs us that at Bucharest there is a medical commission charged to examine the diplomas and medical knowledge of all the foreign medical men who come to practise there. He himself, notwithstanding his title of doctor, and certificate as an interne of the French hospitals, was conducted to the Coltza hospital, and requested to examine two patients, which having done, and satisfactorily determined their diseases, indicated the appropriate treatment, etc., he was authorised to practice. He attributes this unaccustomed favour to the advantages he received as an interne, which enabled him to acquit himself well at the bed side. Generally speaking, the holders of French and English diplomas are remitted by this commission for another six months, on account of insufficient knowledge of medical practice.—*Gazette des Hopitaux*. From this account, it would appear that medical examinations are much better performed in Wallachia than they are among us.

ALLEGED CRUELTY TO THE WOUNDED IN NAVAL WARFARE.—In the article on "Wounds" in the *Encyclopædia Britannica*, 4th edition, published 1810, the author, after mentioning the necessity of a surgeon being cautious in pronouncing on the character of any wound, adds that "this is particularly necessary on board ship, where, as soon as any man is pronounced by the surgeon to be mortally wounded, he is forthwith, while still living and conscious, thrown overboard;" or words to that effect. This statement is untrue; for Smollett and other writers, who have exposed with unsparing hand all the defects in the naval system of their day, would have scarcely left this unnoticed, when they attack much slighter abuses. If such a thing ever occurred, even in the worst of times, it must have been an isolated case.—*Notes and Queries*.

SHAVING.—It is in vain to urge that shaving is a nuisance. Every one admits the fact, but no one is ready to forego the infliction. The dread of being thought odd, peculiar, and eccentric, is too much even for the wisest of men to overcome. A learned lady once deplored the misfortune of her sex, and the heavy weight of that primæval curse which declared that "in sorrow woman should bring forth children." "Ah, madam!" said a still more learned person in reply, "beware your fate no more: the evil you lament is not of frequent occurrence, and does not afflict old age. Think of man's misery, who has to shave every day: you may then decide whose afflictions are the greatest." "But man's are self-imposed," said the lady; and the philosopher was silenced.—*Illustrated London News*.

DIFFICULTY OF INTRODUCING PURE AIR INTO THE HOUSE OF COMMONS.—On the 10th ultimo, Mr Spooner made some startling statements to the House of Commons concerning ventilation. It would seem that the nation in its anxiety to preserve the lungs of the 650 gentlemen who legislate there, has paid no less a sum than £250,000. And yet all the members concur in saying that this costly attempt at ventilation is a perfect failure. The quarter of a million has been literally scattered to the four winds of heaven, for not one of them can be induced to cross the threshold of the House.—*Glasgow Commonwealth*.

MONSIEUR ROUX.—This distinguished surgeon died on the 24th of last March of apoplexy, at the age of 73. He entered the army when 15 years old, in 1797, as a surgeon of the third class, and served two years. He then studied in Paris, and in 1802 succeeded Bichat as teacher of anatomy. For the last 30 years, he has been regarded as one of the first practitioners in Paris, and on the death of Dupuytren became the senior surgeon of the Hotel Dieu.

CHAIR OF NATURAL HISTORY.—The Town Council of Edinburgh have unanimously memorialised the crown to present Professor Edward Forbes to the chair, vacant by the death of Professor Jamieson. This course, though not unprecedented, is very unusual, and we trust will have due influence with the government.

MEDICAL ORGANIZATION IN ALGERIA.—The French government has divided all the colonised territories of Algeria into departments, to each of which a medical man is appointed, under the name of *Médecin de Colonisation*. His salary is 2000 francs (L.80) per annum, and if required to be mounted he receives 500 francs (L.20) in addition.—*Journal de Méd.*

RADICAL CURE OF INGUINAL HERNIA.—On the 28th of last March M. Jobert (de Lamballe) presented a patient to the Academy of Medicine in whom he had produced a radical cure of inguinal hernia, by puncturing the sac and injecting iodine, which, by occasioning adhesive inflammation, had caused permanent obliteration of the sac.—*Gazette des Hopitaux*.

ELECTION OF M. CLAUDE BERNARD AS PROFESSOR OF GENERAL PHYSIOLOGY.—A new chair of General Physiology has been created in the Faculty of Sciences at Paris, to which M. Claude Bernard has been appointed. The course will extend over two years, and be given only during the summer session at the Sorbonne.

MEDICAL MEMBERS OF THE LEGION OF HONOUR.—The members of the Legion of Honour amount in all to 52,709. Of these there are 1850 medical men, thus divided:—Military, 867; naval, 233; civil, 750; military and naval apothecaries, 109; civil apothecaries, 29.—*Gaz. des Hop.*

PROFESSOR SKODA has presented to the charitable institution for supporting students when unwell the sum received for the fifth edition of his work on auscultation and percussion—namely, 3000 florins.—*Allgemeine Medic. Centr. Zeit.*

PUBLICATIONS RECEIVED.

A Treatise on Acute and Chronic Diseases of the Neck of the Uterus. Illustrated with numerous plates, coloured and plain. By Charles D. Meigs., M.D., Professor of Midwifery, and the Diseases of Women and Children in Jefferson Medical College, etc. Philadelphia and London. 8vo. 1854. Pp. 116.

An Inquiry into the Pathological Importance of Ulceration of the Os Uteri, being the Croonian Lectures for the year 1854. By Charles West, M.D., Physician-Accoucheur to St Bartholomew's Hospital, etc. London. 8vo. 1854. Pp. 95.

On the Use of an Artificial Membrana Tympani in Cases of Deafness, dependant upon Perforation or Destruction of the Natural Organ. By Joseph Toyubee, F.R.S., Aural Surgeon to St Mary's Hospital, etc. Second edition. London. 1854. 8vo. Pp. 32.

Cyclopædia of Chemistry, with its Applications to Mineralogy, Physiology, and the Arts. By Robert Dundas Thomson, M.D., Professor of Chemistry in St Thomas' College, London, etc. With numerous Illustrations. London and Glasgow. 1854. 8vo.

Dissertatio anatomica inauguralis de cerebri et medullæ spinalis systemate vasorum capillari in statu sano et morbo, etc., eruditorum examini submittit Everardus Henricus Ekker Rheno-Trajectinus. Trajecti ad Rhenum. 1853. 8vo. Pp. 120.

Over de vorming en verspreiding van kankercellen in den omtrek van kanker en het gewigt hiervan bij het doen eener operatie. Door J. L. C. Schroeder van der Kolk. Utrecht. 8vo. Pp. 30.

Archives de Physiologie de Therapeutique et d'hygiène sous la direction de M. Bouchardat, Professor d'hygiène à la Faculté de Médecine de Paris. No. I. Janvier 1854. Mémoire sur la Digitaline et la Digitale. Par E. Homolle et T. A. Queveune. Paris. 1854. 8vo. Pp. 376.

Notes on the Pathology and Treatment of Cholera. By John Rose Cormack, M.D., Edinburgh, etc. London. 1854. 12mo. Pp. 67.

A Letter to the Right Hon. the Earl of Granville on the importance of rational Gymnastics, as a branch of National Education, etc. By M. Roth, M.D. London. 1854. 8vo. Pp. 39.

The papers of Drs Peddie, George Paterson, Lindsay, Skæe, and Alexander, will be inserted as early as possible.

We have been under the necessity of postponing the conclusion of Stokes on the Heart, with numerous other Reviews, and much Periscopic matter.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Pathology of Delirium Tremens, and its Treatment without Stimuli or Opiates.* By ALEX. PEDDIE, M.D., F.R.C.P., Edinburgh, etc.

BEFORE attempting to explain the following views of the pathology and treatment of delirium tremens, I think it necessary to state what I consider to be the symptoms of the disease in its genuine and uncomplicated form; for it appears to me that the opinions generally received and acted on in regard to this affection are erroneous, and have resulted from a loose and partial observation of its phenomena, and inattention to the history of individual cases. In consequence of this, the disease is frequently confounded with very dissimilar affections, or its usual features lost sight of under the effects of injurious treatment, and thus its true nature misunderstood.

The most characteristic symptoms of delirium tremens are general muscular tremors—more especially of the hands, and of the tongue when protruded—along with complete sleeplessness, and delirium of a muttering, sight-seeing, bustling, abrupt, anxious, apprehensive kind. The affected has not ability to follow out a train of thought, to explain fully an illusion or perverted sensation, or to perform any act correctly; for he may be one moment rational and the next incoherent; now conscious of his real condition and of surrounding realities, and then again suddenly excited by the most ridiculous fancies—principally of a spectral kind—such as strange visitors in the shape of human beings, devils, cats, rats, snakes, etc.; or by alarming occurrences, such as robberies, fires, pursuit for crimes, and the like. He is easily pleased and satisfied by gentleness and indulgence, and much fretted and agitated by restraint and opposition. The face is generally of a pale dirty colour, and wearing an anxious expression; eyes startled but lustreless, sometimes considerably suffused, and the pupils not contracted unless considerable doses of opium have been administered, or very decided arachnitic symptoms have supervened; skin warm and moist, often perspiring copiously; tongue sometimes loaded but generally pale and moist, occasionally remarkably clean; appetite small, but the patient will

often take whatever is presented to him ; thirst by no means urgent, and seldom or never any craving for spirituous liquors ; urine scanty and high coloured, and in some cases which I have tested, containing a large quantity of albumen, which, however, disappears immediately after the paroxysm is over ;¹ alvine evacuations bilious and offensive ; and the pulse usually ranges from 90 to 120, generally soft, but of various degrees of fulness and smallness, according to the strength of the patient and the stage of the affection. The precursory symptoms are by no means peculiar or pathognomonic, but common to many febrile affections implicating the sensorium in the way of repeatedly disturbed and sleepless nights, with, perhaps, more of a hurried and agitated manner than usual for some days previously. The paroxysm, which is distinguished by the phenomena above described—occurring with remarkable uniformity, independently of age and constitution,²—usually runs its course, if uncomplicated and properly treated, on the second or third day, though sometimes earlier, and it seldom extends beyond the fifth day. It then terminates in a profound natural sleep, which may continue for many hours, and from which, if it even lasts for six hours, the patient awakes weak and languid, but quite coherent. The casualties of the disease are convulsions or coma, which, if not immediately fatal, is apt to leave the unfortunate sufferer a wreck for the remainder of his life.

The paroxysmal phenomena may occur in whole or in part in the progress of pneumonia, bronchitis, fever, erysipelas, and other diseases affecting the habitual drunkard, or after he has received a personal injury which occasions a severe shock to the system. I need scarcely add, that delirium tremens thus complicated, is frequently fatal under any mode of treatment ; and the remarks I am about to offer are not meant to apply to such instances. I may also observe, that the foregoing account of the phenomena of the paroxysm is descriptive

¹ Probably albumen is always present. In the thirtieth volume of the Transactions of the Medico-Chirurgical Society of London, Dr Bence Jones has endeavoured to prove, from three cases of this disease, compared with three of phrenitis, that the phosphates are found in much smaller proportion in the urine of the former than in the latter, and that thus a new diagnostic mark is presented of the nervous and non-inflammatory nature of delirium tremens. His observations, however, are not sufficiently numerous to be conclusive, and there are many probable sources of error. In the last instance of the disease which I have treated (see note to Case VI.), I allowed a drop of urine to evaporate spontaneously, and found numerous beautiful phosphatic stellæ present ; and I have no doubt that these will often be found in abundance in the most genuine cases of the affection.

² The age at which delirium tremens most generally occurs, seems to be between 40 and 50 ; the constitution is the irritable, excitable, and somewhat weakly ; and what is interesting, it is very uncommon in the female, although from what cause I cannot explain, for the number affected does not bear any relative proportion to the number of dram-drinkers. In all my experience I have never seen a single case of the disease in the female sex. Rayer observed it in 7 women out of 176 cases ; Bang in 10 out of 456 cases ; and Dr Hoegh-Guldberg of Copenhagen (*Commentatio de Delirio Tremens*), in 1 out of 173 cases.

of the disease when running its ordinary course without being interfered with or obscured by the action of stimuli, opiates, or other treatment.

There is a form of *mania* which is sometimes mistaken for delirium tremens, but which must not be confounded with it, although characterised by very considerable muscular tremor. It is nothing more nor less than a severe and protracted form of intoxication,—an affection of the brain and membranes, in which there is great vascular excitement, resulting from the direct or immediate action of alcoholic liquors. A physician even so careful and discriminating as Dr Watson, has noticed two cases¹ as instances of delirium tremens, the first of which partakes more, and the last entirely of the characters of the affection which I am now about to describe. It has been styled by Darwin the *Delirium Ebriosum*. It originates from a single fit of intoxication, or at least from a short course of intemperance (in vulgar phrase, “a boose,” or “a ramble”) engaged in by persons of a peculiar mental constitution and temperament, and which is most commonly induced by some depressing emotion. It is marked by an uncontrollable desire for more drink, which, when gratified, excites to further imperious demands, begetting indecorous conduct, and engendering passions so wild and vicious, that when the hereditary mental constitution is imperfect, and the previous moral habits loose or depraved, not unfrequently lead to the perpetration of violent and criminal acts. The other symptoms and circumstances characterising the paroxysm, are dry heat of skin, particularly of the scalp; general muscular tremors; flushed countenance; a sullen, determined, or fierce aspect; red, ferrety eyes—such as in the cases of Dr Watson already noticed;—dry tongue; strong, quick pulse; and loss of appetite for everything but liquor—and that of the strongest kind, although in some instances, beastly ravenousness for anything or everything eatable which comes in the way. This state may be brought on once in a lifetime, from some accidental circumstance leading to an act of intoxication; or it may be induced at particular periods—distant, perhaps, months or years, as in the case of those unfortunate individuals to whom the name of *dipsomaniacs*, or *oinomaniacs*, has been applied. All who have witnessed the various forms of disease affecting the drunkard, will readily distinguish genuine cases of delirium tremens from this and other affections attended with delirium, as its characters are so well marked.

The substance of the opinions generally held regarding the essential nature of delirium tremens may be stated to be—that it is a disease of exhaustion or irritation of nervous power, and that it has the habitual abuse of intoxicating liquors for its predisposing cause, and abstinence from, or the abstraction of, an accustomed stimulus for its exciting cause.

¹ Principles and Practice of Physic, vol. i. pp. 394, 395.

The first part of this definition, namely, that *delirium tremens* is a disease of exhaustion or irritation of nervous power, appears to me to be vague, and not easily explainable, either on the principles of physiology or pathology. Various authors,¹ in the most arbitrary manner, make use of the terms exhaustion and irritation, separately or together, as it seems to suit their purpose. . One can understand what the term nervous irritation, or what that of nervous exhaustion may mean, and also how the latter state may succeed to the former; but that these two conditions—so opposite in their nature, and standing more properly in the relation of cause and effect—should coexist in this disease, is not so easily comprehensible; and consequently I believe that the idea of exhaustion—as interpreted according to the common sense acceptation of the word, namely, weakness, has led to much error in the treatment of *delirium tremens*. The more that the history and phenomena of genuine cases of this malady are considered, the more numerous do the difficulties surrounding the above explanation become. It seems to be forgotten that the disease is not occasioned by a fit of drunkenness, but that it is the result of the long-continued excessive use of stimulants, and therefore on the ordinary and well understood physiological law that exhaustion succeeds excitement in an almost invariable ratio, this affection ought rather to follow the cessation of an out-and-out debauch, than from a course of systematic imbibition. The affection is, I believe, quite specific and peculiar. It is something more than simple “nervous irritation” or “nervous irritability:” it is, I consider, essentially a specific form of nervous poisoning, which, in every instance—whatever be the state of the constitution, or however combined or associated with other diseases—is distinguished by a very remarkable uniformity of phenomena. In every instance of *delirium tremens*, the stimulus or alcoholic principle, a powerful narcotico-acrid agent, in whatever way atomically combined or chemically changed after its introduction into the system, acts slowly on the nervous pulp through the medium of the circulation, poisons its substance and sets up at last what may be termed an alcoholic erythism, or, if I may be allowed the expression, an alcoholism.² This in turn, no doubt,

¹ Copland, *Dict. Pract. Med.*, p. 497. Watson, *Pract. of Physic*, vol. i. pp. 400-1. Carpenter, *On the use and abuse of alcoholic liquors in health and disease*, pp. 28, 29, 30; and others.

² Since writing the above, my attention has been drawn to the work of Dr Huss of Stockholm (see an able analysis in the *Brit. and For. Med. Rev.*, No. XIII., 1851), on what he calls “*Alcoholismus chronicus*,” or “the chronic alcohol disease.” *Delirium tremens*, however, is not recognised under this appellation; and does not appear to be viewed as an alcoholism at all by this author. The term is applied solely to a group of affections of the nervous system occurring in those long addicted to the abuse of spirituous liquors. These are distinguished by tremors and jerkings of the voluntary muscles, and diminished or increased sensibilities of surface to a greater or less extent; they are of gradual development; and terminate very frequently in paralysis, epilepsy, or idiocy, without any notable pathological alterations of structure. Such symptoms and results, variously modified by and combined with organic

produces a certain amount and kind of debility in the cerebral functions, but combined with over-action of the circulation through the membranes of the brain, constituting a decided form of irritation, the tendency of which is, if not allayed by judicious treatment, to inflammatory action, and serious encephalic mischief.¹ Scipio Pinel considered delirium tremens to be "a first degree of paralytic cerebritis ;"² Dr Abercrombie appears to have considered it as "a dangerous form of meningitis ;"³ Dr Bright actually includes it among his cases of "arachnitis ;"⁴ Dr Hoegh-Guldberg⁵ also views it as a

disease, are by no means uncommon in this country, but they appear to be much more frequent, and earlier developed in Sweden and the northern parts of Germany, owing, it is supposed, to the very pernicious composition of the alcoholic liquors in general use. "With but few exceptions the symptoms have been caused by the potato brandy, which is served out over the counter of the spirit shops to the lower classes of this metropolis. Spirit from grain is not common, and the distilled spirit freed from the volatile oil (*finkelölja*) does not suit the palated taste of the habitual spirit drinker. The presence or absence of this oil must be carefully borne in mind in estimating the causes of the disorder."—P. 34, *Dr Huss's work*. It further appears that these potatoes, skins and all, are generally diseased or decayed ; that mildewed grain is also used, and various poisonous vegetable products, such as spurred rye, *lolium* (frequently mixed with bad barley), and the seeds of the *raphania raphanistrum*—which latter was by Linnæus himself thought to be the cause of this disease ; and this poisonous mixture is likewise favoured by preparation in copper vessels.—*Review*, pp. 56, 7. While Dr Huss acknowledges that all this must greatly increase and confirm the maladies described by him, he regards the alcoholic principle as the chief cause. On the other hand, the reviewer expresses a doubt (p. 59) "whether the *alcoholismus chronicus* be really dependent even chiefly upon alcohol," and considers it probable "that its phenomena result from the habitual use of alcoholic drinks holding various (narcotic and acrid-narcotic) poisons in solution in different amounts, and differing in their nature and action, but all having in common, that they exercise a highly deleterious influence, especially on the *nervous system*." Besides the probability that the various maladies described by Dr Huss are not altogether owing to a condition of *alcoholism*, it must be borne in mind that there is a great want of uniformity in the character and course of the attending phenomena, that there is nothing in fact to point out a peculiar physiological action such as may be attributed solely to alcohol. It appears to me therefore that they must be regarded simply as bad effects from drinking habits on the cerebro-spinal system in particular, through the general impairment of the nutrition of the body ; and be placed on the same footing as those diseases of the heart, arteries, liver, kidneys, and other organs with their respective functions, also resulting from intemperance, and with which the so-called *alcoholismus chronicus* is more or less associated. The application of the term *alcoholism* to such affections as these, appears to me to be inappropriate—that given by Romberg (*Dis. of Nervous System*), namely, *tremor potatorum*, is preferable ; and in the following pages, I trust it will be apparent that if there is one disease more than another arising from habitual excess in alcoholic drinks, in which a *peculiar toxicological effect* is manifested, it is delirium tremens.

¹ Watson, *Prac. of Phys.*, pp. 400, 401.

² *Traité de Pathologie le Cérébrale*, par Scipion Pinel, p. 400.

³ *Diseases of the Brain and Spinal Cord*, p. 63.

⁴ *Medical Reports*, vol. ii. p. 10.

⁵ *Commentatio de delirio tremente*, quoted in *Brit. and For. Med. Review*, vol. vi. p. 328.

febrile affection, indeed a species of arachnitis. Such opinions, I think, are not only nearer the truth in regard to the nature of the disease than those generally received, but they obtain strength from its symptoms, the injurious effect of stimulating treatment, and the appearances observed in fatal cases; for while in the brain itself there is generally observed peculiar paleness of the tissue, shrunken convolutions, and enlarged sulci, the membranes present great vascular fulness, and the arachnoid is generally opaque, thickened, and presents more or less sub-serous effusion. To get rid of the difficulty some have spoken of delirium tremens as occurring in two forms—asthenic or sthenic, congestive or inflammatory; but such distinctions, while presenting a fine hair-splitting, diagnostic aspect, really serve a bad purpose, by originating perplexing doubts and difficulties. Like plumbism, mercurialism, ergotism, or narcotism, alcoholism is, I am of opinion, specific in its nature. Lead, mercury, and other agents, may affect individuals in different degrees from difference of age, constitution, continuance of exposure, etc.; or the effect—like that of the virus of small-pox—though the same in kind, may be modified in one case more than another. When alcoholic liquors have been long abused, the active principle affects the system, I think, like some other poisons, by accumulation. The experiments and observations of Dr Percy¹ prove that alcohol has a peculiar favour for cerebral matter, fixing at once on it, by a sort of elective affinity; and indeed in fatal cases from direct intoxication, its actual presence in the substance of the brain is demonstrated. Now, in the longer continued abuse of alcoholic liquors, is it probable that the selection of the agent will be different? Its accumulation may be slow, and the change in chemical constitution may be considerable, but it is not the less sure. Every additional drop imbibed brings the grey matter of the brain into that state which assists in the development of the alcoholic erythism, and thereafter occasions those relative changes in the sanguiferous system of the encephalon, the tendency of which is, as I have already affirmed, to pass from irritation, from abnormal activity of circulation and functions, to inflammatory action, according to the severity of the attack, and other circumstances. It therefore does appear strange that physicians while describing delirium tremens as a disease of exhausted or irritated nervous power caused by intemperance, should recommend and practise as a remedy the very agent which occasioned it, or another, namely, opium, which, although unable of itself to produce it—as I shall afterwards show—greatly assists and hastens the affection in those who habitually indulge in intoxicating liquors; and moreover an agent, the physiological action of which is to occasion engorgement of the vessels of the brain—vessels already too highly charged with blood containing a poisonous

¹ Experimental inquiry concerning the presence of alcohol in the ventricles of the brain.—*Prize Thesis*, 1839.

ingredient. This is truly acting in the spirit of the homœopathic dogma—"similia similibus curantur!" But of this more hereafter, for I am now brought to the consideration of the second position, which assumes in explanation of this affection, that while the abuse of intoxicating liquors is its predisposing, the abstraction of, or abstinence from an accustomed stimulus, is its exciting cause.

With the first part of this proposition I quite agree; but the opinion that the privation of a usual stimulus must be regarded as the exciting cause of the malady, I consider to be entirely erroneous. Analogy, certainly, will not bear out this theory. Mercurial fumes, or the oxides of mercury, when long inhaled or absorbed into the body, as in the case of gilders, quicksilver-miners, and others, in the course of time produce an attack of shaking paralysis—the *tremblement mercurial* of the French pathologists; but will it be averred that the workmen long exposed are more likely to be affected with tremors, if removed from this poisonous atmosphere and occupation, than if they continued at their work? The reverse is well known to be the fact, not only in the case of such artizans, but of those also who are beginning to suffer in a somewhat similar way from lead poisoning. In both affections, when the symptoms are recent, a cure can only be effected by removal from the injurious occupation; otherwise the symptoms deepen with hourly increasing rapidity, until tremors are succeeded by sleeplessness, delirium, and ultimately coma.

But, then, even granting it possible that the privation of a wonted stimulus may be the exciting cause of delirium tremens, is it a fact that it is so?

The supporters of the theory now under discussion do not make a positive assertion on this point.¹ They speak of the disease as "commonly resulting from the abstraction of the accustomed stimuli after a habitual or continued indulgence in it, or after a protracted fit of ebriety;"² or as, "chiefly when sobriety has followed a protracted debauch."³ Again, "Delirium tremens occurs more frequently when the accustomed stimulus is withheld."⁴ "The disorder frequently does not show itself until the accustomed stimulus has been withdrawn for a certain period."⁵ "Very frequently, from some cause or other, this habitual stimulus has been taken away."⁶ The stimulus "has been, in general, suddenly withdrawn before the disease distinctly shows itself."⁷ From these quotations it is apparent that some instances of the disease are admitted to occur without

¹ The only exception to this which I have met with, is the opinion of Blake, who says, "Delirium tremens is purely idiopathic, arising *invariably* from the same cause, namely, the sudden cessation from, or material diminution of, intemperate habits."—*On Delirium Tremens*, p. 23.

² Copland, Dict. Pract. Med., p. 498, sect. 9.

³ Ibid. sect. 10.

⁴ Carpenter, sect. 27.

⁵ Taylor, Med. Jurisprudence, p. 613.

⁶ Watson, Practice of Physic, vol. i. p. 398.

⁷ Alison, Path. and Pract. of Med., p. 734.

any privation of accustomed stimuli; and Dr Watson honestly says, "Sometimes it comes on in men who are perpetually fuddled, even although they have not intermitted their usual allowance of drink." It happened thus in several of the cases which I shall give at length in the present communication; and I could mention many other instances in which there was no diminution in the quantity of liquor consumed—and some even in which there was a decided increase—up to the moment of seizure with the disease.

Since, then, intoxicating liquors may be, or rather often are, the directly exciting, as well as the predisposing cause, of delirium tremens, the second position in the theory—stated with all the precision of a law, and which, as such, ought to be invariable—is untenable; and as the idea expressed in the first part of the theory, that, namely, of exhaustion of nervous power, has been formed on the supposed correctness of the second proposition, the whole structure must be abandoned as without foundation. To me it is apparent, that habitual excess in the use of stimuli is ~~like~~ the exciting and the predisposing cause of delirium tremens; and that if a suspension or diminution of habitual supplies be at any time attended by symptoms of the disease, these are not to be regarded as resulting from change in the quantity consumed, but as occurring in spite of such change, and because the peculiar constitutional effect has already been induced, and the premonitory stage of the affection already begun. I am persuaded, that every practitioner who has seen much of this disease must, on an impartial review and consideration of his cases, confirm this remark. For my own part, I can affirm, that in a very considerable number of instances the patients were drinking freely up to the period when the disease was developed, there being no interval, and no diminution of quantity; and in those instances—which, I admit, have been frequent—where there really was some diminution from the amount of previous supplies, it was on account of the system having already been brought into the condition of alcoholism, and a less quantity now produced a greater or equal effect, compared with that of the larger quantity taken formerly. There are, I admit, in some instances, an entire cessation from the use of stimuli, or very nearly so, at the time when the symptoms of delirium tremens are in the course of development, but this is because no more can be taken by such individuals:—they are already saturated, as it were, with the alcoholic poison. From overlooking these circumstances, I believe all the statements in regard to the effects of diminution, suspension, or abstraction of an accustomed stimulus have originated. The error is a popular one, and has arisen from imperfect inquiry into the history of individual cases, and incorrect observation regarding the circumstances connected with the supposed reduction or abstraction. When called to see a case of delirium tremens, on inquiry as to the habits of the patient, we are frequently informed by his friends, that for a long time large quantities of spirits, or wine, or malt, or of all of these—and per-

haps, in addition, morphia or opium—had been systematically consumed, but that for some time—a few weeks perhaps—much less had been taken, and within the last few days little or none; and then the inference is drawn for us, that the poor patient has actually brought on the attack by meritorious efforts to free himself from a habit of which he had begun to be ashamed. Now all this is very plausible, but not in accordance with the strict facts of the case, as the unfortunate victim himself, if put on his word of honour, will probably confess. The statement ought to be, that he was formerly in the habit of consuming large quantities of his favourite stimulus, until he found that a much less dose began to affect the system; that then he reduced the amount still further, but experienced an equal, if not greater, constitutional effect therefrom; and thus, from day to day, reduction was forced on him by his own sensations of gastric irritation, nervous excitement, and muscular debility—these feelings having been, in fact, neither more nor less than the premonitory symptoms of the attack of delirium tremens, and just what might have been expected if—as I have ventured to assert—the alcoholic principle is to be viewed as a cumulative poison.

The habitual and excessive use of intoxicating liquor, however, does not affect all individuals alike. Some drinkers are early cut off by diseases of the liver, heart, or other organs, to which they may have a hereditary or constitutional liability; others in fevers or inflammations, which they have no stamina to contend against; others by apoplexy or paralysis, from the direct effects of a debauch; and some from hereditary predisposition, or otherwise, are early doomed to spend the remaining years of a miserable existence in mania, idiocy, or in a general paralytic condition of the system.¹ Some few drinkers, again, by reason of extraordinary constitutional vigour, escape all these ills, and live on to old age; but the greater number who are not early removed from society by the diseases enumerated, suffer more or less from the attacks of delirium tremens. I believe there is a certain peculiarity of constitution which predisposes the individual to become affected with this malady—provided drink has been systematically indulged in—that, namely, in which there is a highly sanguine temperament, and a nervous, irritable disposition. And I believe also, that the readiness with which the disease occurs, and the mode in which it is developed, are well explained by other individual peculiarities and circumstances. While one is attacked suddenly, without any diminution of the quantity, or change in the kind of stimuli, another is more slowly and gradually affected, and it may be, after very considerable reduction in the supply of liquor. In these respects, however, the effects of the stimulant are simply analogous to those of various other potent medicinal agents. For

¹ This comprehends the group of affections described by Huss under the name of the Chronic Alcohol Disease, and which has already been referred to.

example, some constitutions are easily affected with mercury, others with difficulty. Salivation may all at once be displayed in one individual, who has taken the drug for a long time in very full doses, while in another it is produced by very gradual degrees, although with an equal amount of the mineral. But in both instances, after the constitutional effect is once produced, it may be kept up and increased to an excessive and serious extent by very small quantities of the mercury; and in the latter circumstances it would be very absurd to aver, that salivation was owing entirely to the more recent and smaller doses of the drug; or, still more absurd, if the mercury was altogether withdrawn, to say that the increase or continuance of salivation depended upon the abstraction. In like manner, when the nervous tissue of the brain has become charged with the alcoholic poison beyond a certain point, the effect it produces is kept up, and even increased, notwithstanding very considerable reduction in the amount consumed; and we are thus enabled to explain why diminution is almost universally supposed to be the cause of the malady, when, in fact, the indisposition to take more is itself one of the precursory symptoms of alcoholism. In the delirium ebriosum, there is urgent desire for drink during the violent stage of the affection, until a paroxysm of sickness occurs, which induces exhaustion, and then sleep, but in the delirium tremens, there is seldom any desire for it, even at the beginning of the attack, and certainly none when the affection is developed. Illustrations of the tendency to accumulation might be drawn from the effect of other medicinal agents, each acting in its own peculiar way, and on particular organs or functions. This, however, appears unnecessary, for I think it has been clearly shown, that the alcoholic principle—imbibed systematically—passing through the channel of the blood, in whatever way atomically combined or chemically changed, has its influence concentrated on the nervous pulp of the brain, accomplishes its work on the perceptive, sensory, and motor powers,—in one case quickly, in another, if not soon, at last suddenly; or, by disturbing the varied functions of the economy, it induces such a condition of the system, that a smaller quantity taken will ultimately produce a more intense and lasting effect.

These views of the subject, to my apprehension, explain also why delirium tremens is so readily brought on in dram-drinkers, when subjected to external injuries, or when seized with any kind of inflammation or fever. The sudden shock to the system in the one case, and the altered balance of the circulation and disorder of nutrition in the other, brings, I conceive, the individual at once into the condition of susceptibility to this disease, which would not otherwise perhaps have been so early accomplished. The effect is somewhat similar in the case of those who possess the gouty diathesis, for an injury of a limb is extremely apt to precipitate an attack of gout, which, in the ordinary course of events, would probably not have taken place for a considerable period of time. There is, I con-

sider, in the habitual drinker of a nervous temperament, a tendency to delirium tremens, as there is in the *bon vivant* of a certain temperament to gout, and as there is in the epileptical or hysterical subject, to epilepsy and hysteria, although in each instance there is great dissimilarity as regards condition, cause, and effect; and any sudden excitement, shock, or severe malady which powerfully affects and disturbs the vascular and nervous systems in individuals so predisposed, may greatly aid in bringing on a characteristic attack or paroxysm. It is in this irritable state of the habitual drunkard's constitution, although he may not be on the verge of delirium tremens, that alcohol, from its presence in the blood—in whatever way combined—and its interference with the nutrition of the brain and nervous system, will superinduce on the receipt of an injury—say a gunshot wound or a fracture—a delirium presenting somewhat of the appearance of that disease, but which in reality has more of a typhoid character. This complication has been named by Dupuytren "*Delirium nervosum s. traumaticum*;"¹ but it is not identical with delirium tremens, although some writers have considered it as such. Dr Blake asserts that at "almost any time he *could have* brought on an attack of delirium tremens in the habitual drinker, by simply taking him into hospital for three or four days, and keeping him on spoon diet."² But he does not appear, however, to have tried this experiment. It is an assumption from a theory supposed to be true, and has obtained probability from the fact already admitted and explained, that the disease sometimes occurs in those taken into hospital on account of sudden or severe shocks to the nervous system from injuries and other maladies, who have the alcoholic erythism strongly developed, and who are, in fact, already on the verge of an attack of delirium tremens. The gourmand would feel equally weak and miserable, and his general tone be for a time depressed by the abstraction of good living; but however strongly the gouty diathesis was in such a case, this deprivation of good things would not occasion an attack of gout, although an injury of any kind, nay, a scratch, might. So the dram-drinker, in whom the delirium tremens diathesis is not yet fully established, and who is not already under the precursory symptoms of the disease, could not be subjected to a paroxysm by such treatment. From the sudden change on his circulation, he would doubtless experience much mental disquietude and physical discomfort, and be made "shaky," according to common phraseology, for a time; but this would soon pass off, without the occurrence of the usual pathognomonic signs of delirium tremens, more especially without those spectral illusions which are common to poisonings, with several other agents of the narcotico-acrid class. Some even, borne away by the theory that this is a disease simply of "exhausted nervous power," and refining on the idea, have gone so far as to aver that it may occur independently of the

¹ "Annuaire Médico-Chirurgical des Hôpitaux."² Op. cit. p. 18.

use of intoxicating liquors altogether. Thus it has been alleged to have been produced by the use and disuse of opium and of tobacco; to have been resulted from protracted mental application, from excessive depletions, from rheumatism, from exposure to extreme cold, from hunger, etc., when no liquor of any kind had been indulged in. In all such instances there must have been some mistake. I can suppose that the continued use of inordinate quantities of laudanum might occasion delirium tremens, as has been reported, from the amount of alcohol necessarily consumed, which of itself would be sufficient to occasion it, and more especially when combined with opium, which, as I shall afterwards show, has a great influence in hastening its development; but that opium alone ever produced the disease I do not believe. I have never seen it; and when it is supposed to have been the cause, there must undoubtedly have existed some misapprehension of the history of the case, or some concealment as to the previous habits of the individual, for nothing is more common than for an opium eater to indulge also in some stimulating beverage. Opium, when habitually taken by itself, may, in the course of time, break down mental and physical energies, giving the aspect of premature old age, if not occasioning actual imbecility or paralysis, but it will not produce delirium tremens; and when left off suddenly, the poor victim of the enslaving habit will for a time feel very wretched and feeble, but will not manifest the pathognomonic symptoms of delirium tremens, and will have the best chance of regaining to some extent his constitutional vigour. In regard to any influence which the disuse of tobacco may have in occasioning this malady, I would say that it is quite out of the question, and that any attack occurring in the case of the recent smoker, must have been owing to the conjoined habit of drinking. As an illustration of the erroneous notions prevailing in regard to the matter, and to the disease generally, I will give one of the last published instances of delirium tremens—a good example of a mistaken cause, and a misunderstood effect—wrong theory, and wrong practice. It is entitled “*Delirium Tremens produced by Abstinence from Tobacco.*”¹ I shall introduce italics to draw attention to some points of importance.

“Delirium tremens, and its twin sister, traumatic delirium, are now so well understood to be dependent on *asthenic irritability of the nervous system*, that but one opinion prevails as to the principles which should regulate their treatment. *Sudden disuse of accustomed stimulants* is always to be deprecated, and in the event of a patient of known intemperate habits coming under surgical treatment, especially on account of an accident, care should always be taken that he is not deprived of his wonted allowance of alcohol. There is, however, another very potent drug in but too common use among the lower orders, the probable effects of suddenly relinquishing which, have, we suspect, been too little considered, and respecting which, the notes of a case lately under the care of Mr Curling, appear to offer a valuable hint to the practical surgeon. A withered old woman, a *gin drinker*, and a habitual smoker; was admitted on account of a *severe burn*. *Stimulants were from the first freely allowed her, and opiates administered*, but in spite of them she continued extremely restless,

¹ Medical Times and Gazette, No. 163, Aug. 13, 1853.

wandering at times, and quite unable to sleep. Her manner and aspect indeed *much resembled those of delirium tremens*. At this juncture, *several days after admission*, Mr C. ordered that she should be permitted to smoke. The salutary influence of the permission was at once apparent, the woman became quiet and tranquil, and on the next night slept fairly. All tendency to delirium disappeared, and she afterwards progressed steadily to recovery."

Now this was nothing but a mild case of delirium tremens, from habitual gin-drinking, precipitated by the severe burn, and aided by the stimulants and opiates so freely given; and in consequence of these combined circumstances, not in spite of them, the restlessness, etc., continued. The absence of the tobacco had nothing to do with this state of matters, but the disease originated from its ordinary cause, and was running its ordinary course of a few days. Convalescence was in all probability begun when the tobacco was allowed, but if not, no doubt its effect would be good, for it would act, not as a stimulant or a narcotic, but as a sedative—soothing and depressing the cerebral excitement, and sleep would follow as a natural consequence. It would not surprise me if tobacco alone, given in other cases, proved beneficial. Then as regards the other causes, independently of alcoholic liquors, said to produce delirium tremens, the kind of delirium differs in each case, or partakes more of the characters of insanity; and there is also a corresponding diversity in the nature of the wakefulness and of the muscular tremors and other symptoms—all of which circumstances, if space permitted, could be explained on very different grounds. But the mental phenomena of true delirium tremens, distinguished by a quick, eager, busy, apprehensive, spectral character, viewed in conjunction with the peculiar tremors, and sleeplessness, and with the uniform course and character of the other general symptoms, are surely sufficiently diagnostic. The term delirium tremens is, no doubt, pathologically incorrect, for incoherence and tremor may coexist in very dissimilar states of mind and body, and originate from a diversity of causes; but it has been so long assumed by the profession, and known by the public as applicable to a disease originating solely from continued and excessive indulgence in alcoholic stimuli, that a more general signification cannot be recognised without leading to confusion and error. The cause and the course of this interesting disease are so very different from that of the affections above noticed, that no affinity in nature or pathology can be admitted. The functions of the brain in it are, I conceive, interfered with in consequence of the vitiated nutrition of its substance, and the irritation of the membranes. A peculiar erythism and excitement, as I have already asserted, is thus set up by the continued introduction and presence of the alcoholic poison; and every drop of intoxicating fluid now supplied to the circulation increases the poisonous action. If the supplies are still increased beyond this point—the furthest limit of endurance—the unfortunate individual will, in all probability, be seized with fatal convulsions or coma; or be cut off, or shattered for

life, by the establishment of decided inflammatory action from protracted excitation of the brain and its membranes.

Now, if I have succeeded in showing that the alcoholic principle tends, by long-continued and excessive use, to occasion delirium tremens as a result of a specific—a poisonous action on cerebral matter; and that this happens on the principle of accumulation (as is the case with many other agents, such as mercury, lead, iodine, opium, Indian hemp, strychnia, etc., each acting in its own peculiar way), it should follow, that even a small quantity administered in the treatment of that disease, must necessarily increase the mischief instead of curing it. On theory, therefore, the rule of practice appears to me to be sufficiently evident; but whether or not the above reasonings and statements are considered sound and satisfactory, I must claim no inconsiderable amount of experience in recommending an entirely non-stimulant and non-narcotic plan of treatment. I am aware that in advocating the disuse of stimulants and opiates I may be considered guilty of a medical heresy. The practice objected to has, I have reason to believe, been for long almost universally followed in this country, to a greater or less extent. It has arisen, partly, from blind adhesion to the popular error I have already attempted to expose, which assumes that delirium tremens originates from, and is aggravated by a diminution, suspension, or abstraction of an accustomed stimulus, and therefore to be treated successfully only “by a hair of the dog that bit;” and it has arisen partly also from acquiescence in those modern pathological notions which attribute so much to the disordered organisation, and diminished nervous power of the solids, and so little to vascular disturbance, to chemical change, and to poisonous action of the fluids of the body. It is pleasant to observe the spirit of inquiry now drifting towards the much neglected claims of a humoral pathology, and bent on investigating the nature and extent of blood-poisonings; and I despair not of seeing, ere long, still greater advances made in this direction.

As regards the treatment of delirium tremens on the views which I have endeavoured to unfold, I may plead the experience of upwards of fifteen years; and state, that during five previous years I also had ample opportunities of witnessing the practice of others, and of personally testing the merits of the mode of treatment then, and still ordinarily pursued. In the earlier period of practice the observations were made almost entirely in connection with hospital and dispensary attendance, affording a great many examples of the disease in its pure and in its complicated forms, as occurring among tavern-keepers, brewers, butchers, and the lowest order of dram-drinkers generally; latterly the instances have been mostly among a better class of society, yet the disease presenting the same features, and originating from the same degrading cause. The frequent sudden fatalities which I witnessed from arachnitis, convulsions, and coma, when stimulants and opiates were freely administered; and the length of time ere recovery took place, even in the most favour-

able instances of the malady, when these agents were given more sparingly and cautiously, long since convinced me that their tendency is highly dangerous. I do not say that I would never give a stimulant in delirium tremens. It may possibly happen, although I have never met with such a case, that in the advanced stage of the affection the pulse may begin to falter, the heart lose its usual rhythm, the surface of the body to become of a leaden hue, the tremors to disappear, and subsultus tendinum occur, and delirium of a muttering character only continue, then I should certainly say that the flagging powers of life would require to be sustained by some diffusible stimulus. Here there would be no alternative. Then, again, I would not hesitate to give an allowance of his usual stimulus to a habitual drunkard when affected with a wound or ulcer to obtain a healthy action therein, or to administer stimuli of one kind or another freely in ordinary fever, or in the typhoid state of traumatic delirium, so that his circulation may be enabled to keep up the functions of organic life until food could be made use of. This would only be using legitimate means to maintain his ordinary condition of body; but it is quite another thing to prescribe alcohol when the individual is already manifestly in a state of alcoholic poisoning.

From all that I have seen and read, I believe that the combination of stimuli with opiates is a most hazardous practice in the treatment of delirium tremens; for while the former increases the determination of blood to the head, the latter is apt to occasion engorgement there, and thus, I have no doubt, they are the joint cause of many sudden deaths, and of many incurable palsies of body and mind—indeed of the great proportion of those casualties which take place, and for which the disease, and not the treatment, is blamed.

Opium given alone in delirium tremens is, I am aware, almost universally considered by the profession to be quite an indispensable agent—the *sine qua non*—for securing what is called the critical sleep; and hence it is prescribed in smaller or larger doses in as routine a manner as sulphur is for the itch, or colchicum for gout. Notwithstanding this high estimation of its value, however, I hesitate not to say that I consider it a very doubtful remedy even in the most promising cases of the disease, and a most dangerous one in others. It is well known that a moderate dose of opium in delirium tremens, so far as regards its action on the brain and nervous system, is in the first instance exciting and preventive of sleep. I have frequently seen such doses as in other affections would have been considered very large, in this greatly increase the agitation and excitement after each successive administration; and although sleep was secured at times, it was but short and disturbed, and followed by delirium as violent as before. Besides, the most unmanageable cases of delirium tremens which are met with, are those affecting opium or morphia eaters, who appear to be extremely liable to this disease if they indulge in spirituous liquors. From the use of opium or morphia alone, as I have already stated, I believe that true delirium tremens never occurs; but with

the unfortunate slaves of this debasing habit, a very short course of intemperance is sufficient to develop it.¹ I have also remarked in several of these instances, that if, during the attack the usual dose of the narcotic is taken under the impression that it would soothe distress and procure sleep, more especially if that dose be morphia—which is apparently much more stimulating in this affection than opium—the paroxysm is greatly aggravated.² It is evident then, that if opium is to be used at all in delirium tremens, it must be given in a large dose (in from two to three or more grains, and repeated at intervals of a few hours); and it is thus generally given, the object being to overstep the stage of excitement, and force on the desired sleep. Now the acknowledged effect of a large opiate on the encephalon is to occasion engorgement of the vessels, more especially of the veins, and consequently, the larger the dose, the greater will be the amount of sanguineous compression of the brain. What then must be the probable result in a disease in which there is already, if not an approach to arachnitis, at least a very excited action of the meninges, and a preternatural loading of the vessels generally? The cerebral functions are oppressed, and at length overwhelmed, and sub-arachnoid effusion is the result. The symptoms attending this untoward event are characteristic. Sleep is obtained, but it goes on deepening, and, as it becomes more profound, the pulse becomes smaller and less frequent, the surface of the body covered with a cold sweat, the face pale, the pupils contracted, the breathing slow and soft (although sometimes stertorous). An epileptic fit may now occur and terminate the scene, or the powers of life gradually become more and more depressed, and the victim perish as if in a profound and gentle sleep. Now this progress and catastrophe, although viewed as evidence of an unmanageable—a malignant form of the disease, in a bad subject, is nothing more than the common course and result of injudicious management. Even Graves, who prescribed opium in delirium tremens in the manner I will afterwards notice, warns emphatically against its premature and incautious use.

“Opium,” he says, “if given in the beginning, will increase the congestion and bring on sub-arachnoid effusion. I treated a case of delirium tremens in this way too boldly, and the man died with sub-arachnoid effusion; it was a lesson to me, and I advise you to profit by my experience.”³

I am convinced that it is in this way very many of the sudden deaths we hear of in delirium tremens occur. I saw it frequently in early practice, and have seen it occasionally since in the practice of others; and I am persuaded that any practitioner who has been accustomed frequently to treat this affection with large doses of opium, will be able, on reflection, to explain his want of success, and the occurrence of casualties. When, in fact, recovery takes

¹ See Case No. V.

² Clinical Lectures, vol. i. p. 530.

³ See also Case No. V.

place after a long sleep forced on by a large opiate, it is simply from the wonderful conservative power of nature resisting the evil influence of the agent, just as some will recover from a severe apoplexy or a palsy. The practice is one of the utmost hazard. If death were the certain alternative in delirium tremens should sleep not be early obtained—for it is said that “the patient must sleep or die,”—there might be some reason in attempting to force on the sleep by opiates. This, however, is certainly not the case, and consequently such interference is not only uncalled for but most improper, when there is danger to be apprehended from the practice. Sleep occurs as the natural, the favourable crisis, or rather termination of the disease; for it is not to be viewed as a part of the affection, or in the same light as we are accustomed to regard a critical sweat or other discharge. It is the result and the proof of an improved condition of the brain and nervous system—a salutary relaxation succeeding a state of dangerous tension. It will take place in the mild but genuine forms of the affection at the proper period, which, as I have already remarked, is on the second or third day, when the paroxysm has run its course, when the peculiar erythsm, the “nervous irritability,” is brought to an end, and a condition of “exhausted nervous power” now truly produced. That this may likewise happen in severe examples of the disease, although no opiate of any kind is given, the cases with which I shall close the present paper will prove; and while I am convinced that the plan of treatment now to be recommended will be found the most efficacious, I have no hesitation in saying that in a larger proportion of instances sleep would take place spontaneously at an earlier period, and the subsequent condition of the patient be much more sound and safe, by doing nothing at all, than by the use of opiates. I have seen very decided cases of the disease recover well when a mere placebo was given with a view to keep up the appearance to friends of something being done, and prevent them from using as remedies things which would be hurtful. Dr Ware of Boston, in an excellent memoir on delirium tremens,¹ strongly advocates from experience the do-nothing plan of treatment. Among other things, he says of opium:—

“In the cases which I have formerly treated with opium, and which have at last terminated well, a salutary sleep has not taken place till the close of the third day, let the quantity of opium be what it would. I have, indeed, seen sleep induced by opium at an earlier period, but it was premature, it passed into a state of coma, and the patient died. I am satisfied, therefore, that in cases of delirium tremens, the patient, so far as the paroxysm alone is concerned, should be left to the resources of his own system, particularly that no attempt should be made to force sleep by any of the remedies which are usually supposed to have that tendency, more particularly that this should not be attempted by the use of opium.”

¹ Quoted in the British and Foreign Medical Review, vol. xxiii. p. 603.

Dr Cahill¹ also cites several cases of the genuine disease, in which he found opiates decidedly injurious, and treatment without them salutary.

The treatment recommended by Dr Graves,² to which I have already referred, is advocated on the ground that opium is highly dangerous in the early part of the paroxysm. His rule of practice is to begin with tartar emetic alone, with the view of combating vascular excitement, then to add a little opium, and gradually to increase the quantity, keeping its action carefully guarded and controlled by the antimony, until at last, when engorgement of the cerebral vessels is no longer to be apprehended, to use opium alone. If opium is to be given at all in delirium tremens, this is certainly the safest mode of prescription. For some time I tried it, but from considerable previous experience of the beneficial effects of antimony in this disease,³ I soon became convinced that it was from that agent solely, especially its effects in the first stage, that ultimate benefit was derived; that the relative quantity of opium employed at first is too small⁴ to counteract the power of the antimony, or to produce any notable effect whatever; that in ordinary cases, ere the time arrives for increasing much the amount of the opium, the affection has run, or nearly so, its natural course, and the period for the salutary sleep commencing is at hand; and that when a greatly increased dose is given before this much wished-for change has arrived, there is a proportional increase of excitement and consequent delay of its occurrence.

From these considerations, I resumed the use of the antimony alone; and, during the last ten years, I have treated upwards of eighty cases of the genuine disease, many of them very severe ones, with uniform success,—not only in regard to the speediness of the immediate recovery, but the comparatively thorough restoration to a healthy condition of body and mind;—as much so at least, as could possibly be expected in individuals, many of whom had been, and were likely soon again to become, habitual drinkers. The dose which I have been accustomed to give has ranged from one quarter to one-half of a grain, in simple solution, every two hours, sometimes at shorter intervals, according to the degree of excitement and irritability. The action of the antimony appears to be chiefly sedative.

¹ Dublin Medical Journal. Observations on the Treatment of Delirium Tremens without Opium. Vol. xv. p. 397.

² Clinical Lectures, vol. i. p. 530.

³ This experience of the effects of tartrate of antimony I had before I was aware that Dr Graves had recommended it with opium, or that Stoll, Göden, Klapp, and others, had advised it in emetic doses.

⁴ Dr Graves' formula for first use is:

R. Antimon Tart. gr. iv.
Tinct. Opii. ʒi.
Aquæ ʒviij.

Signa. A tablespoonful to be taken every second hour. There is thus in each dose only five drops of laudanum to $\frac{1}{4}$ th grain of antimony.

Its direct influence is to reduce the vascular excitement of the brain, soothe the nervous system, and diminish muscular power; and its more indirect action is exerted on the functions of the skin, kidneys, and intestinal canal. In two or three instances only have I found it necessary to suspend its employment, in consequence of diarrhoea and hemorrhagic discharge from the bowels; and in these cases I substituted digitalis and ipecacuan with marked benefit; and I do not recollect of ever seeing it produce continued vomiting, although occasionally I have found the first or second dose eject from the stomach a quantity of bile. It is for the sake of its emetic effect that, in Germany and America, it has been prescribed in large oft repeated doses, even from four to seven grains every hour, and that, too, according to report, with benefit.¹ But although there is, doubtless, extraordinary tolerance of this agent in delirium tremens, I do not think that the use of such, or any other very heroic means, are warranted. Bleedings, large opiates, or large doses of tartar emetic, are all, although certainly not equally, unsafe, and therefore to be deprecated. An antimonial course of treatment in moderation, and with the design I have indicated, gently diminishes excited action, induces weariness of muscle, general nervous exhaustion, and mental languor. It thus removes all hindrances to the occurrence of the salutary sleep. It prepares the way for it, not by forcing, but by favouring it; and when the individual, exhausted, seeks his couch, and finds repose, that goes on, not as a drugged sleep, but as a purely natural and profound repose, from which he awakes with restored reason and muscular control.

Although I have recommended the tartrate of antimony as a chief remedy in delirium tremens, there are several other means essential to its successful treatment. In the department of medicinal agents, however, I have only further to suggest, that, should the bowels not be moved by the antimony, the compound powder of jalap (3i) will generally be found speedy and efficacious. The other means of cure belong strictly to regimen and diet; and the first of these in importance is bodily freedom. Nothing is more hurtful in delirium tremens than the restraint, particularly that of the strait-waistcoat. I have seen instances, and heard of many more, where I have no doubt the cerebral excitement was so increased by the never-ceasing struggle for liberty, that fatal convulsions at last afforded release. All the control required is the presence of one or two judicious attendants, who will humour the patient in his whims and fancies; who will speak and act regarding them so as to assure him of safety, and to relieve him of apprehension, which is the most characteristic feature of the delirium; and who will mildly but firmly interpose, if he attempts anything which may accidentally prove injurious to himself or others. Of course injury inflicted wrathfully

¹ Quoted in the work of Höegh Guldberg, already referred to. See also British and Foreign Med. Rev., vol. vi. p. 330. Also Copland's Dict. of Pract. Med., p. 501.

or vindictively is not to be anticipated, for rage, violence, or outrage, do not occur in this remarkable disease, but only in that affection which I have already briefly noticed, and with which it is sometimes confounded, namely, the madness of drink. Hence the frequent accounts met with in the public prints, of homicidal, suicidal, and other violent acts, said to be perpetrated during fits of delirium tremens, originate in an entire misapprehension of the nature of the two diseases. The apartment, however, in which the delirium tremens patient is confined should be well secured, for he may rush out at the door, or jump over the window, in the fright and frenzy of supposed danger. The larger, too, the room is, so much the better, that he may have ample space to advance and retreat, according as he wishes to scrutinize or avoid a suspicious or distressing object of his fancy; to arrange and re-arrange articles of furniture; or to carry on, after a fashion, the duties of some bustling occupation. All this expenditure of muscular effort, without any restraint, aids greatly the antimony in producing a safe kind and amount of physical and mental exhaustion, from which the patient, languid and worn out, at last lies down voluntarily, and falls into the much-desired sleep. It is thus, too, that "the walking drill," according to Dr Blake's experience in the West Indies,¹ was found efficacious in warding off attacks of delirium tremens in the case of drunken soldiers; not, however, as supposed, from the exercise proving a new stimulus in place of the rum, to which they had no access, but from its wearing-out effect, while the proper nutrition of the body was maintained. No one would ever think of ordering continued and monotonous hard work, and muscular fatigue, for an affection of "exhausted nervous power."

During the entire paroxysm of the attack, it is of some consequence to afford the patient abundance of light; not, however, as supposed by Dr Blake,² for its stimulant or excitant effect, but for its aid in correcting false optical impressions. The excited brain is very apt to receive erroneous impressions from the appearance of surrounding objects, if there is an uncertain light. Hence the exaggeration of many of those agitating and terrifying illusions and phantasms which more distinct vision would prevent, or quickly dispel. During the daytime, therefore, there should be no half-closed shutters, nor half-drawn blinds or curtains, but advantage taken of the clearest light available; and during the evening or night, the more distinct the artificial light is so much the better. Perhaps perfect darkness may serve the purpose equally well; but this can be available only in the well-padded chamber of a lunatic asylum; and, besides, in private practice, the other parts of the plan of treatment here recommended, which requires the presence of an attendant to regulate the doses of antimony, or other sedative, and to administer, from time to time, suitable nourishment, could not be carried on

¹ Op. cit., p. 19.

² Op. cit., p. 60.

without the admission of light. This leads me to remark, in conclusion, that, during the administration of the tartar emetic, I give, at intervals of a few hours, a moderate quantity of good beef-tea, mutton broth, or chicken soup, and sometimes *café au lait*, with the white of an egg switched up with it. Thus, while the vascular action in the brain is being subdued, and the nervous system liberated from the presence of the alcoholic poison, the functions of organic life are sustained, and a better ultimate recovery is secured.

I shall now conclude this paper by appending reports of six cases, in order to illustrate the views I have advocated in regard to the nature and treatment of delirium tremens, and these I shall give in detail, so that it may be acknowledged that the true disease has been understood and described. These cases, too, it must be borne in mind, are not complicated, or the symptoms masked by the effects of stimuli and opiates—unless in the instances of the unauthorized supplies hereafter to be noticed. They may, therefore, on first consideration, probably not appear as remarkably severe instances of delirium tremens. They are indeed, however, most characteristic examples of the affection,—three of them at least being considerably above average severity; and I have no hesitation in stating my conviction that all of them would have assumed a more severe complexion and taken a more serious course, had the ordinary remedies been employed; nay, that it is the stimulo-narcotic plan of treatment alone which makes this disease in almost any case appear a formidable one. For obvious reasons I shall be excused in not mentioning the real initials of the individuals concerned, or even the dates of attack. The cases are not selected; for, with one exception, they all occurred in succession at a period—some years since—when I purposed bringing my views before the profession; and, but for extending this communication to an undue length, I could have given the details of many more examples of the disease and its treatment. Enough, however, I trust, has been brought forward, or will follow, to show, that delirium tremens is a form of alcoholic poisoning—or an alcoholism; that its exciting, as well as its predisposing cause is the habitual abuse of intoxicating liquors; that these produce a specific form of irritation of the brain and membranes, the tendency of which is to arachnoid inflammation; that the chief phenomena attending this disease are invariably uniform in their character, and distinguish it from every other affection; that the occurrence of the salutary sleep is the normal termination of the paroxysm, indicating diminished activity of the cerebral circulation and functions, and the commencement of convalescence; that the cordial and opiate treatment is generally pernicious, and frequently dangerous; and that the main indications of cure, are, to reduce the cerebral excitement by a moderate but decided and steady course of antimony, or other agent capable of exerting a somewhat similar influence, and thus favour—not force—the wished for sleep;

to soothe the feelings and dissipate the fears of the affected by kind and judicious superintendence, and the permission of light and liberty; and to support the physical strength by a moderate allowance of animal nourishment.

CASE I.—Mr A., æt. 48, innkeeper. A habitual drinker, but seldom or never intoxicated, has been attended by me in eight different attacks of delirium tremens—more or less severe—within six years; and on all the occasions treated successfully without stimuli or opiates. For some time previous to the present attack he is said to have been drinking less than in former years, in consequence of ill health, and still less within the last fortnight, although every day, early and late, imbibing a little with his customers. On my first visit I found him very dull; without appetite; sleepless; complaining of slight cough and pain in the chest; tongue slightly furred; bowels constipated; pulse 90. As yet no visions, and no decided tremors. Calomel gr. iv. Pulv. Jalapæ, comp. ʒi., ordered.

2nd day.—Bowels slightly opened; cough troublesome, but no bronchitic signs. In other respects same as on previous day. On account of the catarrhal symptoms ordered a mixture every four hours, each dose containing, Sol. Mur. Morphicæ m ʒ, and Liq. Acet. Ammon. ʒij.

3rd day, noon.—Paroxysm of delirium tremens evidently begun. Had passed a restless night. There is considerable tremor and agitation of manner. Pulse 94, full, but soft. Discovered that he had been getting a little wine and spirits from time to time during the last three days. Forbade everything of the kind, and withdrew also the mixture ordered yesterday.

4th day, 10 A.M.—Pulse 106, soft and small; hands and tongue very tremulous; face pale; perspiring copiously. Could not stand still for a moment, but darted from one window to another, as he anxiously expected the police to come for thieves, who, he said, were tied up in the next room. They had been stealing his property for the last six days, and he had just been writing down a list of the articles amissing. This inventory he showed to me, but it was utterly unintelligible. He knew me at once; and answered correctly questions when this could be done in four or five words. It appeared that he had been in bed, but was sleepless, tremulous, and agitated, until 3 A.M., when he was seized with what his wife described as a fit. She then gave him a wine-glassful of brandy, and ever since he has been up and much excited,—running about the house after imaginary rats and thieves, and once escaped to the street in his night-shirt publishing his wrongs. A trustworthy attendant was now placed over him with instructions to see that he had as much freedom and light as possible. All stimulants prohibited, and the following mixture ordered:—℞ Tart. Ant. gr. iv. Vini Ipecac. ʒij. Aquæ ʒviij., ʒv.—a tablespoonful to be given every two hours. A cup of tea, café au lait, or beef-tea, to be given at a few hours' intervals, if cared for.

8 P.M.—Pulse 96. Bowels twice moved since the morning; perspiring copiously; and much calmer, although still talking in a rambling manner concerning all sorts of difficulties and troubles. A cupful of beef-tea twice taken. The antimony has been given regularly, and is to be continued until there should be an appearance of depression, and a wish to go to bed.

5th day, 11 A.M.—He had his last dose of tartar emetic at ten o'clock last night, and soon thereafter appeared exhausted, and was prevailed on to go to bed, when he slept profoundly from 11 P.M. to 5 A.M. He had then some tea and bread, and had been sleeping again until now. Is dull, but quite rational; pulse 70, soft and regular; skin moist; bowels once purged. To be kept very quiet, and to have nourishment as formerly directed.

6th day.—Had slept the most of previous day, and all the last night; is now quite convalescent, although weak.

Remarks.—A good example of an ordinary case of delirium

tremens; reduction of accustomed stimuli from inability to take more; aggravation of all the symptoms from (unauthorised) administration of stimuli, and probability that the disease would have taken a more severe form had these been continued; early and decided improvement under treatment with antimony, animal nourishment, and careful watching; the patient a living testimony of the safety of a non-stimulant and non-opiate treatment from good recovery under so many attacks of the disease—the more frequent the recurrence of delirium tremens, the greater being generally supposed the danger.

CASE II.—Mr B., æt. 48, spirit-dealer. Long an habitual drinker. His average daily amount for some time had been four gills of whisky and one bottle of beer, taken from early in the morning until late at night; and there had been no diminution in the quantity previous to the present seizure. Had slept very little for a week, and none at all on the last two nights; and for some days was very tremulous, and quite unable to transact business.

1st day's visit, 3 P.M.—Was very distressed and agitated during the last night,—walking constantly up and down through the house, terrified with visions; had his last glass of whisky at 11 this forenoon. Pulse 104, small; skin cool and clammy; great muscular tremor; tongue foul; eyes yellow and lustreless; mind constantly occupied with false and horrific impressions of all kinds, although in no very definite form; but can answer a question put directly to him. *Instructions*—Plenty of light; complete liberty to promenade through the house, the doors and windows being secured; and two intelligent men to attend and humour all his fancies. To have a wine-glassful of the following mixture every two hours:—℞ Tart. Ant. gr. iv., Infusi Quassiae et Aquæ $\bar{a} \bar{a}$ 3x., whether it sickened or not, and only to be discontinued if he should go to sleep. Beef tea and coffee with milk to be given occasionally. 8 P.M.—Took one glass of the mixture at 3.30 P.M., which caused vomiting of a quantity of bilious matter; one at 5 o'clock, which was followed soon after by a loose alvine evacuation; and one at 7 o'clock. He is at present pale and perspiring; very tremulous and restless—in constant apprehension of rats and strange men; quite sensible when spoken to; pulse 110. To have the mixture only every third hour. Beef tea, etc.

2d day, 10.30 A.M.—Pulse 106, very small; perspiring freely; face very pale; urine scanty and high-coloured; great tremulousness. He can put out his tongue, or rise up, or sit down when desired, but that is nearly the amount of his intelligence. He is in constant motion, not rapid or boisterous, but chiefly busy in arranging bed-clothes, carpets, small articles of furniture, and sweeping imaginary crumbs from off the table. Had never been in bed, and had taken only three doses of the mixture since I saw him last. Took a glass from me, supposing it to be pale brandy:—no sense of taste. The mixture to be continued regularly. Was seen by my friend Dr Cappie at 3 P.M., and again at 9 P.M., who found him much the same as when last reported. Had been purged several times. Antimony, etc. continued.

3d day, 2 P.M.—In bed, sound asleep; pulse 84, of good character; a good deal of subsultus tendinum; skin very moist; paleness of countenance gone. It was stated that he had appeared very much exhausted last night about 12 o'clock; was then got to bed, fell asleep almost immediately, and did not awake until 7 this morning. When awake he was not quite sensible, but took some bread, coffee and milk, and fell asleep again. Continued so for other two hours, and was then perfectly coherent, but not inclined to speak. He had some more breakfast and an egg, and went to sleep again. An hour ago he was awake for a few minutes, and took some beef tea. The antimony had been given once this morning:—to be discontinued. Nourishment only to be offered when he awakes.

4th day.—Found him quite well; mind perfectly clear, and had been able to read a little.¹

Remarks.—An ordinary case of the disease, rather more severe than Case I.; no suspension of wonted libations up to the period of seizure; excellent illustration of the *modus operandi* of the tartar emetic; and also of the benefit derived from the other means recommended for sustaining the organic functions, and bringing about natural sleep.

CASE III.—Mr C., æt 30, engineer. Had been twice formerly under my care in delirium tremens, and recovered well without the use of stimuli or opiates. Has been drinking largely and constantly for some months past, and exhibiting at times excessive irritability and violence of temper—even to the extent of threatening the lives of his wife and children. In apprehension of this disposition he was some time since treated for delirium tremens in the Morningside Asylum; but on what plan I do not know, save that he was confined in a dark chamber. On the present occasion, at my *first visit*, I found that he had been drinking up to the moment of his attack, which had commenced decidedly two days before. His pulse was 110, soft, and of tolerable strength; hands very tremulous; aspect extremely haggard; skin moist; tongue clean. He had been quite sleepless for two nights; but not violent in his manner or conduct. He was laughing and talking incoherently,—looking constantly under his pillow, and carrying on a conversation with imaginary beings underneath, in this way,—“aye, oh yes, yea, certainly, just so,” etc. On requesting to know what the devils were wanting, he replied, “a glass of whisky.” Prescribed—℞ Tart. Ant. gr. vj. Aquæ ℥ij. Solve. A wine-glassful every two hours; and desired that he should be closely watched, and kindly treated. To have some weak beef tea occasionally.

2d day.—Had a few minutes sleep this morning, but his general aspect is in all respects worse. Pulse 116. He is very restless and agitated, wishful to get out; thinks his shop is on fire; that the police want to get hold of him, and has many such like fancies. I discovered that a bottle of table-beer had been given to him this morning. A wine-glassful and a half of the antimonial solution to be given every two hours.

3d day, 11.30 A.M.—He had walked about all yesterday in a state of great excitement; got the antimony very regularly, and lay down for the first time about 10 P.M., when he fell into a sleep. This continued until 6 A.M., when he awoke quite collected, and has since continued so. Pulse 80; hands very tremulous; has taken a good breakfast, and is in all respects apparently convalescent.

4th day.—Quite well.²

Remarks.—A well-marked case of delirium tremens occurring in an individual in whom the *delirium ebriosum* might rather have been expected; but the long-continued course of intemperance gave to this attack the usual characters of cerebral alcoholism. The case also shows the liability to this disease without diminution of the

¹ This patient has since had another attack—not quite so severe, originating, without any diminution of habitual supplies, and successfully treated in the same way as on the former occasion.

² Since the above occurred, this patient was removed to the Royal Infirmary, under another attack of delirium tremens, where he died in an epileptic seizure. I understand that the plan of treatment practised in that instance was the one usually followed—namely, restraint in a strait-waistcoat, stimuli, and opiates.

wonted stimuli, and the tendency to aggravation even from a slight stimulus, such as table-beer; it also illustrates the singularly sedative effect of antimony.

CASE IV.—D., Esq., æt. 40, of highly sanguine temperament and nervous, but kind, disposition. Has had two previous attacks of delirium tremens under my care, both very severe. Was a total abstainer for some time following his last illness, having been informed that he would not probably survive another attack, or if so, that in all likelihood he would become insane; but had been gradually led back to drinking habits through company. His digestion having soon become so impaired that he could not take substantial nourishment, he drank systematically to overcome the distressing sensations of sinking. Four days prior to my first visit he was unfit for business; had disturbed wakeful nights; was very tremulous and nervous—being aware that delirium tremens was approaching, and fearing that he might be deprived of reason; and he had lost entirely the appetite—even for drink. Some brandy, however, had been given to him on the previous day, and his bowels were well cleaned out by laxative medicine.

1st day, afternoon.—Found him very much agitated, and talking quickly and incessantly. He was hearing sounds which reproached him as a bad man. Thanked me for visiting him, saying that he was undeserving of notice, having been such a rogue, and so cruel to his wife and children. Pulse 100, soft and full; skin warm and moist; countenance pale and anxious; tongue and hands very tremulous. R. Tart. Antim. gr. iv., Aquæ font. et Aquæ Cinnam. ā ā ʒiij. Sig. One tablespoonful every two hours, or oftener should there be more excitement.

2d day, 10.30. A.M.—Had several times last night a few minutes sleep; pulse 104; tongue white. He is at present more excited and restless; looks on himself as a lost man; is constantly hearing strange sounds; and is every little while eagerly examining the corners of the room. An experienced male attendant now placed over him with usual instructions. Antim. continued.

8 P.M.—Much the same. Mind more agitated with regret, and the apprehension of some impending calamity.

3d day, 10 A.M.—Has spent a sleepless and agitated night. His aspect is very anxious and apprehensive. Considers himself “between the deep sea and the devil; in fact, too bad for the devil himself.” Pulse 108; tremor great; tongue cleaner; bowels confined; sweating considerably at times; urine scanty and high coloured. Tartar emetic continued; and to have Calomel gr. iv. Pulv. Jalapæ Comp. ʒi.

7 P.M.—Pulse 110, smaller than formerly; bowels have been twice opened. He is at times moving rapidly about as if searching for something; now whispering as if aware of some secret; and then again standing gazing at the floor bowing and scraping, and answering questions as if before a high tribunal. Already 12 grains of the tartar emetic have been given. To have now $\frac{1}{2}$ grain doses every two hours, or oftener if he becomes more excited. Also to have some beef-tea occasionally.

4th day, 11 A.M.—Had passed a violent night; fancied robbers were in the house, etc., and occasionally he shouted so loud as to be heard in the street. Had a large basinful of beef-tea at one time, and some coffee and milk at another, during the night. Between seven o'clock last night and seven this morning he had taken 6 grains of antimony. Shortly after the last dose he fell asleep, and has slept until now—four hours—awakening quite sensible. His look is now free from apprehension, but haggard and as if worn out. Voice and manner wonderfully firm. Pulse 96, and of good character. To have beef-tea, and no more antimony.

8 P.M.—Is again much worse. He is lying on the sofa sullen and dejected, with a very maniacal aspect, and declaring that he was the worst of men and

doomed to die. It appeared that he had gradually been getting worse since the forenoon. His keeper and friends supposing him beyond risk, had allowed several acquaintances to visit him; and it was my firm conviction at the time, that a stimulant had been administered by some one. The half-grain doses of tartar emetic were ordered to be renewed.

5th day, 11 A.M.—Had wandered from room to room all the past night. Thinks that the newspapers contain a great deal about him, and that various enemies are plotting his destruction, etc. Pulse 98, firm; tremor inconsiderable; perspiring freely; bowels opened twice during the night. Antimony to be continued.

10 P.M.—Was very calm about noon; sat for some time in the parlour with his wife, reading the newspapers, and kissed the children. He then again became much excited. I found him standing in an attitude and with an expression of reverential awe, arms extended, body slightly bent forward, and eyes turned upwards. His language was as if answering questions put by his Maker, such as, "Yes, Almighty God."—"Dr Peddie cured me of my fever, Almighty God."—"M.D., Almighty God," and so on. Having sat down for an instant I was implored with an expression of deep alarm and concern to get up, otherwise I would be killed on the spot. Pulse 118, not so firm as in the morning; skin moist; tongue pretty clean; bowels twice moved. Had taken tea and some soup. The antimony to be continued.

6th day, 10 A.M.—Had passed a very restless night, having had altogether only about half an hour of sleep, from which he has newly awoke. Lying in bed with his clothes on, pretty calm. He said, "Don't speak to me as you little know what a bad man I am. The sooner I am out of the world the better, for I am to be publicly whipped through the streets to-morrow morning," etc. Pulse 92; not much tremor; tongue a little furred. Bowels repeatedly purged. On account of this looseness I withdrew the tartar emetic and substituted—*R* Vini Ipecac. Tinct. Digitalis, \AA \AA 3ss., 20 drops to be given every three hours.

9 P.M.—Had been much much calmer since 2 P.M. The last mixture to be continued.

7th day, 10.30 A.M.—Had slept soundly from ten last night until nine this morning. Is still gloomy and desponding. Digitalis, etc., to be continued at intervals of eight hours, and as much nourishment as he will take.

8th day.—Passed a good night, and is in all respects much better.

From this period he gradually improved. Sleep became more and more refreshing, and the mind stronger. In a week he was able to resume all his ordinary occupations; and has continued ever since—after a considerable lapse of time—temperate and well.

Remarks.—This case is an example of true delirium tremens at the outset, modified somewhat in the relapse, with symptoms of a maniacal character occurring in an individual apparently so predisposed, but not of the nature of delirium ebriosum. This maniacal relapse was in all probability occasioned by some improper tampering. It shows also the benefit derivable from ipecacuan and digitalis, when the tartar emetic begins to purge too much.

CASE V.—Mr E., \AA 41, journalist. Had for many years been in the habit of morphia eating. The ordinary quantity consumed was 9 grains of the solid muriate per day, or 3i. in the week. Had been under my care (over a period of several years) during seven previous attacks of delirium tremens, most of them severe, and on all occasions treated without stimuli or opiates. After each attack he abstained for a few days or weeks from morphia, but the necessity of fulfilling some literary engagement drew him again into the vice. His whole appearance was that of the confirmed opium eater, yet there never had been any tendency to delirium tremens so long as alcohol was abstained from. The occasions in which he indulged in intoxicating liquors to any extent, only occurred at intervals of many months. Begun by the excitement of the social

board on some festive occasion, they continued for from two to three weeks, but never so as at any time to produce decided intoxication. The quantity generally taken amounted to three wine-glassfuls of spirits, and one pint of porter daily; and this short course of drinking invariably led to an attack of delirium tremens. While indulging in liquor, he had always found it necessary to diminish a little the habitual dose of morphia, on account of the nervous irritability and tremor which he soon began to experience.

1st Visit, 5.30 P.M.—Was made aware that the course of cause and effect has been the same on the present as on former occasions; and that for the last four days, feeling himself under all sorts of horrors and fancies, and unable to sleep, he had left off the morphia entirely. Last night, however, he had taken three grains, having been induced again to try if it would produce a composing effect, but instead of this, it made him, as he himself expressed it, "ten times worse." He is at present walking up and down in a most wretched condition. States that strange visitors had been all day talking to him in whispers; that his breath as it went in and out took the form of whispers accusing him of misconduct, which, he says, is little needed, as his conscience is sufficiently reproving. Feels as if he had two heads—the one conjuring up fancies, and the other thinking and judging correctly. Asps are also crawling on his breast, and he cannot shake them off, etc. Pulse is 100, soft, and of very good strength; pupils contracted; tongue clean; no appetite. Tremors, not only of his hands and tongue, but of his head and whole body have much increased to-day; and his voice also is bleating and unsteady. Says he has slept none for some nights, and is much afraid of the one approaching. He also states he had vomited himself freely with some antimonial solution which he had beside him since last illness; and likewise that he had purged himself freely. Desirous of ascertaining whether any other depressing or sedative agent would answer as well as the antimony formerly employed, the following was prescribed:—*R* Tinct. Aconiti gtt. x., Aquæ *℥*iv.,—a tablespoonful to be given every two hours, and some beef-tea to be given occasionally.

2d day, 10 A.M.—Has spent a very restless night; had some short, but very disturbed sleeps. Asps and other reptiles are crawling in great multitudes about the bed. Appearance of countenance most wretched; skin slightly clammy, and of a dirty colour; tongue a little white; pulse 110, weaker than yesterday; urine scanty, and high-coloured; no appetite. Had taken during the course of the night one-half of the mixture (Aconite 5 *m*), but being unable to visit him frequently, so as to watch the effects of the medicine, and fearing it might prove too depressing for the circulation, I withdrew it, and ordered instead 10 drops of Ipecacuan wine, to be given every two hours, and *℥*i. of the Pulv. Jalapæ Comp. at noon. To have also beef tea.

4 P.M.—Much more excited and delirious; says there is a court sitting in judgment on the five senses, etc. Tremors are excessive, and he is sweating profusely. He confesses to me that his attendant, thinking him very weak, had at noon given him a wine-glassful of spirits. And on close questioning, also admits that he has some morphia in his possession, but will not say whether he has taken any, and will not give it up. To have 20 drops of the Vin. Ipecac. every two hours.

10 P.M.—Is rather better. Pulse 108. Says that he has had a great many strangers visiting him, pressing him very much to go out with them. Pupils contracted.

3d day, 10 A.M.—Has passed a most agitated night; walking from room to room incessantly; sometimes waving a white handkerchief from the window, under some notion of making peace with God. Has taken a little breakfast. He had gone to bed a little before my visit, and is lying with his eyelids half-shut, and squinting when they are open; is working with his hands in the air, as if arranging things, or seizing objects between the finger and thumb, or pointing a way. There are great general tremors, and considerable twitchings of the eyebrows. Pulse 110, and very weak. To have plenty of good beef tea; also to have the Vin. Ipecac. continued.

4 P.M.—Pulse 114. Is sitting up in bed, very pale, and talking more humorously. A lady and gentleman, whose portraits are hung on the wall, have been speaking to him from out of their frames, and annoying him much, but he had discovered that by a particular wink of his eye, he could make the one jump into the frame of the other, and thus stop their discourse. His attendant delivered to me a small packet of morphia which was found in his possession, a search having been made for it by my instructions; and he did not deny that he had been helping himself to a little from time to time. Ipecac. to be continued; also beef-tea, etc.

4th day, 10.30 A.M.—Had spent a very turbulent night. Is lying at present with a poker in his hand, with which he had been warding off intruders. Now, however, he is asleep, and has continued so since 9 A.M., but is very distressed, judging from his moans, and the movements of the muscles of the forehead and eyebrows.

2 P.M.—Has just awoke after sleeping five hours. Pulse 84; hands very tremulous, and voice bleating; tongue cleaner. Mind confused, but no raving. Inclined to be quiet. To have beef-tea or a mutton chop, if he can take it. Discontinue medicine.

5th day, 10 A.M.—Has passed an excellent night. Pulse 80, soft, and of good character. Feels his head quite cool and clear; and talks intelligently on some favourite subjects of study.

6th day.—Greatly improved. Intends to walk out a short distance.

7th day.—Feels better to-day than he has done for months.¹

Remarks.—The above case is an illustration of the opinion that morphia alone (or opium), will not produce delirium tremens, but that the combination with stimuli will very readily induce an attack—even a very short course of drinking. Here also it is apparent, that the morphia latterly produced a stimulating and injurious effect, so much so, that during the premonitory stage of the delirium tremens, the patient voluntarily diminished the dose; and that subsequently, its stealthy use protracted and aggravated the attack. I am of opinion that the ipecacuan did good (although, perhaps, not so much as the tartar emetic would have done); and that its effects would have been more apparent had spirits not been taken on the second day of the paroxysm, and morphia repeatedly.

CASE VI.—Mr F., æt. 41, clerk. For a number of years a hard drinker, and for some time past indulging to a great extent, but said to have been very moderate during the last six or eight days, in consequence of general indisposition. Appears to have twice had delirium tremens. On the last occasion is said to have been very violently and dangerously ill for three weeks, and to have been treated with spirits and opiates.

1st day.—Pulse 98; tongue foul and tremulous, also considerable tremor of hands, and agitation of manner; countenance anxious; bowels much disordered; sleepless. To have R. Mass. Pil. Hydrarg. gr. iv., Ext. Colocynth. gr. vi. Misce ut fiant, pil. ij.,—for immediate use; and afterwards 30 drops of the Vini Tart. Ant. to be taken every three hours.

2d day, 6 P.M.—Was unable to see him earlier. Pulse 98, full, but soft; bowels have been freely purged. Slept none since last visit; has been very restless all day; and being himself apprehensive—from previous experience—of the approach of delirium tremens, has ordered the windows to be nailed

¹ This patient died sometime since apparently from an apoplectic effusion, without having been under any form of treatment. Upwards of two years had elapsed since the attack above described. The practice of morphia eating, however, was soon resumed, and was persevered in to the last.

down, and his razors removed. Antimonial wine to be continued. 9 p.m. Is now seeing objects in bed, and becoming more restless. Pulse 100, of good character. Tongue cleaner. Now to have the antimony in larger doses. R Tart. Ant. gr. iv., Aquæ bij.,—a wine-glassful to be given every two hours. Arrow-root and coffee with milk allowed. To have liberty to walk about the house; to have all his opinions and diseased impressions humoured as much as possible; and to have the advantage of clear light.

3d day, 11 a.m.—Says that he has had eight hours of excellent sleep, and feels quite well and comfortable. In reality, however, he has spent a very restless night; up and down through the house several times with a lighted paper, looking for thieves. Has been much quieter since the last dose of the antimony. Pulse 106; more tremor; considerable warmth and moisture of skin. Antimony to be continued, and beef-tea to be given occasionally.

4th day, 10 a.m.—Pulse 110, weak, and slightly intermitting; muscular tremor great; perspiring copiously; pupils large; face pale; urine scanty and high coloured; bowels open; no sleep; and very much excited with all sorts of fancies, although he can answer any question distinctly. 8 p.m.—In most respects much as he was in the morning. Pulse 120; skin clammy. He has been travelling all day along with his wife, and something or other has constantly been going wrong. Now, they are both (his wife to please him) sitting in bed with their knees drawn up to prevent water covering them, as it has got into a boat in which they are crossing a river, etc. To continue the antimony, etc.

5th day, noon.—Pulse 100, and steadier. Tremor not quite so great. Has taken some breakfast with relish, and more beef-tea. Has been in bed several times over night, but has only had one hour's sleep. At present is doing penance by walking on the floor barefoot. To continue the antimony, etc. 4 p.m. Is much calmer; pulse down to 86. In order that fatigue and consequent sleep might be produced, advised a half-hour's walk out of doors in charge of a friend. 7 p.m. Sent for hurriedly. He is much more excited—more than he has been for the last 24 hours; pulse 115; said that he saw his wife disposed of at a lottery a few minutes ago to another gentleman, which he considered most disgraceful, etc. He had been out walking for a very short time only; and although I suspected that liquor had been given to him by some one, I could not ascertain that this was a fact. Antimony now to be given in half grain doses every two hours.

6th day, 11 a.m.—Has passed a very agitated night. A short time since made an attempt to get over a window, and knocked down a large flower box into the street in the attempt. Is now writing dispatches to the Duke of Wellington, as he thinks himself in a besieged fortress. The writing is mere scratches of the pen, no letters being formed. Pulse supposed to be about 120, but the muscular tremor and tendinous jerkings are so great as to prevent its being correctly counted. Half grain doses of antimony to be given every hour, and beef-tea liberally. 11 p.m. Shortly after last visit the excitement had begun to subside, and with each successive dose of the tartar emetic he was observed to become calmer and more rational. At 3 p.m. he was taken out by a hired keeper for a short walk, but the latter having shortly before had some drink, took his charge into a spirit shop to get more. Here Mr F., although offered liquor, declined; but the attendant having drank freely, and being unable to take care of himself, the former had actually to help him home. Worn out by the exertion, Mr F. went immediately to bed—half-past 4 p.m.—and has slept soundly until now. He is quite composed and rational. Antimony discontinued.

7th day, 11 a.m.—In all respects quite convalescent. Had eight hours sleep over night.

8th day.—Left cured.¹

¹ Since the above illness, this patient has had another exceedingly well-

Remarks.—The above is an instance of a somewhat severe attack of delirium tremens, in which recovery took place under the use of tartar emetic, etc., at a much earlier period than on the previous occasions when the treatment was stimulo-narcotic. It is also an additional illustration of the fact that the accustomed stimuli are frequently diminished by the patient himself when the disease is forming and advancing, simply from a sense of inability to stand the same amount as formerly. It likewise illustrates what I have frequently observed, namely, a severe outbreak of excitement shortly before recovery commenced, but which must be unhesitatingly met with the antimony, perhaps in increased doses.

ARTICLE II.—*Remarks on Quekett's "Histology;" on Kölliker's "Human Histology, vol. i.;" and on the Physiological Importance of the Nucleus of the Cell.* By MARTIN BARRY, M.D., F.R.S., F.R.S.E.

THE lectures delivered by Professor Quekett at the Royal College of Surgeons of England, on the Elementary Tissues of Plants and Animals,¹ give a most comprehensive view of a widely extended subject, without any neglect of minute details. And, far from being a repetition of what others had described, they are everywhere marked with evidence of original research—research, too, in a collection of preparations that has no rival, and evidently with a keen microscopic eye.

The chapter on "cells," in the translation of Kölliker's "Manual of Human Histology," vol. i.,² furnishes evidence of progress, deeply interesting to one who sees in all the figures, and in many of the author's remarks, confirmations of facts which he himself, the author of the present communication, had recorded fourteen years before.

When the microscopic inquirer has become acquainted with important facts, and left no means untried for testing the accuracy of his observations, it is his duty to record the same, however much

marked attack of delirium tremens of considerable severity, from which he recovered satisfactorily. He was early put under the tartar emetic by an intelligent student of medicine, who lodged in the house, and who had observed the success of the practice on the previous occasion (and who had cut short two other attacks by the same means). While the paroxysm was at its height, I examined the urine voided, which was scanty and high-coloured, and found it highly albuminous, and on spontaneous evaporation exhibiting a vast number of phosphatic stellæ. Immediately after sleep was obtained, I examined another sample. It was unchanged by heat, but became slightly turbid with nitric acid, and on evaporation there were no phosphates discoverable, but a large amorphous deposit of urate of ammonia. Two days subsequently the secretion was again examined, and found to be perfectly normal.

¹ London, 1852.

² Translated and edited by George Busk, F.R.S., and Thomas Huxley, F.R.S. Vol. i. 1853.

opposed to existing views ; for if he has faithfully represented nature in his drawings, he may “ bide his time,” resting assured that the day will come when others will see what he has seen. Of this my own experience has given me many proofs. The volumes by Quekett and by Kölliker furnish many more. Such at least, I think, will be found the case by those who may feel disposed to compare the drawings of these excellent observers with my own.

In 1840, when my views on the physiological importance of the nucleus of the cell were announced,¹ I had the misfortune to stand quite alone ; for no one then had questioned the views of Schleiden and Schwann, that the nucleus, after forming the membrane of the cell, “ either remains inert,” or, “ as a useless member, is absorbed.” A separation of the nucleus into two or three parts, where previously observed—viz., in the globule of pus and mucus—had been attributed by Güterbock, Henle, and others, to a chemical reagent used in the examination—acetic acid.² But I showed that neither acetic acid nor any other foreign substance is required to produce the separation in question ; this separation being natural, apparently common to nuclei in general, and forming part of the process by which cells are reproduced. Farther, I showed that young cells originate through division of the nucleus of the parent cell, instead of arising as a sort of “ product of crystallization in the fluid cytoblastema ” of the parent cell, as since supposed by Reichert.³

The following are some of the principal facts which I recorded—the first three in the Phil. Trans. for 1840, and the others in the Phil. Trans. for 1842 :—

1. The nucleus neither “ remains inert,” nor “ is absorbed as useless,” after the formation of the membrane of the cell.

2. The cell is propagated by division of its nucleus.

3. This division of the nucleus is initiated by division of the nucleolus.

4. Fibre is *originally* flat, and marked with striæ, and it has *originally* a crenate edge.

5. Fibre shows a constant tendency to lose its striation, and become rectilinear.

6. In some cells the nucleus is seen giving off fibre.

7. Sometimes the entire nucleus becomes a *ring* of fibre.

8. Sometimes the entire nucleus passes into a *coil* of fibre.

9. Sometimes fibre given off by the nucleus of a cell is met with, simply deposited upon the inner surface of the cell-wall.

10. Fibre given off by the nucleus of a cell sometimes thickens the cell-wall.

11. Fibre is sometimes seen entering into the formation of membrane.

¹ “ *Researches in Embryology, Third Series ; a Contribution to the Physiology of Cells.* ”—Phil. Trans. 1840.

² Henle, Hufeland's Journal, Mai 1838, p. 62, Pl. I, figs. 9-12.

³ Das Entwickelungsleben im Wirbelthier-reich, 1842, p. 2.

12. The axile fibre of nerves has a structure sometimes giving it transverse striation and a crenate edge.

13. The "white substance" of nerves has a structure, giving it originally transverse striation.

14. A mother-cell is sometimes seen divided into two compartments by two daughter-cells, whose walls are composed of fibre, and where in contact with one another form a thick septum.

Is there to be found among the figures given by Professors Quekett or Kölliker anything like a confirmation of these facts?

1. *The nucleus neither "remains inert," nor "is absorbed as useless" after the formation of the membrane of the cell.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"Both kinds of cell-formation agree in this, that the cell-nuclei play a very important part, and appear to be the proper centres of development (Bildungspunkte) of the young cells."—*Lc.* p. 19.

2. *The cell is propagated by division of its nucleus.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"The first thing to be observed in this case" (the development of cells within other cells), "in the parent cell, is a metamorphosis of its nucleus, which grows, acquires two nucleoli, becomes elongated, and divides into two."—*Lc.* p. 22.

3. *This division of the nucleus is initiated by division of the nucleolus.*—Phil. Trans. 1840.

Compare with the following by Kölliker in 1853:—"Cell-nuclei are produced endogenously in nuclei, or by their division under the influence of a nucleolus, which also divides."—*Lc.* p. 33.

4. *Fibre is originally flat and marked with striæ, and it has originally a crenate edge.*—Phil. Trans. 1842. More than two hundred drawings from nature.

Compare with Quekett's fig. 23, of a moniliform fungus occupying many of the cells, and threading its way between others, in diseased potato. Compare also with his fig. 127 *b.* from an enchondroma. In the latter his "concentric laminated deposit" appears to consist of my flat striated fibre. See, besides, Quekett's fig. 103 *b.* of striated fibres from the ligamentum nuchæ of the giraffe. He mentions having seen striated fibres also in the rhinoceros and the sheep, and even in arteries. (Among my figures will be found one of an artery from the pia mater with two strata of striated fibres, the inner stratum longitudinal and the outer stratum being a spiral fibre transversely crossing these. Phil. Trans. 1842. Plate VI., fig. 16.)

Compare with the following remark of Kölliker:—"The margins of the elastic fibres are in general quite rectilinear, but in some rare cases appear to be notched." (*Lc.* p. 61.) Kölliker's notches show what the original structure had been, just as the toothed fibres of the crystalline lens according to my observations are the remains of the structure of which they were originally composed. Phil. Trans. 1842, p. 104. Though in the lens we have a permanent,

but probably in the elastic "notched" fibres only a transition, state.

5. *Fibre shows a constant tendency to lose its striation, and become rectilinear.*—Phil. Trans. 1842, p. 92.

Compare with Quekett's fig. 18, of "elongated cells" of *boletus igniarius*, and with his description of fibres of the shell-membrane of the bird's egg. (*Lc.* p. 123.) (This shell-membrane was mentioned by me among the parts in which I had found fibres presenting the original striation, (Phil. Trans. 1842, p. 91), which seems to have altogether disappeared in those seen by Professor Quekett). Farther, compare in Quekett's fig. 105, of connective tissue the smooth fibres *e*, and the striated fibres *a*, with my fact, No. 5, now mentioned; that fibre shows a constant tendency to lose its striation. For *a* in Quekett's fig. 105 exhibits a very early, and *e* a later stage.

Compare with Kölliker's fig. 28, of fibres of connective tissue exhibiting "the complete absence of striation," a state as I have said, which fibre shows a constant tendency to assume. Kölliker, however, had no idea of such a change, for he mentions this complete absence of striation as among the peculiarities distinguishing the fibrils of connective tissue from "their nearest allies, the finest elastic fibres and muscular fibrils." (*Lc.* p. 70.) Future research will, I have no doubt, convince this observer that originally there is no peculiarity in this respect—the fibrils of all tissues presenting originally a like striation.

6. *In some cells the nucleus is seen giving off fibre.*—Phil. Trans. Plate X., figs. 133 to 136 from cartilage of the ear, and in Plate V., many figures of cells in coagulating blood.

Compare with Quekett's fig. 6, from *opuntia vulgaris*, and with his figs. 124 and 127 *b*, from an enchondroma.

7. *Sometimes the entire nucleus becomes a ring of fibre.*—Phil. Trans. 1842. Plates V., VI., figs. 4 and 17.

Compare with Quekett's fig. 6, of cells with thickened fibres from *opuntia vulgaris*, and with his fig. 84 A. The quoit or ring-shaped bodies there seen, "affording evidence of concentric deposit," appear to be altered nuclei of cells.

8. *Sometimes the entire nucleus passes into a coil of fibre.*—Phil. Trans. 1842. Plates V., VI., XI., about fifty drawings of nuclei which have become coils of fibre.

Compare with Quekett's fig. 6, of cells from *opuntia vulgaris*, and with his fig. 84 A. The spirals there seen I apprehend to have arisen from the union of many coils, each coil an altered nucleus.

9. *Sometimes fibre given off by the nucleus of a cell is met with, simply deposited upon the inner surface of the cell wall.*—Phil. Trans. 1842. Plate V., figs. 8 to 12.

Compare with Quekett's fig. 84 A, of a short spiral vessel. And what a display of fibre, given off, as I believe, by a nucleus, is seen in his drawings of fibro-cellular tissue, figs. 61 and 62. It would

seem that here, as the nucleus gives off fibre, what is left of the nucleus continues parietal to the last, and that it entirely exhausts itself in the formation of one long spiral fibre, the end of which (see the figure) denotes the position of what was the nucleolus.

10. *Fibre given off by the nucleus of the cell sometimes thickens the cell-wall.*—Plate X., figs. 133, 134, 136, from cartilage of the ear.—Phil. Trans. 1842.

Compare with Quekett's figs. 124 and 127 *b*, from an enchondroma.

Compare with many figures given by Kölliker. For instance, with that of the cells of cartilage from the epiglottis, fig. 21; with that of cells of the ligamentous discs of the bodies of the vertebræ, fig. 122, described as representing "*concentrically thickened walls and shrunken nuclei*"; with that of cells of costal cartilage, fig. 124 *c*; and with that of cells in rachitic bone, fig. 132.

When fibre is simply deposited upon the inner surface of the cell-wall, it represents, as it were, a commencement of the thickening.

Compare this remark, and my figures of 1842, with the following by Schacht in 1853:—"The increase in thickness of the cell appears to arise from the deposit of solid substance in the interior, upon the original cellulose of the cell-wall. This deposit of new matter frequently appears to take place in the form of a spiral; in very young wood-cells, for instance, in the youngest cells of a fresh twig of *pinus abies*, a most delicate spiral band may be observed in spring and in summer; in the older cells it can hardly be perceived. The band which is seen in spiral cellular tissue, the markings in the thickening layers of the liber-cells of *vinca minor*, the arrangement of those spots, the thickness of which is less than that of the rest of the cell, and which occur in the thickening substance of the wood-cells of *caryota urens* and *hernardia sonora*, as well as the disposition of the slit-shaped pores of many wood-cells (those of *cycas*, for example), all afford arguments in favour of the thickening substance being deposited in the form of a spiral."¹

Farther, compare these remarks by Schacht in 1853, with the following by myself, published in the Phil. Trans. for 1842, concerning some appearances which I had seen in plants:—"These spirals appeared to interlace with one another; and, by their close contact (fig. 90), to produce the appearance of transverse and elliptical 'pores' or 'dots.' The apparent 'dots' or 'pores' I believe were no other than spaces between the winds of spirals, contained within a tube. Now, spirals *interlacing* in the above way (figs. 87 88, 90) must, by a longitudinal succession of their winds, produce *septa* (fig. 90 *a*) in the containing tube. I cannot help believing that these observations will assist to solve the still undecided ques-

¹ "The Microscope in its special application to Vegetable Anatomy and Physiology. By Dr Hermann Schacht. Translated by F. Currey, Esq., M.A. London: Highley. 1853. Pp. 33, 34.

tion as to the structure of the 'dotted duct.'—Phil. Trans. 1842. P. 108, § 83; plate VIII., figs. 87 to 90.

The "manifest lamination in the thickening substance," mentioned by Schacht as seen in almost all cells of great thickness—for example, in many wood-cells—I apprehend to arise, either from the flat form of fibre, or from the splitting up of this into fibrils, which then adhere or coalesce so as to produce the said lamination.

11. *Fibre is sometimes seen entering into the formation of membrane.*—Phil. Trans. 1842. Plate VIII., fig. 69, where a spiral fibre is seen to have undergone division in order to form the sarcolemma. See also Phil. Trans. 1842. Pp. 92, 62.

Compare with Quekett's fig. 87, of hairs from the fruit of *cycas revoluta* unrolling spirally. And see his remark, illustrated by that figure:—"While speaking of fibres unrolling spirally, I take the opportunity of showing a fact I have lately made out, that even membrane itself may occasionally be met with, that will tear and unroll spirally."—*Lc.* p. 100.

In 1842, I stated that spirals constantly exhibited a tendency to pass into membrane, gave drawings of cells, the walls of which were made up of spiral fibres, and, as just mentioned, showed spiral fibres to divide and coalesce, and thus produce the sarcolemma.

Compare also my fact, No. 11, with the following remarks by Kölliker on elastic tissue:—"A modification of this fibrous elastic network is formed by the elastic membranes, in which the fibres are so closely interwoven, that a connected membrane arises, which in the most extreme cases no longer exhibits any indication of its previous nature, and appears as a perfectly homogeneous membrane with smaller gaps (the fenestrated membrane of Henle)."—*Lc.* p. 61.

"The periosteum of the primarily cartilaginous bones" is described by Kölliker as "consisting, as early as at the fifth month, of common connective tissue and fine elastic filaments."—*Lc.* p. 361.

On "Elementary Processes in the layers formed from the Periosteum," Kölliker remarks, that "The matrix of the bone arises from its fibrous tissue by the simple uniform deposition of calcareous salts."—*Lc.* p. 362.

Cartilage affords a striking instance of formation out of fibre. The softer central substance of the intervertebral ligaments may serve as an example. On this subject, it is said by Kölliker: "The more central portions of this fibrous substance gradually pass into a thin, hard, yellowish lamella of true cartilage."—*Lc.* p. 315.

In 1852, Agardh showed the cell-walls in *conferva melagonium*, *griffithsia equisetifolia*, and *polysiphonia complanata*, to consist of fibre.¹

12. *The axile fibre of nerves has a structure, sometimes giving it transverse striation and a crenate edge.*—Phil. Trans. 1842. Plate VIII.; figs. 80, 81, 85, 86.

¹ "De cellula vegetabili fibrillis tenuissimis contexta." Lundæ, 1852.

Compare with Kölliker's remark, that this axile fibre,¹ in some instances, is "*striated*" (*lc.* p. 392), and sometimes presents a "jagged border" (*lc.* p. 396). If my drawings be referred to, the *structure* will be seen that produces striæ and a jagged border—my crenate edge.

13. *The "white substance" of nerves has a structure giving it originally transverse striation.*—Phil. Trans. 1842. Plates VIII., IX.; figs. 72, 77, 80, 81, 82, 99, 102.

Compare with the following from Kölliker: "The formation of the fibres in the cord and brain is extremely difficult of investigation, and is best studied with the aid of chromic acid. In the human embryo, I find, as early as the end of the second month, the commencement of the formation of the tubules in question, the white substance being distinctly finely *striated*."—*Lc.* p. 491.

The "distinctly finely striated" appearance here described by Kölliker, as seen in the white substance, I think will be found to have been explained by myself in the Phil. Trans. for 1842, in these words: "The filaments, which I believe to constitute the white substance of nervous fibre, are often seen to be, not longitudinal, but curiously interlaced (fig. 102γ), as though each filament ran in a spiral direction" (p. 97). In the figure now mentioned, it will be seen to be not the structure but the *direction* of the filaments that produces striæ.

Other figures besides the one referred to in the last paragraph were given by myself in the Phil. Trans. for 1842. And as they serve to explain the striæ seen in the white substance by Kölliker, as well as the occurrence of *varicosities*, I will trespass upon the reader's patience by quoting from my own writings a few lines more. "The frequent interlacing (fig. 102γ), and apparently spiral direction of the filaments in nerves, now referred to, seem the more deserving of attention, from my having found spirally directed filaments, so very general in the retina, brain, and spinal chord (figs. 17 to 22, 72, 77, 80 to 82, 85, 99). Farther, I have noticed spirally directed filaments, on being broken, to recoil, (figs. 80, 81). Such a change in the white substance taking place within a tube, *might produce varicosities*, and those minute isolated masses hitherto called granular, by which it has hitherto been usual to distinguish nerve."—Phil. Trans. 1842. P. 97.

My figures now referred to seem also to explain the granular appearance of the "sheath of nerve;" for instance, in Kölliker's fig. 158 b, which granular appearance I conceive to be referrible to more or less of a recoil of spirals. (See the preceding paragraph).

14. *A mother-cell is sometimes seen divided into two compartments by two daughter-cells, whose walls are composed of fibre, and where in contact with one another form a thick septum.*—Phil. Trans. 1842. Plate XI.; fig. 150.

¹ "Central or axis-fibre of the nerve-tube," "*primitive band*" of Remak, "cylinder axis" of Purkinje.

Compare with Kölliker's figs. 123, 124, 132.

I think that this comparison of some of the drawings and remarks of Professors Quekett and Kölliker, with my own, shows a confirmation of the facts just mentioned, as having been published by myself in 1840 and 1842.

To Quekett's statement, then, that the mode of formation of the spiral fibre in spiral vessels of plants is "by a secondary deposit in a spiral direction on the internal surface of the cell-wall" (*lc.* p. 95), I have to add that this deposit is effected *by the giving off of fibre in the form of a ring or coil by the nucleus*. And as "all the forms of vascular tissue are referrible to the same origin, this simple cell with its secondary interrupted deposit" (*lc.* p. 95), it follows that all these forms of vascular tissue are referrible to altered *nuclei*.

The term "cell" is often used when nothing is seen but a nucleus, the membrane of the cell having more or less completely disappeared.

This was noticed in cartilage of the cuttle-fish by Quekett, for he says, "Around some of these cells there is a faint trace of cell-wall, and it becomes a question whether many of them should not be considered as altered nuclei." (*Lc.* p. 139). And in fig. 127, he demonstrates the truth of Todd and Bowman's view, that the lacunæ of bone are developed from the nuclei of cartilage cells.

What Kölliker terms bone cavities or "cells" and canaliculi—*lacunæ et canaliculi ossium* (*lc.* p. 300, figs. 117, 119)—are therefore not cells but *nuclei*, and processes sent out by these.

Now, though in vegetables the cell-wall remains, yet just as the lacunæ of bone are developed from nuclei, so it appears to me is the *sclerogen* of plants (Quekett, *lc.* figs. 44 to 48), notwithstanding that anastomosis of the radiating tubes or canaliculi is here prevented by persistence of the cell-wall.

Farther, *Raphides* in their several crystal forms, whether acicular, conglomerate, stellate, octohedral, prismatic, etc., (Quekett, *lc.* figs. 27 to 33), *Phytozoa* (Quekett, *lc.* figs. 58, 59), the crystals of *Margarine* and *Spermaceti* (Quekett, *lc.* figs. 141, 142), and a host of other things are, I think, formed by the nucleus of the cell. Even *Silica* itself, which Quekett is no doubt right in considering a secretion (*lc.* p. 56), analogy seems to warrant us in believing to be secreted by the nucleus alone.

On crystals of oxalate of lime, familiar to all who are in the habit of examining the deposits from urine, Quekett remarks that in this fluid he has found them "contained in the interior of cells, so that, both in the animal and vegetable kingdoms, we have crystals of oxalate of lime as a product of cell secretion." (*Lc.* p. 44). I would only add, that such crystals, as well as all others in both kingdoms, are no doubt formed by the *nucleus* or represent the *nucleus* in altered forms.

It is remarked by Quekett regarding the size of "the bone cells" or lacunæ: "I have ascertained that they bear a direct relative proportion to that of the blood-corpuscles." (*Lc.* p. 174). This is a

confirmation of a fact which I had published long before as the result of wide research: that the larger the blood-corpuscles of the animal, the larger are the elements of the tissues.

Nowhere have my views on the physiological importance of the nucleus of the cell received more ample confirmation than in the article "Semen" of Todd's Cyclopædia, by R. Wagner and R. Leuckart, who constantly refer to Kölliker also as advocating an opinion coinciding with their own. Their figures present a vast number of instances, including both ends of the animal kingdom, in which a single spermatozoon is represented coiled like a young animal, having evidently arisen out of the nucleus of its cell. Farther, it appears to me from some of the same figures that the essential part, *i.e.*, the head-like end or body of the spermatozoon, around which the tail is coiled in its cell, arises out of the *nucleolus*. It is true they arrive at the conclusion that the mode of development is three-fold, *viz.*, "1st, The cell membrane and nucleus of the formative vesicles convert themselves immediately into the spermatozoon. 2d, The nucleus of the formative vesicles alone metamorphoses itself into the spermatozoon. 3d, A new formation, which takes place in the interior of the nucleus (or immediately in the cell cavity), performs the functions of a spermatozoon." But if they saw some spermatozoa to arise, "simply and immediately from a metamorphosis of nuclei," (*lc.* p. 501), these observers may be quite sure that the part performed by the "cell-membrane" (1.) is very subordinate; and as to the "cell-cavity" (3), their own remarks show it to be by no means certain that the developing cells or vesicles are not "in all cases nuclei."—*Lc.* p. 499.

That these "developing cells" or "vesicles" are really nuclei in mammalia, was shown by myself from observations on the rabbit as long since as in 1841. Those observations were incidental, but may be referred to here. It would seem that in this animal there exists first a large cell $100''$ — $80''$,—that the nucleus of this large cell divides into many parts which are smaller nuclei,—that each of these smaller nuclei has its own investment, and that they are the "developing cells" of authors which give origin to the spermatozoa.¹

The products of the testis have, it may be presumed, their equivalents in the ovary; and it happens that the very plate containing my drawings now referred to, of the first elements of the spermatozoa of a mammal, contains also drawings of the first elements of the ova of a singing bird.

Equivalent Products of the Testis and Ovarium.

From the plate now referred to we may, I think, draw something like the following conclusions, *viz.*, that

¹ Phil. Trans. 1841, Plate XXV. fig. 161 $\frac{1}{2}$. It may be deserving of remark that these observations of mine were made on the elements of spermatozoa found—not in the male, but in the female—in the vagina of a rabbit killed two hours *post coitum*.

In the Testis.

A large cell, $\frac{1}{100}''$ — $\frac{1}{80}''$, A, fills with nuclei, B, the inner part of each of which forms a spermatozoon, C, while the outer part becomes the "developing cell" of authors, D. The head of the spermatozoon is a nucleus, E, which subsequently exhibits a nucleolus, F, the fecundating element.

In the Ovarium.

A large cell about the same size, A, fills with nuclei, B, the inner part of each of which is a young ovum, C, while the outer part becomes my ovisac,¹ D. The germ spot in the ovum is a nucleus, E, which subsequently exhibits a nucleolus, F, the part fecundated.

I have just mentioned an ample confirmation by R. Wagner and R. Leuckart, of my views on the physiological importance of the nucleus of the cell. The following remark is by the same distinguished authors: "It might not be without interest to reflect upon the important part which the nucleus plays in the formation of the spermatozoa, since it is an element which is usually only important for the formation of cells, and does not participate in their subsequent metamorphoses."—*Lc.* p. 501.

This remark shows the existence still of views regarding the nucleus, quite unworthy of it. But when the authors add, concerning the said remark, "This is at least the rule; a rule, however, by no means without exception" (*lc.* p. 501), they give evidence of having seen the nucleus doing something more than simply entering into the formation of the cell. They further say: "We already know that in many cases the nucleus is important for the development of certain parts" (*lc.* p. 501), which they mention; and they add, "We know that the nucleus . . . even converts itself . . . into peculiar fibrous formations—into the so-called nuclear fibres (Kernfasern).² Still more remarkable is the metamorphosis of the nucleus in the development of the so-called prickles or nettle organs—those interesting microscopical formations, which are so frequently imbedded in the skin of the lower animals (*e.g.* polyps and medusæ), and which present so great a similarity to certain forms of the seminal fibres, that they were even taken for such by one of us on their first discovery. Kölliker's observations, as well as our own more recent ones, instituted upon hydra, convince us beyond doubt that it is the nuclei of cells which gradually metamorphose themselves into the capsules of the prickles, and which ultimately become free through the dissolution of the cell membrane surrounding them. . . . On examining the external coverings of hydra, we shall readily be enabled to convince ourselves of the formation of these organs. The most different stages of development may here be seen; viz., developed prickles, either free or still enclosed by a cell membrane, from which the organ itself, and especially the fibre enclosed in its interior, recedes more and more, until it finally appears as a mere simple nucleus."—*Lc.* p. 502.

¹ The ovisac acquires a vascular covering, and there is thus formed a "Graafian follicle."—*Phil. Trans.* 1838.

² Vid. Henle (*Allgemein. Anat.* s. 193) and Zwicky (*Metamorphose der Thrombus*).

Compare this with drawings of mine published many years before, viz., in 1842, of fibre given off by the nucleus of the cell.¹

(*To be concluded in next No.*)

ARTICLE III.—*Pelvic Abscess and the Fascia of the Pelvis in the Female.* By WILLIAM O. PRIESTLEY, M.D., M.R.C.S.L., late Vice-President of the Parisian Medical Society.

(*Continued from page 430.*)

Belonging to the system of fasciæ, situated above the superior pelvic fascia, is a disposition of laminæ, more especially in relation with the rectum, which must have an important influence, when abscess occurs in the posterior divisions of the pelvic cavity.

Passing over the anterior aspect of the sacrum, and loosely attached to it by cellular tissue, is a fascial layer, which, when dissected forwards, seems adherent, by its lateral borders, on each side, to the cellular sheaths of the internal iliac arteries. This layer, which in its form resembles a wedge, with the point directed downwards, is distinct from the pyriformis fascia, and superiorly is continuous with the sub-peritoneal cellular tissue, enveloping the common iliac vessels; inferiorly it unites with the fasciæ covering over the coccygeal muscles as they approach the rectum, and it is by the convergence of the two lines of attachment of the coccygeal portion of the superior pelvic fascia, that the inferior extremity of the lamina covering the sacrum, is cut obliquely to the form of a wedge. In relation with the anterior aspect of the layer just described, are the rectum, the peritoneum, and the vessels and cellular tissue forming the meso-rectum.

Two other laminæ, which enclose the rectum between them, take their origin from this anterior sacral aponeurosis. Arising as high as the promontory of the sacrum on each side, from a line corresponding to the sacral foraminæ, they curve forwards around the rectum, and become attached by their anterior margin to the upper part of the vagina and posterior aspect of the neck of the uterus. The superior border of each dissepiment forms a curve downwards from the sacral promontory to the neck of the uterus, and as the peritoneum passes over this border, it is very firmly attached to it, and gives rise to the appearance of falciform peritoneal folds passing forward on each side of the rectum. The inferior border of each is united to the superior pelvic fascia as it passes to the rectum. These laminæ possess considerable strength, but at the same time contain much fatty and elastic tissue in their structure, except in the upper border, which is almost entirely white fibrous tissue, and very

¹ Phil. Trans. 1842. Plate V., figs. 9 to 12; Plate X., figs. 133 to 136, etc.

unyielding. The rectum is united to their inner surface by loose areolar tissue, which permits the bowel to glide between them ; and each, by its external surface, assists to form a triangular space, which has Jarjavay's anterior aponeurosis of the broad ligament for its anterior boundary, and in which are contained the ramifications of the internal iliac vessels with loose cellular tissue.

In the male subject rudiments may be found of these falciform folds passing round the rectum. In one instance the author saw them reach as high as in the female subject, and the peritoneum was thus raised up in such a manner as to form a deep fossa on each side the rectum. The upper borders of the laminæ, on being dissected forwards, were found to terminate in the prostatic portion of the pelvic fascia.

The rectum in its upper two-thirds is thus enclosed in a complete sheath, formed behind by the fascial lamina covering the sacrum, and laterally and in front by the folds which pass forwards from this layer.

This description is not in unison with the one given by M. Jarjavay. This anatomist describes a posterior aponeurosis of the broad ligament, but regarded it as a prolongation inwards of the fascia covering the pyriformis muscle and sacral plexus of nerves. In other words, he regarded this pyriformis fascia as a portion of the fascia of the broad ligament, and not belonging to the pyriformis muscle. He believed the fibrous tissue binding together the cords of the sacral plexus to be the proper fascial covering of the pyriformis. This view seems to be incorrect, as the layer usually described as the pyriformis fascia in our British anatomical works most distinctly terminates at the anterior boundary of the great sacro-sciatic foramen, and the folds enclosing the rectum lie on a plane considerably nearer the median line of the pelvis.

The cellular tissue in the vicinity of the rectum is comparatively a frequent seat of inflammatory tumours. The proximity to the cervix of the uterus renders it liable to participate in any inflammatory state affecting that organ. When once commenced, the engorgement may pass round the rectum, by the continuity of the loose cellular tissue surrounding the bowel. In most instances, however, the tumour is limited to the space between the uterus and rectum, or to one or other side of the rectum ; and as the folds enclosing the bowel, as before stated, are not very decidedly aponeurotic, but contain a considerable quantity of fatty tissue, they will be liable themselves, by taking on inflammatory action, to add to the bulk of a lateral phlegmon, and these may again affect the cellular tissue on their outer surface, which is woven amongst the branches of the internal iliac vessels.

The vessels of the meso-rectum entering the upper part of the compartment enclosing the rectum, and being surrounded by very lax cellular tissue, give rise to an easy mode of communication be-

tween the interior of the pelvis and the *back part of the left iliac fossa*.

M. Grisolles, in his essay on "Phlegmons des Fosses Iliques," mentions that the cellular tissue of the meso-rectum must favour the entrance of pus into the cavity of the pelvis, from the left iliac fossa.¹

It will be apparent, therefore, that if pus finds its way from an abscess in the left iliac fossa by this route, that the rectum will be the chief organ involved. The purulent fluid will be able to pass entirely round the bowel, while only the posterior aspect of the vagina will be bathed by it, and this with a fascial lamina intervening. In such cases the rectum is much the more likely organ to be perforated.

The peritoneum lining the pelvic cavity, is much thicker than that covering the intestines or other abdominal viscera. It is so reinforced by strong fibrous tissue in this region, that it may well act the part of a fascia, in offering resistance to, or directing the contents of a pelvic abscess. It is everywhere closely adherent to the muscular structure of the uterus, except on the narrow lateral margins of that organ; and by this firm adhesion of the peritoneum, and the thickness of the uterine walls, the rarity of cases where the abscess bursts into the uterine cavity may be accounted for.

Almost equally rare, however, are instances of pelvic abscess opening into the bladder. This is to be explained, not from the close adhesion of the thickened peritoneum, as in the case of the uterus, but probably from the loose attachments of the bladder with the surrounding parts, which are necessary on account of the varying degrees of distension of that organ.

A space of considerable extent exists between the superior pelvic fascia below, which forms the true ligaments of the bladder, and the peritoneum above, constituting the false ligaments of that organ. This space is filled by a large quantity of cellular tissue, which is necessary for the ascent and descent of the bladder. This anatomical arrangement would permit the bladder to be pushed aside by an abscess forming in this locality.

Few cases are on record where the uterus has been perforated, and the instances in which pelvic abscesses have opened into the bladder are by no means frequent, although somewhat more so than into the uterus. Examples of the latter mode of egress are given by M.M. Lisfranc and Emery (Rev. Médicale, 1827); Mr Hawkins (Lond. Med. Gazette, 1832); Dr Johnson (Br. and For. Med. Rev., 1836); Dr Battersby (Dub. Quart. Journ., May 1847); and by Marchal de Calvi, in his thesis before quoted.

The examples of opening effected through the walls of the uterus are recorded by M. Dance (Obs. xiv.²), Wainwright (Br. and For.

¹ Archives Gen. de Médecine, 1839.

² Mémoire sur quelques engorgements inflammatoires, que se développent dans la fosse iliaque droit. 1827.

Med. Rev. for July 1841). Although the opening of an abscess into the cavity of the peritoneum is a most fatal result, yet it is by no means so frequent an occurrence as might be expected from its anatomical relations. The peritoneal membrane seems to offer considerable resistance to the invasion of its cavity by purulent fluids. In the pelvis this is in some degree accounted for by its strength; from the addition of white fibrous tissue on its attached surface; but besides this, the readiness with which a lymph exudation can be thrown out on its free surface, when involved in inflammation, must be an important means of defence against so disastrous an issue.

An abscess situated originally in the cellular tissue of the anterior portion of the pelvic cavity, or on the upper surface of the iliac fascia, in its progress, often extends upwards towards the umbilicus, and may thus be felt through the abdominal parietes. M. Bérard, jun., in a communication read to the Société Anatomique in Paris, mentions an instance where the contents of an iliac abscess, following the sub-peritoneal cellular tissue of the hypogastric region, passed from one side to the opposite, rising to a higher level on the opposite side before perforating the abdominal parietes, than the original seat of the abscess in the iliac fossa. In the wards of M. Nonat, in the Hôpital de la Pitié, a patient who had been confined two or three weeks before, was attacked with pelvic cellulitis. An inflammatory swelling was found occupying the whole of the right half of the pelvis, and externally it could be felt reaching nearly as high as the umbilicus, beneath the abdominal muscles. Subsequently an opening was effected through the integuments, on a level with the umbilicus, and a large quantity of pus escaped. The patient only remained a day or two after this in the hospital, and the termination of the case was not known; but after the bursting of the abscess pus could readily be made to flow from the aperture by pressing on the pelvic tumour through the vagina.

The peculiar track followed by the abscess in such cases as these, is no doubt owing to anatomical arrangements.

The sub-serous cellular tissue exists in considerable quantity at the lower part of the abdomen, and is there included between the transversalis fascia in front, and the peritoneum behind, both offering considerable resistance, the contents of an abscess pass upwards between them until the transversalis fascia becomes thin and feeble, when the pus enters among the muscles, and after passing, perhaps, circuitously between the layers, it reaches the surface.

Below the superior pelvic fascia, but scarcely belonging to the perineal region, is a compartment containing the obturator internus muscle. This compartment is open towards the trochanteric fossa, where the tendon of the muscle emerges from the pelvis. It forms a portion of the soft parts bounding the pelvic cavity, and we should naturally expect its contents occasionally to be inflamed from continued pressure of the foetal head or other injury.

The obturator internus muscle is enclosed in a sheath, partly osse-

ous and partly fascial. The bony walls of the pelvis, with the thyroid membrane, constitute the anterior boundary, while the posterior one is formed by fasciæ having the following attachments. Taking an origin around the upper and inner border of the thyroid foramen, it passes outwards to the upper margin of the great sacro-sciatic notch. Here, descending along the ilio-sciatic line above mentioned, it encounters the spine of the ischium, and from thence directed outwards and downwards, it is attached to the inner border of the anterior half of the great sciatic ligament, and the inner margin of the tuberosity of the ischium. The upper portion of the fascia, to which these limits are assigned, as far downwards as the pubo-ischiatic line, has been before described as a portion of the superior pelvic fascia. Below this line, however, the muscle is enclosed in an expansion more particularly its own, the lamina here receiving the appellation of "obturator fascia," as it descends to form the outer wall of the ischio-rectal fossa. It will be evident that should abscess occur in the muscular structure or cellular tissue of this closed compartment, no means of egress is offered except along the course of the tendon, as it emerges from the pelvis.

I have once, in the dead subject, but in a male, seen an instance where a collection of matter having accumulated in the neighbourhood of the great trochanter, it was traced inside the pelvis, passing beneath the fascia, binding down the tendon of the obturator internus, as it emerged from the lesser sciatic foramen; the muscular fibres and cellular tissue of the obturator compartment being in a great measure disorganised.

Marchal de Calvi, in his thesis before alluded to, mentions a case in which abscess having formed in the obturator compartment of fascia, as the result of caries of the acetabulum, the pus passed into iliac fossa, and from thence into the thigh, because no attachment of the fasciæ existed at the brim of the pelvis.

Such cases must be very rare, however, and the general disposition of a purulent formation will be to pass into the gluteal region.

The perineum is conveniently divided by anatomists into *perineal region proper*, situated anteriorly, and an *anal* portion posteriorly.

The perineal portion is triangular in form, bounded laterally by the rami of the pubis and ischium, and posteriorly by a line drawn in front of the anus from one ischial tuberosity to the other. In this anterior division are found the openings of the vagina and bladder, with their peculiar muscles, vessels, and nerves, enclosed in cellular tissue.

In the posterior division of the perineum are found the termination of the rectum, much loose fatty tissue, some muscles, and nerves.

The first true aponeurotic layer met with in the dissection of the anterior half of the perineum is the deep lamina of the superficial fascia. Attached on each side to the rami of the pubis and ischium, it passes inwards to the labia pudenda and clitoris. Behind the transverse muscles of the perineum it is united to the posterior mar-

gin of the triangular ligament. Anteriorly, beneath the pubic arch, it is open in front, and allows the passage of air or fluid forward to the cellular tissue of the abdomen. The areolar tissue behind this deep layer of the superficial fascia is continuous below the convergence of the crura of the clitoris, from one side to the other, as the handle of a scalpel may readily be pushed across. Above the position of the clitoris, however, the cellular tissue of the Mons Veneris is divided in the median line by a strong septum which is attached behind or above to the ligamentous fibres uniting the symphysis of the pubis, and anteriorly is united to the suspensory ligament of the clitoris. This vertical septum seems of such a nature as to prevent an abscess formed in one labium passing along the cellular tissue of the Mons Veneris to reach the labium of the opposite side.

Contained between the deep layer of superficial fascia and the anterior layer of the triangular ligament, which is the next fascial lamina reached in the dissection, are the crura of the clitoris, with the two muscles attached to them, which answer in the male to the ischio-cavernous muscles, the transverse perinei muscles, and the superficial vessels of this part of the perineum, all of them receiving fibrous investments from the deep layer of superficial fascia, and united by looser cellular tissue.

On removing the parts just described the anterior layer of the triangular ligament becomes apparent. It has a firm, white, fibrous structure, with similar attachments to those of the deep lamina of the superficial fascia, with which it is united at its base by the reflection over the transverse perinei muscles before described. It fills up the interval between the diverging rami of the pubis and ischium, and transmits the urethra, vagina, with the fibrous coats of which it is firmly united. The pudic vessels and nerves also pierce the laminæ to supply the clitoris and labia; and the ducts of the Cowper's glands to open into the lower part of the vagina.

The deep layer of the triangular ligament is still farther behind, and comes in close contact in some situations with the pubic portion of the superior fascia of the pelvis. To its posterior border the deep layer of superficial fascia is united, as in the case of the anterior layer, and passes from thence to the visceral superior pelvic fascia. In this way a sort of crescentic partition is formed, the concavity looking towards the ischio-rectal fossa. The compartment of levator ani is thus completely separated from the anterior half of the perineum.

Between the two layers of the triangular ligament are the vaginal and urethral canals, the compressor muscles of the urethra, and the glands of Cowper. On each side, under cover of the osseous rami, are the pudic artery and nerve, contained in a tube of fascia which passes backwards on the outer boundary of the ischio-rectal fossa, beneath the tendon of the obturator internus muscle, and enters the pelvic cavity by the great sciatic foramen.

The cellular tissue existing between each layer of fascia in the

pubic portion of the perineum may become inflamed by various accidents; should suppuration be the result, the strength of the laminae are likely to prevent the pus passing directly forward to the cutaneous surface, and the opening will rather be effected through the mucous membrane of the lower part of the vagina or labium.

I lately saw, with Professor Simpson, a patient in whom an abscess burst very low down in the vagina. The suppuration was diagnosed to have occurred in one of Cowper's glands, and passed in this direction to discharge its contents.

If a purulent fluid gained an entrance into the sheath containing the pudic vessels, it might easily find its way to the posterior half of the perineum; the union of the posterior margins of the respective layers before described does not permit any other means of communication in that direction.

ARTICLE IV.—*Case of Popliteal Aneurism treated by Ligature of the Superficial Femoral Artery.* By JAMES ALEXANDER, Esq., Surgeon, Wooler.

ROBINA OGILVIE, æt. 36, September 20th, 1853. There is a large aneurism on the inside of the right thigh, a short distance above the knee. The tumour is fully as large as the closed fist, and pulsates freely. The pulsation is altogether and at once arrested by compressing the artery at the groin, and recommences immediately on removal of the pressure. There is a moderately loud blowing sound in the tumour. The tumour bulges on the inside of the thigh, and also towards the popliteal space, as if pushing the internal hamstrings before it. The popliteal space proper is not occupied by the tumour, but is merely encroached on at the upper and inner side. The chief bulging of the tumour is on the inside of the thigh, the summit of it lying at a distance of two inches from opposite the upper margin of the patella, and ten inches from the pubes. The circumference of the thigh at the tumour is fifteen inches and a half; that of the other thigh at the same part being thirteen.

The heart and aorta appear to be healthy.

Previous history.—She can assign no cause for the swelling. She noticed first, about two months ago, when engaged in field work, stiffness and pain in the part, but only three weeks ago felt a distinct swelling, with beating in it and severe pain. She left Edinburgh about a month ago, and has walked here for the harvest. For the last fortnight she has been confined to bed, or the house, on account of the swelling and the pain.

She has now severe and constant pain, chiefly opposite to the internal condyle, and also between this and the tumour. There is also pain down the leg to the ankle, on the outside. There is no œdema of the foot or ankle, although on this side these parts seem somewhat thicker than on the left side.

About twelve years ago she was a patient in the Edinburgh Infirmary, with what seems to have been paralysis of the right portio dura, and afterwards general paralysis.

Sept. 20th.—To-day I placed a ligature round the superficial femoral artery. The artery was tied four inches below Poupart's ligament. The operation did not present any unusual difficulties. The fascia lata was freely opened, and on turning out the edge of the sartorius, the vessel was seen through its sheath. The latter was pinched up with the forceps and opened to a limited extent, and the cellular tissue, on each side of the artery scratched aside with the point

of the knife and the aneurism needle, so as to denude only a very small part of the artery, and to leave undisturbed its surrounding connections. The vein was not seen at this or any other time. The needle was then readily passed round the artery. There was scarcely any bleeding, and none at all in the latter stage of the operation. On the ligature being tied the pulsation in the aneurism immediately ceased. The edges of the wound were brought together by three stitches and two light compresses, and a few turns of a bandage applied. The patient was placed completely under the influence of chloroform, and on awaking up she asked for her pipe, wholly unconscious of what had taken place.

10 P.M.—Continued free from pain till 5 P.M., since which time she has complained of severe pain about the tumour and knee, and down the outside of the leg to the ankle. To have an opiate. Tumour firm and solid; no pulsation.

Sept. 21st. 7 P.M.—Pain continued through the night till 5 or 6 A.M.; from this time till 5 P.M. she was free from pain. Since 5 P.M. this afternoon the pain has returned, but much less severely, and is not constant. There is no pain about the wound, and at present the whole limb is perfectly free from pain or uneasiness. To have an opiate. The tumour continues firm and solid.

Sept. 24th.—Has continued well since last report. Complains of occasional darting pain on outside of leg and near tumour, but this does not prevent her sleep. Dressed the wound to-day. It seems to be united, except at the very edges. No pulsation in the tumour.

Sept. 26th.—To-day the stitches were removed; the wound is healthy, and seems healed except where the ligature issues from it. The aneurism is firm, and no pulsation; still occasional pain outside the leg, but it is rarely so bad as to interfere with her sleep.

Oct. 1st.—Till now no change occurred deserving notice. This day the ligature was touched, but felt quite firmly attached. Tumour evidently diminished.

Oct. 2d.—Has had a bad night, and suffers from pain in the back; complains of restlessness and the prolonged confinement to bed. Pulse 90, and rather weak. To have an opiate and a glass of wine daily.

Oct. 3d.—Little change. She requires an opiate every night, and the appetite is not quite so good as a week ago. There are occasional pains in the back and limb. The wound is now skinned over, but the ligature is still firmly attached. Tumour gradually diminishing.

Oct. 4th.—Severe pain has been felt in the wound and down the leg through the night. A very slight degree of heaviness is felt near the centre of the wound upon rather deep pressure, which, however, gives great pain.

9 A.M.—About a tea-spoonful of bloody matter escaped from the central stitch, which had opened through the day. Pain considerably relieved.

Oct. 7th.—Discharge continues through the stitch holes and the channel of the ligature; it is not increased in quantity, and is improved in appearance.

12th.—Discharge continues the same; little pain; ligature still firm.

14th.—Discharge diminished.

18th.—Till this day little change has occurred, but alarm was produced this morning by the escape of a small quantity of blood through the openings whence the matter used to come; it soon ceased on the application of cold cloths, leaving only a serous but rather florid oozing, which continued through the 19th, but on the morning of the 20th the ligature was found detached, and from that time the discharge and pain rapidly ceased. She was confined to bed for three days, by which time the wound was perfectly cicatrized; and in a few days she left Wooler for Edinburgh. The aneurism was much diminished in size, having ceased to encroach on the popliteal space altogether, and the portion of the tumour which still remained on the inner part of the thigh was perfectly solid.

I submit this case to the profession, not as containing any thing

new, but simply as a contribution to the statistics of popliteal aneurism treated by operation; a subject of considerable interest at present, from the recent introduction of improved methods of using compression in the cure of this disease. That well regulated compression can, in many cases, cure aneurism may be considered an established fact. I myself have treated one case of traumatic aneurism of the anterior tibial artery by this plan, the particulars of which are recorded in the *Edinburgh Monthly Journal of Medicine* for 1849; but a great deal more must be proved before we are warranted in concluding that the old operation ought to be altogether superseded, or even that compression is a mode of treatment so harmless as in all cases to admit of being tried previous to having recourse to the operation. Judging from my own very limited experience, the annoyance and pain produced by compressing instruments is very considerable, and the probable danger of the proceeding, which have been pointed out by Mr Syme, with all his wonted sagacity and clearness, have not (to say the least of it) been as yet proved to be imaginary, even on the showing of the advocates of compression themselves. At a meeting of the Medical and Surgical Society in London, reference is made by some of the speakers to "many" instances of failure, and to others in which sloughing, obstruction, and very serious engorgement had taken place. I do not know that these cases have been given in detail to the profession; and until they be, and their comparative frequency ascertained, it seems to me a little premature to decide finally on the superiority of the new practice over the old. Besides, from very obvious considerations, it is in the highest degree probable that (in private practice at least) every successful instance of a new mode of treatment will be recorded, and it is equally probable that the unsuccessful cases will not in the first instance, at any rate, be made public. In an accredited and long established operation, there is little or no inducement to publish successful cases, which therefore remain unknown and unavailable for purposes of comparison; and this must be my apology for narrating this case, in doing which, I merely wish to contribute my very humble share towards enabling the profession to arrive at a fair judgment on an important subject. I may mention, that several years ago I operated for popliteal aneurism in another case, the patient, named Fraser, being under-gardener to the late Matthew Culley, Esq., Fowberry. In this case there was no suffering at all from the moment of the operation, which was perfectly successful. The ligature came away on the 18th day.

Part Second.

REVIEWS.

The Diseases of the Heart and the Aorta. By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, etc. Dublin. 8vo. 1854. Pp. 689.

(Concluded from page 356.)

Chapter 6 is occupied with the Treatment of the Organic Diseases of the Heart. It has been too much the custom, says Dr Stokes, with medical writers, when they have to deal with the diseases of an organ such as the heart, to discuss the treatment of each of its affections separately. The objection to this is, that in so doing they not only assume the isolation of diseased conditions, but overlook the *vital* changes continually going on in the organ. The practitioner, however, should keep this truth constantly before him,—that the vital, rather than the mechanical, condition of the heart is to be his guide in practice. “Let him have a competent knowledge of the effects of disease of the heart upon other organs, and of its own sympathetic derangements, and he will scarcely err in the treatment of cardiac affections. He must not only give due weight to the existence of separate phenomena, and to the results of combinations of diseases, but he must study conditions which are beyond the reach of the pathological anatomist, and learn to ignore the existence even of manifest organic alterations. He must go beyond the facts supplied by modern investigation, having an eye to the general rather than the particular, and found his diagnosis, prognosis, and treatment, on the broad basis, which includes not alone the effects, but the causes and special modifications of disease. The vital condition of organs cannot always be inferred from their physical state, nor the influences which act on the entire economy be explained by anatomy.”

At a time when so much attention is paid to the physical diagnosis of disease, these words of caution are wisely offered by our author; and not here only, but on every other fitting occasion, does he endeavour to impress the lesson they teach upon his readers. It cannot, however, be denied, that the treatment of diseases of the heart has become more rational as their diagnosis has become more extended and correct. Pathological anatomy has proved, that in the great mass of these diseases treatment has no power to effect a cure, and that, indeed, many of the morbid states in question are but efforts called into action, for the very purpose, as it were, of counteracting other and pre-existing morbid changes. Thus hypertrophy of the heart, for instance, once a condition which men attempted to cure,

is now, with our better knowledge, admitted—as the rule—to be “a wise provision of nature,” destined for a special object. No man now-a-days attempts to cure hypertrophy by low diet and bleeding; on the contrary, the signs and symptoms of hypertrophy do not preclude the use of stimulants.

Use the lancet rarely, says Dr Stokes. Small local bleedings (by leeches) do good, but must be adopted cautiously. The most useful sedatives are digitalis and hydrocyanic acid.

Even when acute inflammation, pneumonia, peritonitis, etc., supervene, Dr Stokes avoids the use of the lancet.

The general principles of treatment are the same in the two most common forms of weakened heart, viz., dilatation associated with pulmonary and hepatic disease, and fatty degeneration. Reducing measures must be avoided, and attempts made by tonics and stimulants to improve the condition of the blood, and increase the power of the weakened muscular fibres. But in one important particular these two forms differ.

It is impossible to speak too highly of the beneficial effects of mercury in relieving many of the symptoms, and removing the consequences of that form, which is associated with pulmonary and hepatic disease. “Many years’ experience has convinced me that by the use of this remedy we can delay its production, remove the irregular action which assists in causing the disease, and, above all, prolong the patient’s life, and again and again relieve him from dropsy, and from pulmonary and hepatic congestions.” The subjects of this disease are most tolerant of mercury; there need be no timidity in its frequent repetition. Diuretics and digitalis are often also necessary; but the success of diuretics appears to turn upon their being preceded by mercury. Dr Stokes mentions the case of a gentleman who had undergone “thirty distinct courses of mercury.” The beneficial action of mercury, he says, in this affection is known to many practical physicians, but little information can be found on the subject in any of our medical works.

In the treatment of weakness of the heart depending upon its fatty degeneration, still less than in the former case should we adopt depletory measures; the state is a cachectic state, and our efforts must be directed to the general invigoration of the system; depletion may produce present relief to some existing symptoms, just as it does in phthisis, but the benefit is as dearly purchased. We are hardly warranted by any analogy in thinking, as Dr Stokes does, that it is not impossible that a heart in a state of fatty degeneration may be restored to health.

Chapter 7 contains a detailed account of the conditions of the heart in typhus fever. Twelve years ago our author published an account of his researches on this subject; subsequent experience has confirmed the opinions he then held. He here gives a full history of this subject. A pathological state of the heart in typhus has been admitted by Laennec, Louis, and other observers; but we gather

from the stress Dr S. lays upon its importance, that the practical physician, in his treatment, does not sufficiently bear this condition in his mind.

The state in question is softening of the ventricles, especially of the left ventricle, of a non-inflammatory character, and generally met with in cases where no analogous state of the voluntary muscles can be observed. It is not caused by putrefaction, but is one of the secondary diseases of typhus. The impulse and sounds of the heart are weakened—even in rare cases to the degree of being imperceptible. Diminution and cessation of the systolic sound at both sides of the heart may occur, so that only the second sound is heard; in rare cases even the second sound may disappear, and then the first sound is never present. Progressive loss of impulse, and also of the systolic sound, is the principal indication of the depressed state of the heart. The production of a murmur is rare in this state; but is frequently observed in forms of fever characterised by the absence of the symptoms of putrescence. Those of our readers who wish to study this subject in extenso we must refer to our author's work; it therein occupies nearly 100 pages, and is fully and completely discussed in all its bearings.

In *Chapter 8* we have an account of displacements of the heart. These are transverse and vertical; the former kind are most striking. The heart may be displaced by effusions of air, or of fluids into the pleura; by accumulation of air in the lung, as in extreme cases of emphysema, where one lung is particularly affected; by tumours; by diaphragmatic herniæ. Displacement of the heart occurs earlier, and is a more constant sign in acute pleurisy than in hydrothorax; and this Dr S. hints may be referred perhaps to softening of the tissues through inflammation; is not a much more simple explanation of the fact to be found in the circumstance, that in hydrothorax fluid is almost invariably present in both pleuræ, and therefore the pressure on either side of the heart equal?

Of the effect of air in displacing the heart we have as yet, says Dr S., but little knowledge. Of the fact, however, there can be no doubt. We lately had under observation a lad in whom both the left lung and the heart were thus subjected to pressure by pneumothorax of the right side. The physical signs demonstrated the encroachment made by the distended pleura upon the left lung and heart, and the general symptoms showed that neither of these organs could under such pressure sustain life. An opening was made into the right pleura, and air *rushed out* with violence, and the general symptoms were almost immediately relieved. It is manifest that here air found its way into the pleura during inspiration, but could not escape therefrom during expiration.

The displacement of the heart in emphysema is for the most part vertical.

Displacements of the heart may also arise through diminished volume of a lung, whether this be from chronic tubercle, gangrene,

atrophy, or the consequence of emphysema after absorption. Here the heart may not only be drawn by the diminution to the affected side, but also perhaps forced in that direction by the increasing volume of the opposite lung. In such displacements the heart never returns to its natural position. Dr S. considers that if in a case of well pronounced and chronic tubercular disease of the right lung, where disorganisation exists, the heart is found beating to the right of the sternum, we are justified in expecting a longer duration of life than in cases where this displacement has not taken place.

Rupture of the heart is best known as associated with its fatty degeneration. In the left ventricle the accident most frequently occurs. Dr S. does not see his way clearly to the cause of sudden death in such a case. He objects to Cruveilhier's explanation, that the circulation is arrested by the pressure upon the heart of the blood in the pericardium; nevertheless we think this is the true one. Dr S. objects because he cannot reconcile it with the fact of the pressure which the heart bears in other diseases with impunity. To this matter we have already referred. Dr S. does not sufficiently value the force of equal pressure equally distributed over the whole surface of the heart, as it must be in the case of rupture; nor its particular effects upon the auricles. In the cases of pressure in other diseases alluded to by him, the circumstances of the pressure are very different.

Rupture of the valves of the heart is scarcely comprehensible, excepting in connection with their diseased conditions.

Dr Stokes, on the subject of *angina pectoris*, combats, and we think with good reasons, the theory of Dr Latham, viz., that it is a spasm of the heart. He inclines to the opinion of Dr Parry, who attributes the symptoms to a temporary increase of weakness in an organ already weakened. Angina is associated with some, but no special, organic disease of the heart or the aorta—the cases in which this organ has been reported healthy, were probably ill-observed—and also most generally with some form of its weakened conditions, fatty degeneration, etc. Spasm would at once destroy life; but here the blood is retarded and accumulates, producing syncope, pain, and anxiety. After all, as Dr S. truly remarks, “well marked cases of the affection, as described by Dr Latham, are rarely met with, and the same may be said of the purely nervous cases noticed by Laennec.” I have never seen either of these forms, he goes on to say. What gets the name of angina might more properly be called cardiac asthma, which probably is the combination of a dilated heart with congestion and spasm of the lungs.

Into the subject of nervous palpitations of the heart it is impossible for us here to follow Dr Stokes. The subject is interminable, and most varied. The great point is correctness of diagnosis:—is the derangement functional or organic? and this question is manifestly one which must be answered by bringing to bear upon any given case our general knowledge of the history and diagnosis of dis-

eases of the heart. "The facility," says Dr Stokes, "of making a correct diagnosis between functional and organic diseases of the heart is not so great as modern authors lead us to believe." But he adds, what many persons will think neither satisfactory nor sufficiently definite, "and we more often arrive at a just conclusion by instinctive skill, the result of experience and judgment, than by communicable rules of diagnosis." Here, also, Dr S. discusses the question, though it would seem to have fallen more naturally under the head of diagnosis of valvular murmurs, as to the nature and situation of inorganic murmurs. This subject is one of exceeding interest, and, as we differ from Dr S. in his conclusions, we regret that our space prevents us arguing it with him. His position is, that inorganic murmurs, though systolic, are not always confined to the aortic orifice. Herein he differs from most observers; and neither does he prove his case.

In his chapter upon aneurisms, Dr Stokes contends, that there are no peculiar signs belonging to aneurism by which the sounds produced in the tumour can be distinguished from those of the heart, and that, therefore, Dr Hope's statement to that effect is incorrect. Laennec also erred in his assertion concerning the singleness of the aneurismal sound. The most simple expression of the physical diagnosis of thoracic aneurism is this: the existence within the thorax of two centres of pulsation, as indicated by impulse, and by a single or double sound. The first sound is not, as Dr Hope teaches, necessarily a murmur. The presence of a murmur, indeed, Dr S. considers as exceptional and accidental in thoracic aneurism; he has so commonly found it wanting. The whole subject of aneurism is fully and fairly discussed, and illustrated by our author's large experience, and many original observations. There is a fact in the diagnosis of aneurism not here referred to, and to which, as far as we are aware, writers in this country have paid little or no attention; its actual value as a sign we are not able to state, but we think the positive manner in which the fact has been stated by Hamernjk, warrants us in placing his statement before our readers for their consideration. Distension of the arteries, through loss of their elasticity, retards the flow of the blood; and thus the interval between the beat of the heart and the pulse at the wrist is increased in chlorosis, typhus, etc. Just, in like manner, does the increased breadth of the ascending aorta, when converted into an aneurism, act as a retarding power to its flow. The radial, temporal, and external iliac arteries normally pulsate synchronously, but if there should exist an aneurism of the thoracic or abdominal aorta, or if its coats should be much altered, then it will be found that the beat of the external iliac is later in time than that of the radial. So, also, will the beat of the radial artery of an arm, in which an axillary aneurism exists, be subsequent to that of the other arm. The like is true of the beat of the metatarsal arteries in a case of popliteal aneurism, and so of the temporal arteries in aneurism of the innominata and external caro-

tid. This sign, adds Hamernjk, is most important and infallible; tumours may resemble aneurisms, and aneurisms may not pulsate; but if the artery on one side of the body be subsequent in its beat to that in the other, we may be certain that the artery is diseased (Untersuchungen Physiol. Path., p. 81). Speaking of aneurisms of the descending thoracic aorta, Hamernjk observes: that such are of very rare occurrence, and still more rare those characterised by pulsations at the left side of the thorax behind; only one such case exists in the museum at Vienna. This very rarity justifies us in judging pulsating tumours there felt not to be aneurismal, but rather purulent deposits near the aorta, caused by tubercular disease of the vertebræ or ribs. Such swellings may, after a certain time, lose their pulsations. Their true nature may be diagnosed by this, that in such cases the pulsations of the radial artery and the femoral at Poupart's ligament are *synchronous*.

The treatment of thoracic aneurism, Dr Stokes observes, must be palliative, not curative; the so-called treatment of Valsalva is inconsistent with all we know of pathological anatomy and practical medicine. Where the heart's action is undisturbed, medical interference, beyond what is necessary for the relief of pain, is unjustifiable. Should there be excitement of the heart in combination with the thoracic aneurism, we may attempt to moderate its force; and it may happen that the heart is weakened, and then the particular treatment adapted for that condition is required. "The practitioner should never forget that local diseases, themselves incurable, may, nevertheless, co-exist with an excellent state of the general health, for a period indefinitely long, and the conclusion is but too obvious, that as the lesion cannot be cured, the system at large should not be tampered with." Dr Stokes concludes his work with a full account of abdominal aneurism.

We cannot separate from our author without stating in brief the impression which his work has left upon our mind. That this book will take its stand among the classical productions of medical literature, we cannot doubt. Here is accumulated, not so much for the purposes of the mere student, as for the information of the practitioner, the results of the carefully gathered experience of a wise observer—results which have been tried and tested by a quarter of a century's observation. This, therefore, is not to be considered a mere monograph on diseases of the heart; not one of those aids and short cuts to knowledge, which so often greets and delights the student's eye; but what it should be, a faithful transcript of actual disease, such as the observer finds it at the bedside, portrayed with all its difficulties, endless complications, and varying characteristics. And is not this just the particular feature which stamps the productions of those great physicians who have left memorials behind them? How different such from those many resumé of diseases whereon the critic's wits are too often called upon to pass judgment! Where everything is in its place, the pathology, history, diagnosis, etc.,

all manifest, the whole subject brought down to the comprehension of the most simple, made as neat and distinct as an easy mathematical demonstration! The text-book is perfect, manufactured after the most approved fashions of the day in the workshops of literature! How often has the student, thus primed with knowledge, when he faces actual disease, shrunk back on himself. Ah! quantum de spe decidit!

Herein, then, lies, in our opinion, the great value of Dr Stokes' work, that to all such productions his is distinctly opposed. The student, while accumulating facts, is taught to reason on them. More than their barren statement finds a way into his mind. He is brought away from that text-book knowledge, which narrows his intelligence to the observation of a *part*, and which teaches him to forget that this part has its sympathies and bonds with the *whole*. *Here* he learns the fatality of such illusion of ideas; he is made to view disease in its comparisons, in its accidents, in its combinations, and in its universal effects; in fact, to view it as the accomplished physician, whose object is to master neither the diagnosis, nor the pathology, nor the history, nor the results, but all of these; and all of these for concentration around one special purpose—the treatment. *Here*, instead of cramming his brain with dry and barren facts, he obtains a reasoning knowledge of disease.

Such is our author's work—the philosophic production of an experienced physician.

Remarks on the Examination of Recruits, intended for the use of Young Medical Officers on entering the Army. By H. H. MASSY, A.B., M.B., 4th Light Dragoons. London. 1854. Pp. 131.

THIS is a well-timed, and we think likely to prove a very useful publication. The duty of examining recruits is one of the most responsible of the multifarious duties devolving upon a military surgeon, and at the present moment its importance to the public service can scarcely be over-rated. The late Mr Henry Marshall, in his several valuable publications on this subject, had already done much to rivet the attention of the army surgeons to this part of their duty, and to guide them in the execution of it. To those, however, who have not had the means of making themselves masters of his more extended observation, the present manual must prove highly acceptable.

Mr Massy seems well aware of the loss of confidence and other unpleasant consequences which might fall upon a young surgeon were he to commit any error, or series of errors, in the passing of recruits, and also of the extraneous considerations by which it is sometimes attempted to influence him:—

“It must not be forgotten that bad recruits remain as living reminders of a

serious fault. Circumstances often conduce to the influencing men's minds in returning recruits as eligible, such considerations are always to be dismissed; the question admits but one interpretation, is or is not the individual fit for the duties of a soldier? No incidental circumstances or associations ought to bias the opinion of a medical man; good looks often exert an influence in approval, but if to the suppression of a disability, this is a cardinal error. The respectability of individuals is sometimes another inducement, yet I am doubtful that it should be so as a rule; they are often the most worthless members of their family, in which case they are invariably bad soldiers."—P. 6.

In Mr Massy's remarks on the age of recruits we are not altogether disposed to concur; were we enabled upon all occasions to calculate accurately upon the exigencies of the service, and to say at what time recruits might be required for the duties of the field, or to proceed to a foreign, more especially a tropical, station, to replace those who may have fallen victims to an unlooked-for mortality, there would be less objection to enlistment at an early period of life, to which the author seems to lean, particularly as regards the cavalry. When a tailor or a weaver is to be turned into a light dragoon, perhaps the sooner he gets acquainted with his horse the better, but the same remark does not apply to agricultural servants; and where there is even a chance of men being "required for immediate active service, to undergo immediately after enlistment the fatigues of a campaign, to endure hardships, to sustain weight, to resist by strength and moral energy the many sources of disease thence arising, a close approach to the fullest strength, both physical and mental, is undoubtedly advisable." The testimonies of Sir James M'Grigor and Dr Triscombe, of Kirrekhoﬀ and of Cocke, to say nothing of others, bearing more particularly on tropical service, are strong upon this point. We have known, upon one occasion, some hundred boys left behind their regiment as unequal to participate in the duties of the field. The heavy loss of life, the helplessness under sickness, and the multiplied evils, which we have personally witnessed from premature enlistment, incline us greatly to the opinion that the age of 20, or a close approximation to it, an age, in short, at which a lad's youth is completed, and some steadiness of habit required, would be the safer and the better rule for all branches of the service. With regard to the stature of recruits, the leading principle which should guide us here, namely, the average height of the district from which the recruits are to be taken, has not, we think, met with due consideration; and at the present time, from the embodiment of the militia, a facility exists for ascertaining this average, whether by counties or groups of counties in the eastern, midland, northern, etc., whether as applicable to the highlanders of Scotland or the natives of the Lincolnshire fens. We have an obscure recollection of something of this kind being attempted as regards the heads of a militia regiment towards the conclusion of the old war, when Dr Spurzheim was in this country, and phrenology

above principle, it is an easy matter to lower it upon any emergency, as has recently been done; and there is no doubt that a greater number of unexceptionable men will be found below than above the average height of any given district; indeed, it is only for special duties, and for limited numbers, that we should ever recommend the standard to be raised much above the average height. Dr Jackson observes that "in the present time when the fate of battle is often decided by firearms, to which the hand of a man of six feet gives no more impulse than the hand of a man of five, it is not easy to see the reason of the rule which so generally influences the desire of those who select subjects for the formation of armies;" and the late Dr Fergusson of Windsor, a very experienced medical officer, expressed a decided opinion that "he would last longer if the required size and stature of the British soldier were not pitched at too high a standard."

In proceeding to consider the several disqualifications specified in the instructions to medical officers for passing recruits, Mr Massy has followed a plan which has enabled him to concentrate a great amount of practical information within a very limited compass, by commenting successively on the several disqualifying circumstances. This is, we believe, precisely the plan which has been followed by the Professor of Military Surgery in this University for many years past, who, in lecturing on this subject, takes the recruiting instructions in his hand, and comments seriatim on the several items as they occur. As specimens of the manner in which Mr Massy has executed this part of his work, and as proofs of the attention which he has given to the subject we select one or two passages, bearing on what may be considered the lighter or more local causes of disqualification. On the "Loss of many teeth, or the teeth generally unsound." This has sometimes been considered as applying only to the loss of the front teeth as incapacitating a soldier for the mechanical operation of biting off the end of the cartridge with facility; but says Mr Massy:—

"The objections to unsound teeth are numerous and valid when judiciously applied. Many have stated that the loss of the incisors of either jaw incapacitate a soldier from effectually biting off the ends of cartridges when loading. This is unquestionably the case, as some force is requisite to tear the strong paper of which they are constructed; an act that could not be quickly accomplished if deficiency or decay of the incisors was extensive. A serious objection is likewise constituted in the fact that general unsoundness or loss of many teeth is commonly an indication of a delicate constitution, symptomatic of depraved health, or occasionally of the strumous diathesis; sometimes of the abuse of mercury, or the effect of mineral acids. The teeth are a most important provision in nature for the perfection of digestion; they serve as appreciable distinction in classifying the animal kingdom; their peculiar shape, arrangement, and density differing, and evidencing not only the nature of the food, but how essential in some is accurate mastication to the accomplishment of digestion. The loss of many molars and bicuspsids must tend to the production of dyspepsia, thereby alone injuring the general health or assisting the

train of causes producing disease of organs where there pertains any peculiar idiosyncrasy of constitution. Decayed teeth often create excessive sufferings from head and toothache; abscesses form in the gums, necrosis is sometimes induced, ulcers form in the tongue, and occasionally violent inflammation is produced in this organ and adjacent tissue. Decayed stumps are often most difficult of extraction, and the circumstance of men coming frequently to hospital on account of any of the very painful results of decayed teeth, which may admit of only tardy or temporary relief, should assist in deeming this a decided disability when extensive."—Pp. 61, 62.

With reference to flat or splay feet, which some may look upon as only a trivial deformity, we have the following remarks:—

"The remote causes of this derangement are not clearly understood, they have been variously assigned and explained by different writers. A most concise description was published in Rust's Magazine, vol. v.; the original was a circular letter to the Prussian military physicians, by the chief of the military medical department. In Chelius's Surgery the subject is likewise treated of both by the author and in Mr South's notes. This deviation appears to be often congenital, yet it is frequently first apparent after the tenth year; and is most usually symptomatic of debility or a strumous diathesis. From the result of several investigations, the local change seems to resolve itself to a sinking of the arch of the foot, and an inclination inwards as well as downwards of the os calcis, astragalus, navicularis, and cuboides, as well as of the cuneiform bones. Another defect is seen in the elongation of the internal lateral, calceoscaphoid and calceo-cuboid ligaments, together with a yielding of the planter fascia and tendon of the tibialis posticus, as it passes under to be inserted into the navicular bone. On this account the posterior aspect of the navicular bone sinks, and with it the anterior round surface and neck of the astragalus; which of these is primary or how they result is not determined. By these means the arch is completely depressed, and its elasticity destroyed; the strength and the function of progression are materially compromised; endurance and ease in pedestrianism, when the mal-formation is excessive, are altogether impossible. There are other formidable effects, such as 'speedy fatigue in walking, swelling of the foot around the ankles, and soreness of the soles of the feet.' From continued straining of the feet, even chronic inflammation of the ligaments and synovial membranes arise, by which not merely pain but also serous exudation among the tarsal bones takes place."—Pp. 112, 113.

We have, we think, said enough to show that this little publication, along with those of the late Mr Marshall, will put the young medical officer in possession of all that can well be learned from reading on the subject of passing recruits; and we would again press upon the army surgeon the importance of a due attention to this part of his duty, whether as regards his own credit and comfort, the character of his corps, or the best interest of her Majesty's service.

Handbuch der speciellen Pathologie und Therapie. Erster Band. BEARBEITET VON R. VIRCHOW, J. VOGEL, und STIEBEL. Erlangen 1854. 8vo. Ss. 551.

Handbook of Special Pathology and Therapeutics. Vol. 1. Conducted by R. VIRCHOW, J. VOGEL, and STIEBEL. Erlangen, 1854. 8vo. Pp. 551.

THIS work, instead of being a handbook or manual, is destined to consist of several volumes, the articles being compiled by various

writers, after the style of our Encyclopædias. It presents the usual systematic character of German compilations, and will be found useful to those who are investigating modern pathological literature. At the same time some caution will be requisite, before allowing all the credit which the individual authors claim for themselves. We have been induced to make this remark from observing the following passage in reference to leucocythemia, by Professor Vogel.

“Virchow possesses the merit, by means of researches in the dead body, of having pointed out this condition, and of having, since then, elucidated it with great perseverance during a series of years. In addition, Bennett of Edinburgh deserves credit for having further enlarged our knowledge of the disease. I was the first in Germany to diagnose Leukhemia during life, and since then numerous other cases in Germany, England, and America, have been observed and described.”—P. 393.

Now we were aware that Professor Virchow, by a note in his own archives (so coarse and unmannerly, however, as to prevent reply), had claimed the discovery of leucocythemia. (Bd. 5, p. 126). But we must confess our surprise at finding Julius Vogel so ill-informed as to countenance his co-editor's pretensions. But as it would seem that the German pathologists are unacquainted with the true history of this matter, we may take this opportunity of communicating to them the true facts of the case:—

Professors Bennett and Virchow held similar appointments in large hospitals (pathologist or prosector), the one in the Royal Infirmary of Edinburgh, the other in the Charity Hospital of Berlin. Professor Bennett's first dissection was made on the 19th of March 1845, whereas Virchow's first dissection was made on the 1st of August 1845; that is nearly six months afterwards. Again, Professor Bennett's case was published in the *Edinburgh Medical and Surgical Journal* for October 1845,—whereas Professor Virchow's case was not published until the second or third week of November 1845, in *Froriep's Notizen*. The first author distinctly points out the peculiarity of this pathological state as being new. He considered the colourless cells in the blood as being similar in conformation to those of pus, as they are; but distinctly states that they were not dependent on inflammation, nor are in any way connected with it—as there was no hematitis, no phlebitis, no secondary deposits, no evidence of inflammation anywhere. Here then, we contend, was the true discovery, and all the facts were carefully investigated at the time, drawings made, and preparations which still exist preserved, of a condition of the blood up to that time altogether unknown. Now as it was impossible for the Edinburgh Professor to have known in March what occurred in Berlin during the following August, or to copy in October what was published during the following November, we think it remarkably audacious in Virchow to insinuate that he did so. On the other hand, the *Edinburgh Journal* for October, may well have been received at the library at Berlin before *Froriep's* weekly paper of the middle of

November, for we know it is taken in the royal library, because we have seen it there. Hence if there be plagiarism at all, it must be by the German Professor. But dates render it certain that the observations were independent of each other; although the Edinburgh Professor was the first, and not he of Wurzburg, as Vogel has stated.

We give Professor Virchow every credit for his labours in this matter. He first made the statement that the corpuscles were the colourless cells of the blood and not those of pus. But it must be remembered that, as so often happens in the history of science, the way was prepared for him by Professor Bennett's having previously shown that they did not originate in inflammation or purulent absorption. Again, the possibility of the corpuscles being the colourless cells of the blood was discussed at length by Dr Bennett, who says, "we know of no instance where they existed in the amount, or ever presented the appearance described." On this hint Virchow spoke, said they *were* the colourless corpuscles, and thereupon claims all the merit of the discovery. On the other hand, the name of white blood, derived from the colourless aspect of the clot, was absurd. This white appearance of the coagula was most carefully described by Bennett, who avoided the error of constituting it the essential part of the morbid condition, and founding a name upon it. Neither is it at all applicable, for when observed in the living body the blood is not white, as Dr Parkes remarked. Hence this name, upon which Virchow lays so much stress, is altogether inaccurate, and conveys in itself a most erroneous notion. The blood of acute rheumatism and pleurisy, has just as much reason to be called white on account of the buffy coat; and besides, the term white blood has long been used in cases where the serum is milky. Virchow also made literary researches, and was fortunate enough to meet with three other cases which he described before Professor Bennett met with a second in January 1850. But after this the elaboration and correct generalization of the whole subject is again due to the Edinburgh pathologist, who recorded the results of his researches with chemical analyses in a series of papers in this Journal during 1851, and published his separate work, March 1852,—whereas the elaborate paper of Virchow did not appear until six months after, in August, in which he talks of "the free and easy appropriation of other men's intellectual labours which is peculiar to the Scotch." It may have been mortifying for the Wurzburg Professor to find himself here again six months behind the Edinburgh one—a mortification, we regret to say, which he exhibited by personal invective and aspersions, undeserving the character of a teacher and of a man of science. So much for the discovery and description of leucocythemia.

Neither can we compliment Professor Vogel for his candour regarding himself. It is true he says he was the first to diagnose this condition in the living subject in *Germany*, and that *since then* other

cases have been met with, etc. Whereas the fact is, the first to determine this condition during life was Dr Fuller of St George's Hospital, London, so early as December 1845. It was also diagnosed in London by Drs T. K. Chambers and Walshe in 1846; by Dr R. Quain in 1848; by Dr Parkes in 1849; by Professor Bennett in the Edinburgh Royal Infirmary in January 1850; and again by Dr Fuller in February of the same year,—whereas Vogel's case did not enter the Clinique at Giessen, until the following July of that year. Even in Germany, Virchow has himself informed us that H. Müller, on examining fresh blood from the arm of a living man, found in it as many colourless as coloured corpuscles, on the 24th of January 1848! (Archiv. Bd. 5, p. 54).

This attempt at appropriation, in a matter which admits of such easy refutation, does not predispose us favourably with regard to the spirit in which, under the auspices of Virchow, this new Handbuch is likely to be brought out.

Veterinary Medicines, their Actions and Uses. By FINLAY DUN, V.S., Lecturer on Materia Medica and Dietetics at the Edinburgh Veterinary College. 8vo. Edinburgh, 1854. Pp. 412.

“THE merciful man is merciful to his beast.” So says the old proverb, and it is a good and true one. He who would wish to lay claim to this divine quality, as manifested by kindness to his quadrupedal servants, will, with us, rejoice in the improvement which has taken place in late years, in the means of affording relief to our domestic animals when suffering from disease. The medical cure of our dogs, horses, and cattle is now entrusted to men duly educated and properly qualified. It is not a great many years since the term “horse doctor” implied a better sort of farrier, who might be useful to play off against the horse dealer when you wanted to buy a nag, but who, as regarded the diseases of your horse, was no more worthy of confidence than your groom or ploughman. It is otherwise now. The horse doctor of the present day is a well educated and scientifically instructed practitioner. He is lectured to, and rigidly examined upon anatomy and physiology, chemistry and materia medica. He dissects, gets clinical instruction, receives a diploma. He is no longer a horse dealer, but a veterinary surgeon. If any one wishes to test the truth of these remarks, let him, next April, attend the examinations of candidates at the Highland Society's veterinary college. He will there see to what sort of probation our modern veterinary surgeons are subjected. In the meantime, he may take Mr Dun's Treatise on Veterinary Medicines as a sample of the kind of instruction given to the veterinary students. It is a very useful, unpretending work, highly creditable to its author, and to the institution where he lectures on materia medica. The introduction contains a succinct, and, to our mind, common-

sense dissertation on the general action of remedies. The body of the work consists of concise and accurate descriptions of the articles of the materia medica used in veterinary practice, carefully brought up to the present state of science, as regards chemical history and physiological action. Of the practical applications to veterinary cases we do not profess to judge; but, if they be as correct as the chemical, pharmaceutical, and physiological portions of the work (and we have no reason to believe them to be otherwise), the British veterinary students and veterinary practitioner are under deep obligations to Mr Dun for having given them a good book on the veterinary materia medica in their mother tongue, which hitherto they have not possessed. We must observe, however, that Mr Dun, from his want of practical experience in human therapeutics, is sometimes led, when taking the action of drugs on man as a standard of comparison, to adopt too implicitly statements which are not warranted by correct observation. Thus, when he tells of a dog requiring about a drachm of aloes, "or about twice as much as would kill a man," he adopts a statement for which we believe there is no foundation; in fact, half a drachm of aloes might possibly fatally exhaust a debilitated man, but would not, as this sentence implies, prove fatal, we believe, to a healthy individual. Mr Dun has done good service by this book. It remains for him to add to his claims upon the gratitude of his fellow veterinarians, by giving them a similar treatise on Veterinary Hygiène and Dietetics, to which, we have reason to believe, he attaches, and most properly, a high importance.

Dr Conquest's Outlines of Midwifery. A New Edition. By J. WINN, M.D., etc. London, 1854. 12mo. Pp. 323.

DR CONQUEST'S work was highly respectable in its day. It was written chiefly for students. "Neither time, nor labour, nor bedside observation have been spared by the editor (Dr Winn), in his endeavours to sustain the essentially practical bearing of the original work, and to make it of value to those who cannot find leisure to consult more elaborate treatises on obstetric science" (*Preface*, p. 3). We have carefully looked over the pages of the book, reading especially the editor's notes, and can discover no sign of unsparing labour, but the reverse. Moreover, we can scarcely say for which class of readers it is suited. It is, above all, ill suited for such as have little leisure.

The book, however, is not destitute of merits. It contains Dr Conquest's own text unaltered. It has several illustrative woodcuts, is printed in a well-sized type, and is generally simple in its statements. It may, therefore, be of use to beginners, and in the education of monthly nurses.

On the other hand, we think it necessary to draw the editor's attention to the few following points, which we take as they acci-

dentally turn up, and suggest the propriety of decided alterations in them if the book ever reach another edition:—

At page 34, the Graafian vesicle is described as “composed of two membranes, a central yellowish pulp (vitellus), and, within this, the *ovulum* or germinating vesicle”! At page 35, we are informed that the corpus luteum “at the end of the fifth month of gestation is, as a general rule, completely absorbed”!! At page 58 is a bad imitation of Dr John Reid’s diagram of the placental circulation, preceded by some remarks patronising Goodsir, Weber, and Owen, and followed by conclusions in which Dr W. has confounded himself with Dr Reid. At page 61, we have the old and erroneous diagram of Wagner, of an embryo about the fourth or sixth week, and subjoined the ludicrous statement that it represents an ovum and its membranes in the third and fourth months. At page 103 we have a diagram of the “first stage completed,” in which the child’s head is represented as resting on the perineum. At page 185, in describing (in seven lines!) the methods of inducing premature labour which deserve attention, we have no mention of the use of the douche. At page 211 the possibility of superfœtation is denied, although its occurrence was explained in a late volume of this Journal. The description of hydatids of the uterus is very bad, p. 216. At page 311, M. Jobert’s name does not appear in connection with vesico-vaginal fistula, and instead we have a notice of Mr Baker Brown’s cases, which are *likely* to be successful.

We draw attention to these few scattered particulars, not merely on account of their glaring inaccuracy, but also because this book has received unqualified commendation from more than one of our contemporaries. The medical press has lately produced several obstetric publications of the same scope as the present one, and at the same time such as are more trustworthy guides to the practitioner in his daily routine. We would mention especially the work of Nægele, on midwifery for females as one, the translation of which, from the original, or from the French edition, would worthily occupy the time and talents of Dr Winn, or of any other gentleman, who feels inclined to labour in bringing before the profession the works of others.

Part Third.

LECTURES.

INTRODUCTORY LECTURE DELIVERED TO THE CLASS OF MILITARY SURGERY
IN THE UNIVERSITY OF EDINBURGH, MAY 2, 1854.

BY SIR GEORGE BALLINGALL.

I ENTER, gentlemen, said the professor, on the duties of the present session, under a sense of responsibility which I have not heretofore felt; and it is all the more heavy because I am becoming the less able to bear it. We are not now engaged in “little wars” in distant quarters of the world, but, for the first time since I have held office, are embarked in a formidable conflict with a gigantic power nearer home—a conflict in which the lives and the health of our soldiers and seamen must be deeply involved. We are also about to have established chairs of military surgery in the two other capital cities of the empire. This is an object which I have long desired, for which I have strenuously contended wherever I could make my voice to be heard; and for which I must, in some measure, be held responsible, as having been the first to advise it. The occupant of this chair, whoever he may be, will, I expect, soon have to compete with two talented rivals

—rivals in the race of honour and utility—rivals in their ambition to promote the health and the well-being of our brave soldiers and seamen. The contingencies of a great European war, will, I trust, bring me an increased number of those interesting communications which I have ever warmly cherished, whether coming from my contemporaries in the service, or from my old pupils. They also impose upon me the necessity of a vigilant attention to the events of the coming campaign, and to the reports, whether public or private, on the health of the troops and the treatment of the wounded.

After a notice of those most prominent in the department of military medicine and surgery, Sir George Ballingall proceeded as follows:—

I have, gentlemen, upon the present occasion been somewhat brief in my notices of the lives and writings of the army and navy surgeons, in order to make room for a short sketch of the introduction and progress of the class of military surgery in this university. I think it right upon this occasion (possibly the last, on which I may have to address a new audience), to give something like an account of my stewardship—something like a “*compte rendue*”—some account of the adverse circumstances I had to contend with at the outset, and of the very gratifying support which I have received in later years—something which may excuse me, in the eyes of the profession, for many short-comings, of which I am abundantly sensible—and something which may encourage you to perseverance in a good cause.

You are already aware that the institution of a chair like this was first recommended by that distinguished surgeon, the late Mr John Bell, in the memoir to which I have just referred—a memoir which you will find reprinted in the *Medico-Chirurgical Review* for 1844—a memoir which you will do well to peruse. With some enthusiasm in the language, perhaps I may say some extravagance of expression, it breathes throughout a spirit of patriotism and a feeling of sympathy with the wounded seaman, worthy of all admiration. John Bell, however, was never destined to fill this chair. After a lapse of several years the office was instituted in the person of a not less celebrated man, the late Dr Thomson, who had been previously nominated to a professorship of general surgery, established by the Royal College of Surgeons. I know from the best of all authority, namely, from Dr Thomson's own lips, that a main object which he had in view, in connecting the professorship of military surgery with his other office, was the more complete and effective teaching of surgery within the university. But while this measure was one of great importance to the teaching of general or systematic surgery, as connected with this school, I have always held it to have been an unfortunate event for the cause of military surgery. By this arrangement the course of military surgery was conducted, during the whole of the Peninsular war, in conjunction with, or as an appendage to, a more extended course of lectures. The class thus lost its individual character; it lost, above all, the concentration of Dr Thomson's acknowledged talent upon a subject which he could not have failed to elucidate, to improve, and to amplify. I need not, gentlemen, hesitate to repeat what you will find recorded in my evidence given before the Royal Commissioners for visiting the Scottish universities in 1826. I have there said: “Had Dr Thomson been bred a military surgeon, and had he devoted the energy of his mind to the class of military surgery alone, I think it would have held a different rank in the public estimation, and in the university from what it now does.”

The circumstances to which I allude, had exercised a depressing influence upon the class. Mr Bell's memoir, which contains an excellent programme of a course of military surgery, and of which only a very few copies had got into circulation, had done little to open the eyes of the profession to the extensive field which may properly be embraced in such a course. When I succeeded to the chair in 1822, upon Dr Thomson's resignation, I found little facility for the illustration of a course of military surgery. No separate nor complete course of lectures had yet been delivered; no class-room nor museum was appropriated to it in the college; and indeed I may almost say, that it had neither “a local habitation nor

a name." Numerous prejudices and misconceptions in regard to this subject prevailed in the professional mind, the most prominent of which was, that a course of military surgery was only to be a series of lectures on gunshot wounds. The public mind, again, had begun to be turned, well and wisely, to the arts of peace; the country was heartily tired of war and all its expensive establishments; reductions had been effected, and were still proceeding in all directions. Even the small saving to be accomplished by the suppressing of this chair, was, I have reason to know, suggested to the authorities. In the session of 1823-24, I delivered my first course of lectures, supported by thirty-three medical officers in the public service, and only four other pupils. The attendance of the former class of gentlemen was indeed for many years my principal encouragement to go on. From the period I have specified up to the year 1846, when I was obliged, in consequence of the state of my health, to transfer my lectures to the summer session, the average attendance of surgeons of the army and navy, and Hon. East India Company's Service was twenty-two. Many of these gentlemen had returned from foreign climes, full of interesting, and to me very valuable information. Numbers of them had, like myself, entered the service before they were even qualified by age to take a doctor's degree, and had returned to this school for the purpose of graduation. This class of pupils has naturally and greatly diminished in proportion as it has become more common for gentlemen to take their degrees before settling in practice, or entering the several departments of the public service, and in proportion as there are fewer of these gentlemen studying here in the summer than in the winter session.

From the moment I entered upon office I have ever entertained the opinion, that if there was any one class of gentlemen, more than another, to whom my experience had qualified me to be useful, it was to those educating for the East India Company's Service, or serving with Her Majesty's Regiments in that country. I had acquired some experience in the practice of encampments in India, a position in which, whether in peace or in war, the Indian army passes so large a portion of its time. I had been in my tent during a part of two monsoons, and even with all the appliances and means which an officer can command, knew well how trying it is to the health of every individual, more especially to that of the common soldier, to be exposed in such a situation; I had ample experience in the treatment of the fevers, dysenteries, and liver diseases of India. At Prince of Wales' Island I had seen much of those formidable ulcers so destructive to the soldiery in some of our eastern possessions, and I had seen something of the treatment of the wounded at Java. The two last mentioned localities are within a few degrees of the line, where the soldier's constitution had to contend with the baneful influence of the climate, as well as to repair the injury done to his person.

One of my first moves, then, was to apply to the learned gentleman at that time holding the appointment of examining physician to the East India Company, for a recognition of my course, so far as to recommend it to candidates for his department. I was met, however, by technicalities, perhaps not very unnatural for a London physician who had never been engaged in any branch of the public service. I was told that he had nothing to do with surgery, and that he trusted entirely to the College of Surgeons for the surgical qualifications of his candidates. I then memorialized the Court of Directors; but here my memorial, no doubt through the same limited views and erroneous representations, met with a very unmerited and ungracious reception. It was represented as casting reflections upon the character and education of the Company's surgeons, than which nothing could be further from my intention. Fortunately, however, I was enabled to show that that very memorial had been penned by a distinguished medical officer of the Bombay army. I was supported by the favourable opinion of ten medical officers of the Honourable Company's Service, from the rank of superintending, to that of assistant-surgeon, several of whom were then pupils of the class, and expressed themselves to the following effect:—"We, the

undersigned medical officers of the Honourable East India Company's Service, do most willingly express our opinion of the great utility of the lectures on military surgery delivered in the University of Edinburgh to those young men educating with a view to our service. Some of us, who have now retired from the service, have to regret that no similar source of instruction existed at the period when we were educated; and others who are about returning to India, have eagerly embraced the opportunity afforded us by Dr Ballingall, of profiting by his experience in the treatment of the diseases incident to European troops in that country." My cause was kindly espoused by the late Colonel Vans Agnew, then in the Direction, to whom I had been slightly known when serving as Assistant Surgeon to the Royals, when he was Deputy Adjutant-General of the Madras army, and it ended by a qualified recommendation of my course, notwithstanding the unfavourable feeling of the examining physician. This was a "heavy blow and great discouragement" at the outset, but has been amply compensated by the support I have received from the same quarter in later years, and upon which it will be more agreeable to enlarge.

No other circumstance of any moment affecting the class of military surgery occurred until an investigation took place into the state of the Scottish universities and schools of medicine in 1826, when sentiments favourable to this chair were expressed by the late Director-General of the medical department of the army, and by others. I got many kind hints from well-meaning friends that the professorship of military surgery might easily be converted into a professorship of general or systematic surgery, which was still a desideratum in the university. This view of the matter, however, I steadily resisted, and in a memorial, submitted to the Royal Commissioners for visiting the Scottish universities, expressed myself to the following effect: "Whatever may be my sentiments as to the expediency of instituting a professorship of general surgery in the university, and they are certainly favourable to such an institution, I am at the same time decidedly of opinion that such a professorship should not be formed by sacrificing the chair of military surgery, a chair which in the hands of an experienced army surgeon I have ever considered capable of being rendered interesting to the students of medicine generally, and peculiarly important to such of them as may be educating for the public service of the state."

In 1829 the regulations of the Royal College of Surgeons of Edinburgh, to be followed by candidates for their diploma, underwent a protracted and careful revision. Two courses of surgery were enjoined, and a resolution was adopted, giving to those students who chose to avail themselves of it, the option of taking a course of military surgery in lieu of one of these. With the passing of this resolution, and the manner in which it was carried, I have always been peculiarly gratified. It was moved by an able and experienced naval surgeon, the late Dr Kelly of Leith, who, in bringing it forward, said, "I do so as a boon to the student, not to the professor." To this period I look back, as the era from which I date the subsequent change which has taken place in the minds of the profession and the public as to the utility of separate courses of instruction for military and naval surgeons. It was speedily followed by a corresponding movement on the part of the heads of the medical departments of the army, the navy, and the ordnance, giving the same option to candidates for their respective branches of the service. From these quarters, indeed, I have always experienced a steady and consistent support. From Sir James M'Gregor and Sir William Burnet I am in possession of numerous letters expressing approbation of this course, and of my writings relative to it. In one of these letters the late Director-General expresses his regret that similar lectureships did not exist in the other schools of the empire; and the course is now I believe recognised by most, if not all, those licensing bodies by whom two or more courses of surgery are required. The session 1829-30 was the first in which the regulation I have alluded to could have had any effect on the class; and it is somewhat remarkable that while three sessions before this period the number of pupils, exclusive of those in the public service, was only nine, in three years after, in the session 1832-33, it had risen to twenty-nine.

The next most important event affecting this class, was the securing of Mr Alcock's museum. When I heard of this being likely to come into the market, in consequence of the failure of Mr Alcock's health, and his being obliged to relinquish his profession, I urged, in my place in the *Senatus Academicus*, the propriety of obtaining his collection for this school, and after a report upon the subject by Mr Guthrie, and the late Mr Liston, the money was voted, and my wishes very kindly acceded to. This, under a more liberal patronage from the Government, is an expense which, with great propriety, might have fallen upon the public; but you will agree with me in thinking, that it was a suitable and a becoming act on the part of a body which owes all the funds it possesses to two general officers of the army—the late Generals Reid and Stratton. If there is any neglect or omission, for which I particularly blame myself, it is for not having cultivated more assiduously the acquaintance of the late Sir Joseph Stratton. I had known him many years before as a major in the army, in command of the Inniskillen Dragoons, and repeatedly met him in society after I became resident in Edinburgh. Had I been aware of his benevolent intentions towards this university—had I brought him to my class and shown him my museum, I make no doubt that he would have been induced to specify the military surgery, as General Reid did the music, as a special object of his bequest.

In the autumn of 1843, I made a visit to the military hospitals on the Continent, and the schools of instruction for the army surgeons attached to some of these establishments. On my return, I addressed a letter to the late Sir Robert Peel, urging the endowment of additional chairs of military surgery in this country, contrasting the liberal expenditure of the foreign governments, France, Austria, Prussia, and Belgium, in providing instruction for the medical officers of the public service, with the scanty provision made for the same purpose in this country; contrasting also the remarkable difference of duties which devolve upon the surgeons of the continental armies compared with those which must be undertaken by the surgeons attached to the fleets and armies of Great Britain. The former are occupied, I may say exclusively, in treating the diseases of their native climate, while of the latter, not less than two-thirds are constantly employed in the treatment of formidable and fatal diseases, incident to soldiers and seamen on foreign stations and in tropical climates, diseases with which neither practitioners nor teachers in this country are, of necessity, practically acquainted. This letter, although favourably received, could not be expected to command much of the attention of the Premier, occupied, as of necessity he always is, with the graver concerns of the state; and was merely addressed to Sir Robert Peel as having been the Home Secretary, and the dispenser of the Crown patronage, at the time I received my appointment. The letter, however, attracted considerable attention for the moment, and brought me a large number of complimentary acknowledgments from various quarters. But this soon passed away, and it was likely to follow the fate of John Bell's memoir, until in the summer of 1849, when I was resident at Taunton in Somersetshire, a copy was put into the hands of Sir De Lacy Evans, which he acknowledged in the most encouraging terms.

It is to this distinguished officer that the public is indebted for carrying through Parliament a measure, which, if followed out with due zeal, energy, and intelligence on the part of the teachers, cannot fail to be highly beneficial to the health of the soldiers and seamen, and, at the same time, creditable to the country. But let me first advert to the other quarters from which I have received support in urging the endowment of additional professorships. Wherever it has been mentioned, either by the military or medical journals, both those published in this city and in the other divisions of the empire, the measure has, with one solitary exception, met with unanimous support. I allude here to an unfavourable, and, I say it advisedly, an inexperienced critique on my letter to Sir Robert Peel, published in the *Medico-Chirurgical Review* for 1844. This, however, I have no reason to regret, for it gave me the opportunity of publishing in the number of that Journal for July 1844 ("Extra limites") a reply, which

was, I believe, conclusive ; and which enabled me to give to my professional brethren some explanatory details, which would have been altogether out of place in an address to the minister. Of the professional Journals which have taken part in this question, none have advocated it with more steadiness, consistency, and vigour, than the *Lancet*. There you will find, within the last few years, a series of powerful leading articles, supported by reference to authorities of the highest import, those of Sir William Napier, the historian of the Peninsular war, the late venerable Dr Robert Jackson, Mr John Bell, Mr Guthrie, and many others. There also have been published, successively, during the last three years, extracts from each of the introductory lectures delivered to the students of this class. But this is not the only way in which that Journal has promoted the cause. In it have appeared numerous papers and letters, relative to military hygiene, and to the equipment of the soldier, as bearing on his health ; there, too, has been strenuously advocated the cause of the naval assistant-surgeons, a cause which must ultimately triumph, notwithstanding the determined opposition which it has hitherto encountered. In the discussions upon this point, an event in the mutiny at the Nore in 1797, seems to have been lost sight of. On that occasion, the seamen, amongst other things, petitioned for "better attendance when sick and when wounded in action." Let us take care that it does not come to this again.

Of the gentlemen to whom individually I feel indebted for support, are two connected with the service of the Honourable East India Company, and their conduct forms a most agreeable contrast to the little encouragement I got from that quarter at the commencement of my labours ; the first of these gentlemen is Dr Scott, who, for several years past, has so well filled the office of examining physician to the East India Company, and who, two years ago, with the sanction of the Court of Directors, took the very important step of requiring attendance on a course of military surgery, by all candidates for the East India Service, who may be educated in schools where such courses are given. But it is by the communications, both public and private, of Mr James Ranald Martin, of the Bengal Medical Service, that I have been more especially encouraged to persevere, when desponding under feelings akin to those which must have actuated Mr Guthrie, when he penned the concluding paragraph of his recent letter to the *Times*, on the Medical Service of the British Army. But I should do Mr Martin little justice, if I were to withhold from you the knowledge of other cases in which his name stands prominently connected with the honour and interest of the medical departments both of the Queen's and the East India Company's services. He is the author of the "Claims of Medical Officers to Military Honours." This consists in a series of interesting narratives of personal adventure and gallant conduct on the part of medical officers in the service of the state ; and some startling facts as to the numbers of them who have fought, who have bled, and who have fallen in the service of their sovereign and their country. This gentleman's friendly intercourse with Sir Howard Douglas and Sir De Lacy Evans, and his relationship to the late distinguished Adjutant-General, Sir John Macdonald, gave him an influential voice in that matter, and you will, I trust, agree with me in thinking that it was well exercised. But there is yet another important measure affecting more particularly his own branch of the service, in which Mr Martin took part. It is chiefly owing, I believe, to this gentleman's representations, that the rule of seniority so long rigidly followed in appointing to the highest positions in the medical department of the Indian service, has recently been

hospitals, nor would he have been raised to the responsible position of Director-General of the medical department of the army five-and-thirty years before the termination of his professional life.

But to return to the chair of military surgery, the subject was first broached in the last session of Parliament by Sir De Lacy Evans, in a speech which you will find specially reported in the *Lancet* for 8th March 1863. It was kindly received by the Secretary at War, who immediately set about inquiries to enable him to judge of the matter. I was soon after this given to understand, that the medical testimony in favour of the measure was abundant, but that it was desirable to have the opinions of some experienced general officers on the subject. Upon this I immediately wrote to the Earl of Cathcart, who had many years held the appointment of Commander of the Forces in North Britain, and who had been repeatedly present in this class-room. In his lordship's reply, in connection with many kind and too flattering expressions towards myself, is the following:—"Having had the pleasure of being present at several of your introductory lectures, during the period I held the command in Scotland, I had ample opportunity of appreciating the high value and importance of such a branch of instruction to the students who were qualifying themselves for their future duties as medical officers of the army. It is, therefore, with great satisfaction that I now express my most cordial concurrence with Sir De Lacy Evans, in the view he has taken of this subject, being firmly persuaded that by extending the advantage of such institutions to the other capitals of the United Kingdom as he proposes, the result would be most beneficial to the best interests of the country, both in regard to economy of money, and what is of more value, human life." From the late Lieutenant-General Sir Neil Douglas, who also for several years held the command in this country, and who had done me the honour of being occasionally present in this class room, I was previously in possession of a letter containing the following expressions with reference to my address to Sir Robert Peel:—"I have read your pamphlet with much attention, and as I think you have made out a most triumphant case, I trust your highly useful suggestions will be attended to." These letters were forwarded through Mr Martin, with one or two of the others which I possess on the same subject, from Sir Thomas Brisbane, Sir James Russell, my old shipmate Sir Robert Sale, and my old brother officer General Wetherall, the Deputy Adjutant-General of the army, and now about my oldest friend in the service. These letters must, I think, have done much to confirm the favourable opinion which the Secretary at War seems from the first to have taken of the measure which I have so long advocated. I was told, indeed, many months ago, by a distinguished medical officer, that if any Secretary at War was likely to adopt my views, the Right Honourable Sidney Herbert was the man. I heard, however, nothing more upon the subject, until I was agreeably surprised on reading the right honourable gentleman's speech in moving the army estimates this session, to find that with repeated expressions complimentary to this school of medicine, and to the gallant officer who last year introduced the subject to the notice of the House, he proposed a vote of £400 for the endowment of additional professorships of military surgery in London and in Dublin, and this passed without a dissenting voice.

I must now apologise for the egotism into which I have been led, but I commenced by saying that I was going to give a sketch of the progress of military surgery in this university, and from this, after more than thirty years' tenure of office, I cannot disconnect myself. Some allowance will, I hope, be made for a little feeling of exultation, when you consider that at the period of my accession to this chair, the question of doing it away, as a sinecure, was gravely considered, while there now stands recorded in the present session of Parliament an unanimous vote of the house for the endowment of two other chairs of the same description—when you consider that, exclusive of medical officers in the public service, I commenced my first course of lectures with only four pupils, while I terminated

last summer session with forty-seven.

I would gentlemen, advert for a moment to some of those subjects which

must immediately engage your attention at the very commencement of the course, and which press upon my mind at present in consequence of some remarkable coincidences. You will easily understand that the recruiting of the army is a subject of paramount importance to its efficiency. The author of the "Formation, Discipline, and Economy of Armies," has well observed, that the selection of men fitted for the purposes of war, and their instruction in approved forms of discipline, is a subject demanding the closest attention of every patriotic statesman, and of every scientific soldier. We have within these few weeks a well-timed work on the recruiting of the army by Mr Massy, which I shall in my next lecture bring under your notice, along with that of my late friend Mr Marshall. But there are two points connected with this subject, which do not appear to me to have yet sufficiently engaged the attention of the authorities—viz. the principle upon which the standard height for the recruit should be fixed, and the use of the andrometer. The standard has often varied, and must, indeed, vary from time to time, according to the greater or lesser exigencies of the service, but it should never deviate far from the average height of the nation, or of the district from which the recruits are to be taken. The closer it comes to this average the better, in as far as the health, the energy, and endurance of the soldier are concerned. The Andrometer or man-measurer, of which you will see a woodcut in my "Outlines," and of which I shall show you the original specimen, made by the inventor, is calculated to take the admeasurements, and give the proportions of all the more important parts of the person. Let us, then, take a pattern man of the standard height (say 5 feet 6, 7, or 8 inches, as the case may be)—a man perfect in his physique, with whose development no fault can be found, and let us note his proportions. Let every succeeding recruit be then placed in the andrometer, and see how near he comes to this standard of perfection, or where he deviates from it. Were this to be generally practised, no faulty development in any of the more important parts could possibly escape the observation of the most inexperienced observer; and at a time when so many men are to be passed into the service, whether for the militia or for the line, I cannot but think that the use of this instrument would prove a most important auxiliary to those surgeons of militia who may have but little experience in this essential part of their duty, and to those young surgeons entering the army, who may, as I was myself, be called upon to exercise their judgment in passing recruits before they are many hours in the service. I am far, however, from desiring to take from experienced surgeons the exercise of a large discretion in this matter. There is many a cross-made fellow, many a man who is not an "Apollo Belvidere" in his person, who may yet prove an active and efficient soldier; this is, perhaps, more particularly observable in the navy, where there are many expert, vigorous, and hardy seamen, whose persons would not stand the rigid exercise of a sculptor's eye.

The feeding and the lodgment of the troops are circumstances on which their health must ever very essentially depend; and, in a recent speech in Parliament, Earl Grey, in moving for some papers connected with the administration of the army, has put this in a very striking point of view. It appears that, in some of our West India settlements, the proportion of deaths amongst the military was, for a series of years, as high as 130 per thousand; that notwithstanding reiterated representations from the medical officers of its injurious effect upon their health, salt provisions continued to be issued to the troops five days a week. That while the convicts they were employed to guard were allowed five days' fresh meat in the week, and enjoyed good health, the soldiers who were unstained by crime, were condemned to five days' salt provisions weekly, and in consequence suffered severely in their health. This was the more inexcusable, because it was found upon inquiry, that parties were ready to supply fresh meat at a cheaper rate than the salt. Again, with regard to the barracks, his lordship refers to numerous reports from medical officers, complaining of the insufficiency of the accommodation in this respect, the rooms not even water tight, the men sleeping in hammocks and allowed only 18 inches of sleeping room. While it is

somewhat discouraging to find that his lordship had so much occasion to lament the little attention which was paid to the representations, again and again made by the medical officers, he still, you will observe, refers to medical opinion as the proper authority to guide the executive in these matters. "My lords," says he, "I may be told that these disasters are the inevitable result of the climate; if it were so, it would be a frightful thing; but I say it is not the inevitable result of the climate, for, after the investigation to which I have alluded had been made, various measures were adopted to counteract the frightful evils that were ascertained to exist; and in a very few years—if not immediately within the next four years—that mortality was reduced from 130 to 53 in the thousand. But the improvement did not stop there, for, within the last ten years, instead of the deaths being 130 in the thousand, the average number was only 34."

Upon the space to be allotted to men in barracks, I fear that very limited and erroneous views still prevail. We find, upon a not very distant occasion, a distinguished general officer of chivalrous spirit, of indomitable courage, a man who was the father, the friend, and the idol of his soldiers, a man, too, of large experience both in European and in Indian warfare, talking of 300 cubic feet as a sufficient space for the soldier in barracks. I will not couple a name which I so much respect with so grave an error; but when we find an officer of this description speaking of 300 feet as sufficient space, we must think that he had yet much to learn—speaking this way, too, with reference to the construction of a barrack in India, where space is for the most part as easily procured as it is indispensably necessary, where property is in that enviable and indefinite condition which I once heard described by a dignified highland clergyman—the venerable principal of one of our universities—who spoke of the property of his clansmen of old, as consisting, not of what they *had* but what they *could take*.

I have thus cursorily noticed these few points to show you the grave responsibility that attaches to your representations, and to show you how far your range of observation must be extended beyond the common objects of professional inquiry. The province of the engineer, the architect, and the commissary, are not beyond the legitimate field of a medical officer's observation. In the introductory and in the concluding divisions of the course, you will find me referring, perhaps less frequently to medical and surgical authors, than to the "Statistical reports on the health of the army and navy," to the "Engineers' papers," and to the "Blue books" of parliament. You must not be deterred from looking into these by the jokes which have lately been passed on the practice of "pottering over blue books." Believe me, that although this may be made matter of humour by the politician, you will find in some of these books matter of grave importance to the medical officer.

Since I last met my class, several of my old pupils are on their way to Turkey. Amongst these are Deputy-Inspector Linton, the medical officers of the King's Own, and some of the young gentlemen who occupied these benches last summer. My best wishes go with them, and I make no doubt that they will acquit themselves as the surgeons of the British army always have done. Some of the more youthful and ardent spirits amongst you may think that I too, like my fellow-labourer, Mr Tuffnell of Dublin, and my colleague in the Royal Infirmary, Mr Mackenzie, should have been preparing to start for the Danube. I have been before on the banks of the Danube, and nothing would give me greater pleasure than to be there now; but a lame hand, from a painful and anomalous affection of my fingers, an impaired eye, from a recent attack of rheumatic ophthalmia, and, above all, a load of 68 years upon my shoulders, are miserable qualifications for a campaign in Turkey. I must therefore be content, for the present, to remain at my post, and to endeavour to convey to you some of that information regarding the health of the troops, of which my own experience or the kindness of my friends has put me in possession.

INAUGURAL LECTURE. BY EDWARD FORBES, F.R.S., F.G.S., PROFESSOR OF
NATURAL HISTORY IN THE UNIVERSITY OF EDINBURGH.

Delivered March 15, 1854.

"NEXT to the knowledge of ourselves," writes an essayist of the last century, "most valuable is the knowledge of nature, and this is to be acquired only by attending her through the variety of her works; the more we behold of these the more are our ideas enlarged and extended, and the nobler and more worthy conceptions we must entertain of that Power who is the parent of universal Being." In fitter words than these I cannot attempt to describe the end and objects of the study of Natural History. It is a common and not unreasonable practice with teachers of all sciences to magnify that which is especially their own favourite. Here, in this University, where the science that I am to have the great honour of professing has been so long held in esteem in all its varied ramifications, and so long taught within these walls by one of the most eminent of modern naturalists, I feel that the lauding of it is unnecessary.

The words that I have cited express the truth, and no praise can go beyond. It is right, however, that at the commencement I should state to you the views which I entertain of the scope and bearing of Natural History, the methods best calculated to promote the study of it, and the mutual relations that co-ordinate its several departments.

The distinctions, the affinities, the differences, the phases of organised beings, and their relations with geological time and geographical space—these, with the exploration of the structure of the earth's crust, are the subjects of Natural History. They constitute collectively a wide yet consistent and connected range of scientific study. They abound in excellent themes for mental exercise and matter for profound reflection. They bring into healthy play some of the finest faculties of man's mind, and they furnish abundant stores of useful as well as of interesting information. Great intellects have been employed in their investigation, nevertheless they present the peculiarity of being facile of study to a limited yet beneficial extent by the most ordinary of minds and unlettered of observers. That they afford highly valuable means for intellectual training, is an opinion now becoming strongly impressed on many of the leading intellects of the age. Here they have long been so esteemed. Edinburgh has been a great centre of Natural History study, partly from the advantages afforded for its pursuit and the importance attributed to it in this great University, and partly because the facilities of study are here abundant and accessible.

The teaching of Natural History under all its aspects can indeed be conducted with peculiar advantages in this University, owing to the admirable and extensive collection that is available for the illustration of the lectures. I hope the time is not far distant when the vast assemblage of treasures gathered within these walls shall be freely displayed for the instruction of all who wish to learn. Yet, let us never forget that to the untiring energy, watchfulness, earnestness, and devotion to science of one man this great national service is due. The chambers of our museum have years ago been filled, and every accessible recess has been used as a storehouse for additions. Every specimen is a memorial of the zeal and judgment of him who had charge of it, and a witness of the determination with which he carried out the purposes of his charge. This great collection will ever remain a monument to his memory; for, however numerous future additions may be—and I trust they will never cease accumulating—the chief difficulty and foundation of the whole has been overcome. The true use of a museum, that of teaching, was clearly borne in mind from the commencement of the formation of this. In too many instances the curators of collections seem to have fancied that the mere exciting of wonder, the indulgence of vain curiosity, and the producing an impression of surprise through excessive multiplication and bulk, are the grand aims to be kept in view in the establishments under their charge. Others appear to have con-

centrated their minds on neatness and polish—excellent qualities and indispensable, but not to be exclusively exalted, for when thus over-magnified they may stand in the way of work and arrest the course of study. These keepers resemble captains of men-of-war who regard their ships as beautiful pieces of furniture, allow no heavy treading over their shining decks, and look upon all their crew as so many polishing brushes and holy-stones. A museum is turned to its best use when it serves, as it does here, to illustrate and explain the lessons of the teacher. The more visitors it has the better for it and for science. No man, however stolid, goes through a well-arranged museum with his eyes shut, and if he looks at all he is sure to learn something. The thirst for knowledge once excited may become unquenchable. There are many of the wisest men of our age who can trace their first longings for scientific information to a holiday visit to a museum.

But here we have not only an excellent museum at hand—there is a still greater one all around us. If any spot on earth is peculiarly adapted for the study of natural history it is this—the district in whose centre we are now assembled. Everywhere about us are abundant and admirable illustrations of zoology, botany, and geology. Of its excellent and well-explored flora I leave my eminent colleague and old friend, the Professor of Botany, to speak. Amid the rich materials of its fauna I learned some of my earliest and best zoological lessons. To dredge the Frith of Forth under the guidance of shrewd, strong-handed, and strong-armed Newhaven fishermen, was an early ambition of mine, and one never too often gratified. I know the riches of the living treasures that lie in its submarine deeps, and along its shores; and though, since the time I ventured to print occasional notices of these embryo efforts, I have explored most parts of the coasts of the British Isles, and our seas far out, and foreign seas and estuaries, famous for their productions, I have not found any marine region with a population more varied within its limits, and better calculated to illustrate effectively the subjects of a naturalist's studies. Even on land and in the air there is an ample supply of species, quite enough to furnish the student with abundant means of testing the systematic characters taught in the class-room.

As to geology, where can there be a better district for practising the student in field-observation? The leading phenomena of rock-masses are brought almost to the door of our class-rooms. Sedimentary and igneous rocks contend to show us their anatomy and conformation. If a considerable part of the series of strata that constitute the geological scale be wanting, their very absence, through comparison, may be made a theme of instructive discourse, whilst there are many of the most interesting formations, both fossiliferous and unfossiliferous, within the limits of a day's excursion. The variety and beauty of the mineral contents of the igneous rocks around Edinburgh have long been famous, and have imbued every geologist, who received his early training in this University, with a respect for mineralogical evidence, and a habit of readily perceiving and using mineral characters—a great advantage. This habit must be kept up. At the same time, let us combine with it a close attention to palæontological characters. There is a fine field for fresh research open to us in the palæontology of Scottish rocks, one in which every diligent and intelligent student, who pays equal attention to the history of both kingdoms of nature, may find new sources of discovery, and of honourable reputation.

The tastes of most men can be traced back to the habits of their youth, and these habits are, in a great measure, moulded by the circumstances, physical as well as intellectual, amid which that youth has been passed. Grand scenery suggests grand thoughts, and every ennobling thought elevates, not merely momentarily, but permanently, the mind in which it glows. It is a great gain to a university to be placed like this, amid scenes of unrivalled beauty. The youth whose hours of relaxation are spent in the presence of those magnificent

prospects, so rife and many around us, carries with him in after life the memory of their beauty and their grandeur. The man who has gazed upon and felt the worthy delineation of a glorious landscape, a grand Turner or a luxuriant Claude, never forgets the genial, wholesome, glow of admiration that pervades his spirit at the time. How much more must he feel this ennobling sensation when he gazes on the reality of majestic landscape? And if, with all this precious accumulation of the vast and beautiful, there be combined that which is admirable in the minute—if nature, in her smallest elements, be prolific in objects of study and reflection, it is not to be wondered at that this University has been a hot-bed of naturalists, and that their philosophy has been one catholic in essence and far-extending in its range. To cherish this philosophical spirit, combining at once broad views and minute research, basing all its theories on well-ascertained and thoroughly-sifted facts, will be my endeavour, as far as my strength will permit, whilst I occupy this chair. I know too well that it is beyond the power of any man to keep pace with every step the science of Natural History is now taking. But we can track the course of a giant's march, and demonstrate the measure of his progress. The chief result aimed at by the teacher is not so much the filling his pupils' mind with a crowded accumulation of facts, but the preparing it for observing and comprehending them. My own experience hitherto has gone to show that, in proportion to the prospect of a philosophy arising eventually out of the instances presented to him, so much the more attentive and anxious to learn is the student. If well grounded in the principles of the science he is learning, he rapidly appropriates the practical *data* afterwards presented to or noticed by him. And this is as true of Natural History as of every other science.

By the principles of Natural History I mean the statement and exposition of those fundamental laws and general facts that constitute the basis of biological, and, in a great measure, of geological science. We have of late years been so accustomed to extreme division of intellectual labour, to the accumulation of materials, and the investigation of details in one department, without consideration of their relations to those of another, that the grander and co-ordinating sciences have risked being forgotten, and certainly have been too much neglected. Men of mind have so distributed themselves among the lesser ologies—have confined themselves, as it were, within the domestic life of the subdivisions of knowledge—that they have almost lost sight of the statesmanship of science, and ignored the existence of a philosophy. In these times we are too apt to mistake knowledge for science, and to call labourers philosophers. It is a bad sign, and an indication of narrowness of intellectual vision, that the word metaphysical should be used as an epithet of reproach. Of the sciences, in the true sense of the designation, that have suffered most from contraction, Natural History is one. In the days of Linnæus, and before the time of the mighty Swede, in the days of our own Ray, the naturalist, whilst he laboured in the field, sought to extend his view over the expanse of the vast regions of science of which he proposed to be an explorer. The first sketches of these great mind-artists showed the rock in the foreground, and the mountain in the distance; the herb among the stones, and the mass of vegetation that gave verdure and clothing to the landscape; the creatures browsing and bounding over the ground, careering in the air, and sporting in the pool. The pervading and amalgamating harmony was displayed along with the many-sidedness and minuteness of nature. The necessity of working out the multiplicity of facts drew down the naturalist from high places, and he has scarcely yet got out of the ravines and jungles. We must go up the hill again, and take our bearings from the mountain top, ere we can penetrate, with certainty of advancing, the thick and tangled forest of facts still untraversed. Nature is a labyrinth; but all labyrinths have their plan and key. To make the one out, and get possession of the other, the curious must plunge into blind alleys, and come back upon old starting points; but the time thus spent has not been passed in vain, for every avenue and close once

worked over and understood become advances, even though they seem impediments. But to plunge among the myriad details of nature, organic and inorganic, without a conception of the laws and leading ideas at which we have arrived, or can arrive, would be like entering an untravelled region without a map—an inexcusable loss of valuable time. Therefore do I strongly uphold, as the main duty of this my office, the exposition of the principles of Natural History.

The comprehension of the law must be based upon the understanding of the instance. Correct observation is the foundation of Natural History. Not a few writers and speakers upon education use language that would seem to imply that the end as well as practice of Natural History is observation, and observation only. It is really but the means to the end—the corner-stone of philosophy, indispensable and absolutely essential. If we part with it for a moment, we float into the region of dreams. Our most abstract generalisations must be based upon the results of accurate observation, or they are worthless. Simple as the act may seem, to observe correctly is one of the rarest of accomplishments. The naturalist knows too well how seldom any evidence offered by an untrained person is of the slightest value. Man is the most imaginative of animals, and the more ignorant and untrained the more visionary and fantastic are his notions of what he sees about him. In proportion to his want of education, so is he the less satisfied with noting the plain fact. Every object that attracts his attention becomes transformed and mystified. And even when what is called educated, he is not much better in this respect, for though all pains are taken to train his reasoning and æsthetic faculties, his observing powers are left undeveloped. Whilst we have at hand so excellent an educational agent, adapted above all other sciences for remedying this defect, the neglect of naturalists is inexcusable.

It is this training of the mind in correct methods of observation that gives the Natural History sciences so much value as instruments of preparation in professional education. Not unfrequently do we hear the short-sighted and narrow-minded ask, what is the use of zoology, or botany, or geology to the physician and surgeon? What have they to do with beasts, or plants, or stones? Is not their work among men, healing the sick? Of what use, save as remedies, can the creeping things, or the grass that grows upon the earth, or the minerals in the rock, be to the practisers of medicine? Vain and stupid questions all—yet they are sometimes put by persons who profess to promote the spread of education. We hear the same outcry on the literary side of teaching. What is the use of Greek and Latin? Can Greek make a man successful in bargaining, or Latin add to his riches? Why teach philosophy—the world is not for philosophers? What is rhetoric to the farmer? Who has not heard these questions asked over and over again? Yet always by professing advocates of education—practical education. They want something, but the best of them mistake the end for the means. The best want knowledge, but have not learnt that the mind must be trained ere it is prepared to gather and digest knowledge. They want science, but science turns mouldy and unwholesome in an unprepared mind. They forget, or do not know, that education consists chiefly in training, not in informing. That is instruction. At the same time, without a due mixture of instruction, education becomes insipid and distasteful to boyhood and youth. The older the pupil the more instruction must be mingled with the teaching. And when we are professionally educating young men, then the more science we can instil through our educatory lessons the better for them. Were the sciences so infused, to be entirely professional, we should warp and contract the minds. The tonic would be too strong—would not invigorate, but corrugate. We must counteract the natural tendency of purely professional studies—the tendency to limit the range of mental vision. We can do this most beneficially through the collateral sciences, which are sufficiently allied to the professional ones to prevent an undue dissipation of the student's thoughts, and at the same time are

sufficiently different to give them a wider sphere of action. It is in this point of view that we should regard the Natural History sciences as branches of medical education. For my own part, after much intercourse with medical men who had studied at many seats of professional education, some collegiate, some exclusively professional, I have no hesitation in saying that, as a rule, the former had the intellectual advantage. There are noble and notable exceptions, old and young; but the rule is true in the main. The man who had studied in a seat of learning, a college or university, has a wider range of sympathies, a more philosophical tone of mind, and a higher estimate of the objects of intellectual ambition than his fellow-practitioner who, from his youth upwards, had concentrated his thoughts upon contractedly professional subjects of an hospital school. I will not believe that the practitioner of medicine, any more than the clergyman, or the lawyer, or the soldier, or the merchant, is wiser or better able to treat the offices of his calling, because his mind takes no note of subjects beyond the range of his professional pursuit. It is a great pleasure, both to patient and neighbour, to find in our doctor an enlightened friend—one who, whilst he does his duty ably and kindly, has a sympathy and an acquaintance with science, or literature, or art. Such men have gone forth in numbers from this University, and have done much to contribute to its fame. May there be many of them sent out for centuries to come. Of those who come here to study professionally, there are not a few who may some day find themselves isolated in distant and little-explored regions. Far away from friends and the conversation of intellectual companions, any pursuit which can engage and occupy the mind, and above all satisfy its thirst for truth by draughts from the pure and refreshing fountains of nature—any such pursuit becomes a blessing, and converts the desert into a paradise, one often filled with creatures yet to be named. How delightful does it then become to be able to recall the lessons of our student days, and, casting away regret and languor, invigorate our minds by the practice of healthy intellectual exercise. Through no branch of knowledge can this be attained more easily and more excellently than through natural history.

In all directions around us the subjects for our study abound. We require few instruments, and those seldom of a costly character. A few books will serve as notes upon the great volume of nature, whose pages, after all, are the best manuals as well as the fullest cyclopædias. The cultivation of good, strong, straightforward common-sense—that sort which generates a logical frame of mind—is the best preparation, and one which is neither rare nor technical. Faith in science—a just but not blind trust in authority—a firm determination to be cautious and careful, and a resolute and patient spirit of perseverance—these are the qualities that will ensure success for our endeavours. Through them we may advance to discovery, if we have the will, power, and opportunity of doing original work. If our aims be less ambitious, we may, by turning them to good account in our study of animated and inanimated nature, exercise our minds in habits of minute and accurate observation, of systematic comparison, and of philosophical generalisation. We shall find ourselves in the end to have gone through an education of a kind that can qualify us for the strife of life and the ways of men, far removed as they may seem from the subjects of our studies, and as surely prepared for scientific investigation of no light order. It matters little how few the facts studied may be, provided they were sufficient. It matters less whether the knowledge so acquired is useful, in the vulgar sense of the term, or not. It matters much, very much, that we have gained in mental training, and that, through pursuing the line of study just indicated, we have become prepared for understanding the facts and phenomena of the world about us, and acquired a capacity to comprehend the laws and the three great kingdoms of nature.

But are the lessons learned in the study of nature to be confined only to our physicians? Are the worlds of life, animal and vegetable, to be explored only

by the healers of diseases? Are the grand chapters of the earth's history—of God's work in the old pre-Adamite time—to be sealed books to the multitude? If sound training in observation be, as all thinkers must admit, an important though neglected element of a complete education, why confine it to professional students? Who, that has studied the structure of an insect or a flower—who, that has hammered out a fossil from a rock, will not say that he or she felt wiser, and better, and purer, after the work? There must be good in this kind of learning—good, moral and intellectual. Every object that the naturalist examines is a divine thought—every abstraction that he eliminates from his observations is not born of his own mind, but is a divine idea. The universe is not God thinking, as the Pantheists would have it, but the thought, and, therefore, the work of the Creator. What finer, wholesomer, sounder educational exercises can there be than those we set to the student when we bid him observe and reflect upon the phenomena of nature? Whatever be the proposed pursuit of the youth, or the present occupation of the man, the mind, adolescent or adult, will expand through this noble study.

We live in an age when Natural History must necessarily flourish. Formerly the tediousness and expense of correspondence, the tardiness of modes of transit, the dearth and fewness of books, all combined to prevent a spread of the taste for Natural History science. Now this is changed. The very sea-worms now visit our great cities, and delight by their strange and fantastic beauty the gazer who, some four years ago, would have scouted the idea of finding either pleasure or profit in the contemplation of their odd and pulpy charms. The poet, the painter, and the naturalist may now sit side by side, satisfied with the self-same purpose, that of carefully and faithfully without forethought or malice or bias towards one party more than another, observing through close and earnest observation, that which is in nature or springs out of it, and no more.

In the history of all branches of learning, we cannot but perceive something more than the accidents of progress. Each has its epoch of development—its right time for flourishing. Each has its appointed age, as much so as every great tribe of animals has its destined geological period. The elements for successful study gradually appear, accumulate, multiply, converge, and combine—and then springs up the science; scarcely noticed during the epoch of its growth, strong and manifest at the period of its maturity. One science after another has thus grown up—some speedily, some tardily. Those now fast coming of age are the biological and the geological sciences. A science born before its time pines away. This was the fate of zoology. The master-mind of Aristotle called it forth, like a spirit summoned by the wand of a magician to speak out some great truths, to tell how its secrets ought and might be discovered, and then to vanish and be forgotten. It is only towards our own times that we find the biological sciences growing great. More knowledge of the world's productions and greater facilities for a peaceful examination of them than could be commanded by the wise men of the old time were required before these sciences could get the nourishment requisite for their growth. Still younger is geology. It could not be until its precursors had become adult. Zoology, botany, mineralogy, chemistry—all had to be grown up before geology could be born. It did not come until the time was ripe for it. It has grown with unexampled rapidity; yet its strength is co-equal with its magnitude. And the nearer it approaches maturity the more plainly does it take its place in the circle of Natural History.

We are too apt to fancy that the world of life and the world of minerals are separate and distinct stars in the great constellation of science. The student of the one is too much and too often separated from the student of the other, and each is too apt to be wrapped in his favourite and fragmentary portion of natural science. Yet in Natural History, properly so called, in the philosophy of the world organic, as contrasted with the world inorganic, there can be no such disunion of sciences that have been bound together for all time by the

Great Architect and Originator of Nature. Earth and heaven may be far apart, but earth and life are through all the aspects of visible existence intimately interwoven. Not merely in the bodies of living things, but also in their spirit and ways do we find the mineral influence powerfully struggling. The features of Faunas and Floras, the presence or absence of their elements, the degree of individual development, or it may be abortivment of earth-living animated creatures, depend in no slight degree on the confirmation and constitution of the soil. The physical qualities of organised beings are curiously and evidently, though not always directly, modified by the mineral structure and superficial contour of the land surface or sea-bed upon which they live. The sentiments and habits of man himself could easily be shown to assume unconsciously peculiar phases according to the configuration and qualities of our mother earth.

The link between the organic world and the mineral kingdom is maintained by geology. This beautiful science, dealing with the absolutely lifeless on the one hand, and the life that has been on the other, deciphers the ancient history of our planet in the organic hieroglyphics graven on its building-stones. But the characters inscribed by the hand of the Creator of all things are not like the writing on the monuments of men, mere symbols and signs of objects and thoughts that have passed away; they are the things themselves that were, the embodied conception of the great Omniscient Thinker, the skeletons and impressions of the actual creatures that lived and died, each in its appointed epoch. Through them we get the measure of time and the history of the preparation of the world for the advent of man. Two great classes of rocks, antagonistic and conflicting in their origin, make up the substance of the globe. The one, fire-born, have been derangers and disturbers, but, though interrupters, not the less parents of progressive changes and suppliers of materials for the construction of more tranquil formations. The other, deposited beneath the waters, or accumulated under the influence of atmospheric agencies, contain within them the records of primeval epochs and the relics of pre-Adamite populations. Opposite, in their essential nature, as are war and peace, each is equally worthy of study, and to understand truly the history of the one we must master the history of the other. The chronicler of the development of nations who should attempt to record their phases of quiet, and of intellectual and moral advancement, without making mention of their revolutions and periodical outbursts of fierce passion, would tell a fabulous and untrue story, even though his object were merely the illustration of human progress. So the geologist, who should confine his description to the sedimentary strata, and take scanty notice of the igneous rocks that distort or dislocate them, would be telling only half the story, and, as with all half-told tales, mislead and misinform.

Nevertheless, the grand end and aim of geology is the history of life in time. The measure of the value and force of disturbing influences and the estimate of the energy of action of igneous irruptions, are only to be obtained with probability through the evidence of the amount of organic change. This we cannot too constantly bear in mind. We shall never understand geology philosophically, whether we regard the science under its palæontological, its dynamical, or its mineralogical aspects, unless we remember this fundamental maxim. To arrive at the knowledge that is necessary to enable us to work out geological problems surely and safely, we must make ourselves acquainted in the first instance with the principles of Natural History. All the great geologists of our age have found out this—with whatever different opinions they may have commenced their studies. The fossil and the mineral are both letters of the geological alphabet, and the first is a letter that cannot be safely written without Natural History knowledge on the part of the scribe.

The naturalist whose studies are zoological or botanical has gained enormously through the aid of geological research. Between him and the geologist the debt is mutual. The organised creation that surrounds us in the time present is but a

tythe, and a small one, of the entire *Systema Naturæ*. It were vain to pretend to demonstrate the course of affinities and relationship of groups through the Faunas and Floras that are now, and through those only. Were the assemblage of living creatures now peopling our earth and its seas a system of beings wholly distinct from any that preceded it, then might the zoologist and botanist rest content with the forms of life as they are. But there is no truth more certain in science than that which has been demonstrated through the discoveries of geologists, which have made sure to us that the present creation is part and parcel of more ancient manifestations of life in nature, and that the types of being, recent and fossil, belong all, without exception, to one great general divinely-originating scheme. To illustrate the living we must call up the dead; to understand the dead we must compare the extinct with the existing. The time has passed when the naturalist who worked among recent creatures rather boasted than otherwise of ignorance of the fossil. The palæontologist, in like manner, has now found that to comprehend organic remains he must examine and understand their existing relations.

Palæontology, the right arm of the geologist, is in fact a part of zoology and botany—not a natural, but an artificial section; a term of convenience given to a convenient subdivision of study, not because it is indicative of a true distinction, but because it is useful for empirical exploration. It is a pseudoscience, inasmuch as it is necessarily fragmentary and incomplete in its treatment of its objects. Not the less is it important, since it is supplementary yet necessary to complete understanding of either zoology or botany. It deals on the one hand with extinct forms of animals and plants; on the other, with the relation of these organic remains to rock formations. To work it rightly under its first aspect biological knowledge is absolutely necessary. Fossils are mere signs and counters if treated without the requisite preliminary acquaintance with Natural History on the part of the describers. To distinguish and define with precision the species of extinct creatures, a careful comparison of them with their living allies, whether proximate or far removed, must be made by their diagnost. To apply them with safety to the determination of the ages of rocks, then he should possess an acquaintance with the leading principles of pure geology. Thus through palæontology a close and indeed inseparable alliance between biology and geology is maintained; and I, for one, deliberately hold that the learner of geology should be first conducted, step by step, through the principles of Natural History.

All branches of Natural History may be regarded under another and more popular point of view than that which I have taken of them—one, however, that I am far from holding in disdain; nay, on the contrary, believe to be highly worthy of development. This is their economic aspect. Too much is probably expected from this their utilitarian phase by persons who have put faith in the turning of the sciences to profitable account. Zoology will hardly enable us to extend the list of our domestic and useful animals, or botany the number of distinct species of cultivated culinary herbs—nor can geology give absolute certainty to our speculations in mines, or even demonstrate to the satisfaction of the bench and bar what is coal. Nevertheless, they can all be serviceable—and especially the last. If we cannot always through our geological science tell men how they may make their fortunes by working among rocks and mines, we can at least teach them where they need not waste their money.

But the hour is shortening, and I must conclude. After many years of study, and travel, and precious opportunities for acquiring experience, I return to the city where I was first initiated into the science of nature, and where within these walls I learned those lessons of patient inquiry and minute observation, to whose working and training I am indebted for the place that I now hold among the Professors of my *Alma Mater*. To my illustrious predecessor and master who passed from amongst us ripe in years, honours, and fame, so lately, I gratefully record my acknowledgments for the encouragement of those tastes and the founding of that knowledge which have proved

to me a chief delight. Who, that in time past was his pupil and found pleasure in the study of any department of Natural History, can ever forget his enthusiastic zeal, his wonderful acquaintance with scientific literature, his affection for all among his friends and pupils who manifested a sincere interest in his favourite studies. When, in after life, their fates scattered them far and wide over the world, some settling amid the civilised obscurity of rural seclusion; some rambling to the far ends of the earth to sift and explore wild savage regions; some plunging into the boiling and noisy whirlpool of metropolitan activity; none who remained constant to the beautiful studies of his pupilhood was ever forgotten by the kind and wise philosopher, whose quick and cheering perception of early merit had perpetuated tastes that might have speedily perished if unobserved and unencouraged. The value of professorial worth should chiefly be estimated by the number and excellence of disciples. A large share of the best naturalists of the day received their first instruction in the science that was afterwards to prove their fountain of honour from Professor Jameson. Not even his own famous master, the eloquent and illustrious Werner, could equal him in this genesis of investigators. Under his auspices, too, were lasting friendships and unions of kindred minds formed that have been productive in good to the cause of knowledge. Valuable as were his writings—each when estimated with regard to the position of science at the time of its issue, an effective advance—his pupils were even more valuable. The greatest praise of a great Professor is that which proclaims he has founded a school. And where else in the British empire, except here, has there been for the last half century a school of Natural History? I have a difficult task before me, gentlemen, with those traditions of greatness to overshadow my endeavours. But since this most honourable and responsible office has been placed in my charge—the highest to which a British naturalist can aspire—with God's blessing and your assisting exertions I will endeavour fully and fairly to do my duty.

Part Fourth.

MEDICAL NEWS.

DR GRIEVE ON THE CHOLERA IN DUMFRIES.

(To the Editor of the *Monthly Journal of Medical Science*.)

SIR,—Permit me very briefly to allude to one or two points in Mr Blacklock's communication in the current number of your Journal. Mr B. alleges that I am "altogether wrong" in stating that, in 1848, "no consistent plan of superintendence was organized until the arrival of Dr Sutherland." I again repeat my former statement, which is literally correct, that, on the outbreak of cholera in that year, no regular and reliable reports were furnished to the Board, and *no consistent plan of superintendence* was organized, until the arrival of Dr Sutherland, when Mr Blacklock and myself were appointed superintendents.

As regards the Clarencefield case, it so happens, that when engaged, in 1849, in preparing my Practical Observations for the *Medical Times*, I requested a gentleman, residing near that village, to investigate for me this *pet case* of the contagionists, and, in reply, he informed me that, after taking great pains impartially to sift the facts, he found that the weight of testimony was against Mr Blacklock's statement of Mr Smith never "having been within six miles of us;" and, on the contrary, that it was generally believed that Mr Smith

had been in Dumfries a day or two before he was attacked with cholera. Being aware of this, I considered the case a neutral and valueless one, and, on this account, refrained from mentioning it. I have since written to my informant (Mr Scott, Clarencefield Academy), and in his reply, dated the 11th inst., he says: "He (Smith) told me on the morning previous to his death, that he intended to be in Dumfries on that day, for the purpose of obtaining information how to treat cholera." Mr Scott further says that, although Smith's widow is unaware of his having been in Dumfries, that he considers this no decisive proof of the fact, as "many of his (Smith's) movements," which were erratic, "were not known to his family, and he could easily be there without their knowledge." All this is fully corroborated by Mr Halliday, formerly schoolmaster in the neighbouring parish of Mousewald, who was also my correspondent at that time, and with whom I conversed on the subject yesterday.

I now bring to a conclusion this correspondence. My object has not been a love of controversy or dispute, but solely a desire to elicit facts, and arrive at the truth.

I have endeavoured to show that, in both epidemics, the phenomena of the disease in Dumfries militated against the theory of its contagious nature; and I have taken special care that all my statements were well founded and authentic, and hence I can fearlessly say that none of my assertions were "unfortunate mistakes," and that in none of them I was "altogether wrong."—I remain, yours faithfully,

J. GRIEVE.

Dumfries, 13th May 1854.

THE PRESENT PROSPECTS OF MEDICAL REFORM.

Since the publication of our last number, Mr Brady's Registration Bill has been lost, the House of Commons having decided on the 10th ultimo, by a vote of 118 against 69, that it should not be committed. We have also received various accounts of the Conference held in the London College of Physicians, on the 25th of last April. Reporters were not allowed to be present, which, in our opinion, was a most unwise proceeding. A very able account of it, however, has been published by Mr Ancell and Dr Cormack, in the name of the delegates sent by the Metropolitan Counties Branch of the British Association (*Association Journ.*, May 12th). From this document it appears that the Bill, which its framers had fondly imagined would constitute emphatically the Bill of the profession, met with the most determined opposition, not only from the great majority of the medical corporations of the kingdom, but even from a portion of the Provincial Association itself. Thus, the College of Surgeons of England would not send a delegate to attend the Conference; the delegates from the Universities of Edinburgh, Glasgow, Aberdeen, and St Andrews, from the London Apothecaries Company, from the Royal College of Surgeons of Edinburgh, from the Faculty of Physicians and Surgeons of Glasgow, from the Apothecaries Company of Ireland, and from the Metropolitan Branch of the Association—all objected to the Bill. On the other hand, the delegates from the Universities of Oxford and Cambridge, without instructions to support, said that they believed it would meet with the approval of the Universities; and the delegate from the Royal College of Physicians of Edinburgh gave a qualified assent to the Bill. The latter was the only institution in Scotland which in any way supported this unhappy measure.

With regard to the meeting itself, a discussion first ensued as to what was its true object—a consideration of medical reform generally, or of the Association Bill in particular. The chairman was not furnished with any programme, neither had any propositions been prepared to submit to the meeting. Such motions as were made were not seconded, and fell to the ground; and after

three hours of desultory conversation, the meeting concluded without effecting anything. The general principles which are universally allowed to constitute the basis of all medical reform, were never called in question ; but with regard to the method of carrying them out, the most contrary opinions prevailed. On the whole, it was rendered evident that so far from recent efforts having succeeded in producing harmony in the profession on this all important subject, as much contrariety exists as ever. Again, the total want of management and arrangement which prevailed, the exclusion of reporters, the indefinite character of the proceedings, and the conclusion, in which nothing was concluded, indicate a remarkable want of capacity and forethought in those who summoned delegates from various parts of the kingdom. One great good certainly has resulted, and that is the irretrievable ruin and virtual withdrawal of the Association Medical Bill.

The universal question now is, What is to be done ? There is no bill before us, and yet the government, through the home minister, has expressed its willingness to help us, if we will only help ourselves. Surely the medical profession, as a body, is not so stupid and unreasonable as to be incapable of coming to a definite conclusion. Far mightier problems in political economy have been solved than this. What seems to us to be required is a directing power, a something which will lead the heads of departments and corporate bodies to unite for a definite result. This we think might at the present time be easily effected in the following manner.

It is now everywhere acknowledged that three things are requisite to carry out a sufficiently comprehensive measure of medical reform, viz., 1st, Uniformity of education ; 2d, Uniformity of privilege ; and 3d, A check to unlicensed practitioners. If government and the profession would accept these as the basis on which a new bill should be framed, then the former might appropriately summon a committee of medical men for the sole object of determining, not what the principles should be, but how those named are to be carried out. This would render the duties of such a committee definite, and if it fairly represented the profession, and had for its object the framing a plan of medical reform for the home minister, the task could certainly be accomplished. The plan so formed should include a *minimum* course of study, which every institution that proposes to grant licenses for general practice should be bound to enforce. This should be subjected by means of the medical press to public criticism, and such of the medical boards as consider themselves aggrieved (and it is in vain to suppose that some will not), should be carefully listened to, and their objections as far as possible met. Then there should be a second and larger meeting, attended by delegates from all grades and corporate bodies of the profession summoned to discuss the plan brought forward by the committee ; and finally, when amended, to place it in the hands of the minister to receive the sanction of the legislature. It will perhaps be subjected to further criticism and amendment in passing through the houses of parliament, but in this manner we may have reasonable hopes of seeing some measure, which will relieve the profession of the intolerable ills which oppress it, fairly carried out and acted on. Let us get rid of the Utopian idea of endeavouring to please everybody, and only desire to see a useful measure that shall receive the cordial support of the great majority. Experience has at least demonstrated that this is not to be arrived at through any bill emanating from a particular section of the profession ; from any one medical corporation ; from a small constituency, or even from the most able minister alone. All these parties must be united, and the plan we have proposed seems the only feasible one left us to adopt.

With regard to the plan of medical reform which might ultimately be adopted, we shall not now speculate. But there is one subject which we think ourselves bound to enter upon, partly because it is not very clearly understood, and partly because it is one which more than any other is difficult to adjust. We allude to the respective educations of the general practitioner and of the

doctor of medicine. According to some, so long as the Edinburgh doctorate can be held at the age of twenty-one, whilst the English one cannot be obtained before the age of twenty-six, reciprocity of rights is impossible. It is certain that to secure this, the age, as well as the education made essential for the degree, should be everywhere alike; but we venture to defend the practice of the Edinburgh university, as having been more serviceable to the public than that of the English universities, and for the following reasons.

It must be evident that what chiefly concerns the public is the education of *general practitioners*. This, we maintain, is better managed at present in Scotland than in England, because the qualification for the degree of M.D. is not placed so high above that for the lowest degree on which a man can enter on general practice, as to be beyond the reach of the great body of those who are to live by it. Many of the best men in the profession are, and always will be, in straitened circumstances when young. If those who now qualify themselves for the degree, had been told, on reaching the age of twenty-one or twenty-two, that they must wait four or five years more before they could receive it, their funds, in most cases, would not have allowed them to do so. They must therefore have entered on practice without that degree. In such a case, it certainly would not have been worth their while to make any preparation beyond that necessary for the lowest grade on which the profession can be practised. But in consequence of the regulations of our University, keeping the qualification and examinations requisite for the degree of M.D. in it somewhat higher than that required for the diploma of surgeons here, yet not so high as to be beyond the reach of the great body of those who are to live by the profession—and demanding no qualification by age beyond the legal one of twenty-one—we believe that the mass of general practitioners throughout the country are better educated than they otherwise would have been. It is in this manner that the Edinburgh system has been the means of providing for the public service, and scattering through the country able medical men, who practice generally with the degree of M.D., to the great benefit of the public, and to the honour of their Alma Mater. A certain number of them even have, in due time, become worthy consulting physicians.

Now, any measure of medical reform which places such a system in jeopardy, is much to be deprecated, and ought to be received by the legislature with extreme caution. It is to the qualifications of the general practitioners that it is chiefly incumbent on the government to look, for they are the practitioners who treat the early and most remediable stages of disease, and if they are generally well informed, no trouble need be taken as to the qualifications of consulting physicians. This will be sufficiently cared for by the colleges to which physicians belong, whose duty and interest it is to attend to the dignity and status of their fellows. Certain it is that well informed general practitioners will not (at least generally or permanently) submit to the guidance of men whom they do not know to be at least equal in information to themselves. This was clearly perceived by the late Lord Durham, who, in a discussion on this subject in parliament some years ago, urged strongly that the peculiarity of medical practice in this country *chiefly requiring correction* was the *small* number of men engaged in it, who possessed the highest degree in medicine, as proportioned to the whole number of practitioners. Now, this state of matters is more applicable to England than to Scotland, and would be perpetuated and extended by any measure which should confine the degree of M.D. in all parts of the country to men who have attained the age of twenty-six.

Much has been said of the injury *done to the profession* by the highest honours of medicine being given away too cheaply and easily to men whose manners and habits are not creditable to their calling, especially by the Scotch universities. We do not deny that this is an evil, which is, however, less to be charged against Edinburgh than the other Scotch universities. But it is not to be compared to the injury *done to the public*, by shutting the door against the highest honours to all who, during their education, cannot command a

certain amount of funds. In order to meet this evil, however, it might be enacted that no university should give the degree of M.D. without requiring, not only the whole of the minimum course of study imposed upon general practitioners, but one additional session of study to be taken in a university. In this way those universities which now grant a degree without any additional study, and without any university residence, would be assimilated in this particular, to the University of Edinburgh, the degree of which is everywhere respected, and held by some of the most eminent members of the profession.

BIOGRAPHY OF THE LATE PROFESSOR JAMESON.

ROBERT JAMESON was born at Leith in the year 1773. His earliest years were marked by indications of his future career, and his tastes encouraged by a kind father (a merchant), who was wont to boast that ships brought curiosities from all parts of the world for his son Robert. The future Professor commenced the studies of his youth with a view to the pursuit of medicine. He served an apprenticeship with the late Dr Anderson of Leith; and also was a student in the University of Edinburgh. But his enthusiastic love of nature alienated him from professional studies, and he never graduated. Able to follow his own tastes, he commenced very early to engage in original research. When only 24 years of age, he published his "Outlines of the Mineralogy of the Island of Arran and the Shetland Isles." His work at once won for him a deserved reputation, and served as the best of introductions to the naturalists of Germany, whither he proceeded soon afterwards to study mineralogy and geology, under the famous Werner at Freyberg. There he attended the prelections of the great German geologist in company with Mohs, D'Aubuisson, and others, who have since become distinguished. Humboldt and Von Buch had preceded him. For Werner he retained ever afterwards an enthusiastic admiration, and for a long time was the ablest defender of the peculiar theoretical views of the great German, whilst he effectively promoted the advance of geology by introducing the methods of minute observation and precise discrimination which characterised the school of Freyberg. The theories of Werner have perished or become transmuted; but the services rendered by the practical training in which he educated his pupils have left a deep and most beneficial impression on geological science. On his return to Scotland, Mr Jameson resumed his local investigations, and in 1800, published his important work entitled the "Mineralogy of the Scottish Isles."

In 1804, the Rev. Dr Walker, Regius Professor of Natural History, died, and Mr Jameson was appointed his successor, and keeper of the Museum of Natural History. Dr Walker was a remarkable man, and a good naturalist; but his reputation was soon eclipsed by the fame of his successor. In 1805, Professor Jameson projected a great work on the Geology of all Scotland, intending to describe county after county, and published as a first instalment, "The Mineralogy of Dumfries." This may be regarded as the precursor of a geological survey of Scotland, and was a project that plainly shows how much its author was in advance of the time. His excellent treatise on the "External Character of Minerals," and his "System of Mineralogy and Geognosy," followed in rapid succession. The mineralogical portion of the latter work, much enlarged, was re-published in 1816, and has formed the basis for most of the treatises on the natural history arrangement of minerals. In 1817, he sent forth a translation of Cuvier's "Essay on the Theories of the Earth," with Notes—a publication that powerfully promoted the study of geology in Britain. In 1821 appeared his "Manual of Mineralogy, and of Mountain Rocks," and he contributed afterwards mineralogical treatises to the Edinburgh Encyclopedia, and the Encyclopedia Britannica. The article in the latter great work was published separately in 1837. He contributed also not a few papers on Descriptive Geology, especially of the Arctic regions, to various voyages

and geographical works; and in the Transactions of the Wernerian Society, of which he was the founder, occur no fewer than thirteen memoirs by him. Among them are some which show how earnestly, in the earlier stages of his career, he entered upon zoological researches. Indeed, he contributed much towards laying a foundation for the investigation of the highly interesting fauna of the Scottish seas. To the last he was as fond of zoology as ever, and well informed of all that was being done for its advancement.

The foundation of the Wernerian Society was itself a valuable service rendered by him to his favourite sciences. The old Natural Society of Edinburgh had languished for several years, and the new Society was constituted on its remains. The Wernerian elected Professor Jameson its first president, and he continued in office to the last. It did good work in its time, and its vitality was in a great measure dependent on the energy of its founder.

When the conflicting merits of the Neptunian and Plutonian theories convulsed the world of science, Edinburgh was the great centre of geological controversy. The supporters of Hutton and the followers of Werner were ably represented in the Scottish metropolis, and each party appealed confidently to the phenomena exhibited in profusion by the rock-formations of the vicinity. Professor Jameson was the leader of the Wernerians, and a most able chief, for he had enjoyed the advantage of being indoctrinated by Werner himself, and in minute acquaintance with the mineral characters of rocks was unrivalled. He fought on the losing side, but did much good service to science by his opposition to the winning theory, for facts became in consequence far more carefully studied than they otherwise would have been. When he had satisfied himself that the views he had supported were in part too exclusive, he was the first to come forward and declare his change of opinion, particularly in different meetings of the Royal Society, with perfect candour and love of truth. Eventually his opinions became greatly modified, and he might be styled an eclectic geologist. He kept pace in a surprising manner with all that was doing, and appreciated justly every kind of evidence. It has often been said by those who did not know him, that he held paleontological evidence in disdain, and banished fossils from his sight. There could be no greater mistake. To the last he accumulated and arranged organic remains with as much care as his favourite minerals, and, at the present moment, there is in the University Museum as fine a *teaching* series of fossils and models of organic remains as could be desired; all, too, arranged for the purpose of instruction.

Another great benefit conferred by him, originally in conjunction with Dr, afterwards Sir David Brewster, was the establishment, in 1819, of the Edinburgh Philosophical Journal. The editors separated afterwards, and the journal was converted into two. That which Professor Jameson retained, under the name of the Edinburgh New Philosophical Journal, flourishes still in the north, and is one of the very best scientific periodicals of the day. He edited it to the last.

When he came to office, the Edinburgh Museum was but a name. He left it one of the finest collections, in every department of natural history that have ever been brought together. He devoted himself, heart and soul, to its organization and promotion.

Doubtless, his fame must rest chiefly upon his reputation as a mineralogist. He regarded mineralogy under its natural history aspect, and as an element of geological science. His version of the study has been objected to by those who consider mineralogy as a section of chemistry. But whilst never undervaluing the investigation of the composition of minerals, and the classification of them that results from a comparison of their chemical characters, he held mineralogy to be practically valuable as an assistant to the geologist and miner. Therefore he laid stress upon the determination and definition of mineral species through their external characters, knowing as a field geologist, that by these chiefly do we recognise the peculiarities of rocks, especially of igneous forma-

tions. All the many geologists who were educated by him appreciate and apply mineralogical distinctions, and never neglect the recording and observing of them. In the class-room, minerals were thought by many to occupy an undue share of the course, and more than was necessary for this section of his subject, but all appreciated the admirable field lectures which he gave during his excursions to the leading geological localities around Edinburgh. These excursions did more towards the making of geologists than any instruction that has been given within the century. Well do we remember his slender and wiry form erect on the summit of Arthur's Seat, his right hand grasping a hammer, and the crags and grassy spots around him covered with an admiring audience of enthusiastic students, among whom might be seen often veterans of distinguished eminence and rising celebrities from all parts of the world. Then in perspicuous language he would expatiate on the structure of the beautiful and curious hill upon which he stood, and of the country far and wide spread out like a map before him. To those who heard him, that which at first seemed but a beautiful prospect, soon became a great geological chart and an index to the structure of the whole world. During these Saturday expeditions he made the personal acquaintance of every student and amateur member of his class who displayed a knowledge of, or love for, natural history. Friendships between professor and pupil thus arose, which eventually extended over years, and led to a voluminous correspondence. His advice was eagerly applied for and readily given, and many of the best scientific writings of his time owe some part of their excellence to his suggestions and revision. The men who became thus attached to him never forgot the college in which they had studied, and from time to time sent valuable and interesting specimens and collections to be applied by their old master to the development of the University Museum.

As a lecturer, he laboured under certain disadvantages, on which it is only doing justice to his memory to reflect. His voice was weak, and his manner of speaking studiously dry and unimpassioned; for he not only disdained the imputation of making any effort *ad captandum vulgus*, but seemed always desirous of letting it be known, that he took for granted in his audience the same interest which he felt himself in the observation and interpretation of nature. But, to those who really felt that interest, and who bear about with them the recollection of his distinct, but quite tranquil mode of expressing himself, it served as the assurance (particularly to all who availed themselves of his invitations to private intercourse) of a uniform willingness and even anxiety to communicate all the information in his power, and promote all inquiries conducted with a sincere love of truth. These peculiarities of his manner now only serve to give individuality and precision to one of the most pleasing recollections which present themselves to the minds of his old pupils, when they withdraw their attention for a time from the toils and contentions of busy life, and bethink themselves that

"Life had its May, and all was mirthful then,
The woods all music, and the flowers all odour."

Many will gratefully acknowledge, that he who taught them how to exercise their senses on the universe around them, and how to compare and arrange the information thus obtained with that which had in like manner been collected and arranged by so many great intellects before him, was one of the truest benefactors with whom their journey through life has made them acquainted.

In private life Professor Jameson was as eminent for his virtues, as in public he was for his science. He formed the centre of a wide circle of admirers and friends, one whose breaking up will be felt as a heavy loss in Edinburgh. He never married. He was a member of most of the learned societies in the world, and was laden with scientific honours. When he died, the citizens of Edinburgh testified their respect by a public funeral. The *Senatus Academicus* in

their robes, the Magistrates, and members of the public institutions of the city, followed his remains to their resting-place in the Warriston Cemetery.

VARIETIES.

ANECDOTE OF PORTAL.—While he was yet young he made up for himself the air and the aspect of an old man, with periwig, gold-headed cane, coat with great skirts, and in winter a wadded wrapper. These he wore before the Revolution in '89; during the Directory; during the Consulate; during the Empire; during the Restoration. Louis XVIII. and the emigrants found him just the same as they had left him. He had only a thread of a voice, and this became totally lost when any one plied him with embarrassing questions. From the day when Portal set out for Paris in company with Treillard and the Abbé Maury, his ambition had been to be physician to king or emperor, or whoever might sit on the throne of France. The Restoration realised his dream. He was physician to King Louis XVIII. and to Charles X. After the Revolution of 1830 Portal resolutely demanded an audience of King Louis Philippe, "Sire," said he, "I am here to take your commands for the composition of your medical household." "I have my physician," was the king's answer: "Dr Marc has had my confidence for many years past." "Dr Marc is a very skilful man," replied Portal; "we will include him in your medical household." "But," said the king, "I will have no medical household—I will have no other physician than Dr Marc." "Well then, sire, retain Dr Marc; I apply to be his successor." Portal was then eighty-eight years old.—*Véron's Mémoires d'un Bourgeois de Paris.*

THE MELBOURNE UNIVERSITY.—In our last January Number, we directed attention to the remarkable contrast exhibited in the manner of founding universities abroad and at home, and announced the fact that the Legislature of Australia had voted for its university an annual income of L.5000, and added a gift of land of the value of L.150,000. It seems that at present a permanent endowment of L.9000 a year has been granted by the Legislature; a sum of L.20,000 has been voted for the university buildings, and an additional sum of L.10,000 for the year 1854. Four professors have been invited from the mother country, to fill the chairs—1st, of Greek and Latin classes; 2d, Mathematics; 3d, Natural Science; and, 4th, Modern History and Literature. Their salaries are to consist of L.1000 per annum, suitable house accommodation, and an allowance of L.300 for outfit and expenses. We sincerely trust that such extraordinary liberality may secure to this flourishing and wealthy colony distinguished men, who will alike do honour to the parent country, as well as to the new land in which they are to open up the fountains of literature and science.

INDUSTRIAL MUSEUM FOR SCOTLAND.—Government has granted the sum of L.7000 for the purchase of the Trades Maiden Hospital (L.5000), and Dr Alexander's Church (L.2000), both immediately behind the University. It has also granted L.1500 for the purchase and arrangement of specimens. This is the first step in the establishment of a National Museum of Natural History and Economic Geology for Scotland, which is to be connected with the University, and made available for the teaching of its professors.

THE UNIVERSITY OF LONDON.—The medical graduates of the University of London have brought a bill into Parliament, which, if carried, will extend to them all the privileges which are enjoyed by the Universities of Oxford and Cambridge. We do not object to this proceeding as far as the University of London is concerned, but are at a loss to imagine on what principle the extension of privilege is to be limited to them only, and especially why the University of Edinburgh should not be included. The promoters of this proceeding may rest assured that the growlings of the Scotch Lion will still be loud enough to drown their pleadings in its favour, notwithstanding they have succeeded in getting it read twice in the House of Commons within the short period of fifteen days.

MONSIEUR PIORRY, A POET.—The distinguished author of mediate percussion, and of a new medical nomenclature, in nine volumes 8vo, has just published a poem, on God, the Soul, and Nature, in six cantos. The apparition of such a poem, by a clinical professor of the faculty, on the same day that he read a memoir to the Academy on the cure of phthisis, by inhaling the vapour of iodine, has produced no small astonishment in the medical circles of Paris.

NEW VARIETY OF MONOMANIA.—The great alterations now proceeding in the streets and buildings of Paris, appear to have created a new kind of monomania. The case of a merchant was lately brought before one of the law courts, who had been seized with the desire of becoming an occupant of all the houses in Paris. He passed his days in running about the streets, and taking every building that was to be let. The confusion which arose on the approach of the term day, may easily be imagined.—*Journ. de Medecine.*

ANECDOTE OF GRAVES.—"He was with me, going round the hospital," says Dr Stokes, "when we entered the convalescent ward. He began to expatiate on the healthy appearance of some who had recovered from severe typhus. 'This is all the effect of our good feeding,' he exclaimed; 'and lest, when I am gone, you may be at a loss for an epitaph for me, let me give you one in three words—HE FED FEVERS.'"—*Medical Times and Gazette.*

PRESIDENT OF THE ROYAL SOCIETY.—We understand that Lord Wrottesley, the eminent astronomer, is likely to be elected President of the Royal Society, in the place of the Earl of Hope, who retires.

PUBLICATIONS RECEIVED.

- Lectures on Histology, delivered at the Royal College of Surgeons of England, in the Session 1851-2. By John Quekett, Professor of Histology in the Royal College of Surgeons, etc. Vol. II. Structure of the Skeleton of Plants and Invertebrate Animals. Sixty-four woodcuts. London. 1854. Pp. 413.
- The Microscope and its Application to Clinical Medicine. By Lionel Beale, M.B., Lond., Professor of Physiology and Morbid Anatomy in King's College, London. 1854. London. Small 8vo. Pp. 303.
- Manual of Diseases of the Skin: from the French of Cazeare, with notes and additions. By Thomas H. Burgess, M.D., lately Physician to the Blenheim Dispensary. Second edition, considerably enlarged and improved. 1854. London. 12mo. Pp. 432.
- The Census of Ireland for the Year 1851. Part III. Report on the Status of Disease. 1854. Dublin. Foolscap fol. Pp. 150.
- A Treatise on the Cure of Stammering; with Memoirs of the late Thomas Hunt. By James Hunt, M.R.C.L. 1854. London. 8vo. Pp. 80.
- Epilepsy, and other Affections of the Nervous System, which are marked by Tremor, Convulsion, or Spasm; their Pathology and Treatment. By Charles Bland Radcliffe, M.D., etc. 1854. London. Pp. 144.
- Papers relating to the University of Melbourne, Victoria. 1854. London. 8vo. Pp. 22.
- Expériences sur les Injections de Perchlorure de fer dans les Artères. Par M. Goubaux et M. Giraldes. (Reprint.)
- Fortieth Annual Report of the Directors of the Glasgow Royal Asylum for Lunatics. Glasgow. 1854. 8vo. Pp. 58.
- The Improvements in Modern Surgery: An Oration. By Henry Smith, F.R.C.S., Esq., etc. 1854. London. 8vo. Pp. 24.
- Thoughts on Uremia. By Gunning S. Bedford, A.M., M.D. (Reprint.)
- On the Displacements of the Uterus. By T. Matthews Duncan, A.M., M.D., Lecturer on Midwifery, etc. 1854. Edinburgh. 8vo. Pp. 43.
- On the Subject of Priority in the Medication of the Larynx and Trachea. By Horace Green, M.D. 1854. New York. 8vo. Pp. 17.
- Observations on the Climate of the North Island of New Zealand. By A. S. Thomson, M.D., Surgeon 58th Regiment. 1853. Auckland. Foolscap fol. Pp. 5.
- Clinical Lectures on Paralysis, Disease of the Brain, and other Affections of the Nervous System. By Robert Bentley Todd, M.D., F.R.S., Physician to King's College Hospital. 1854. London. 12mo. Pp. 462.

Drs Lindsay and George Paterson's papers will be inserted in our next No.

On going to press, we received through our Bookseller, the 3d No., 6th vol. of Virchow's Archiv., published as usual without date, and containing a note addressed to us, dated January 6, 1854. We find that to some extent we have anticipated a reply in our present No., and we shall expose the other misrepresentations contained in this new note in our next.

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